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THE
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*A WEEKLY RECORD OF CLINICAL MEDICINE AND
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IN TWO VOLUMES ANNUALLY.

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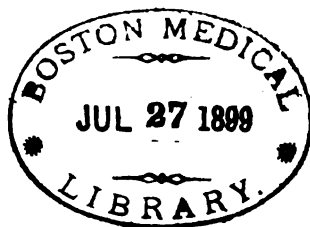
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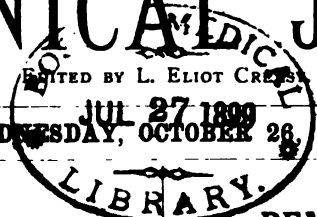
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REMARKS UPON DIET.

BY

W. S. CHURCH, M.D., F.R.C.P.,

St. Bartholomew's Hospital, October 14th, 1898.

GENTLEMEN,—I intend to-day to make some very general, I am afraid rather discursive remarks upon diet. It is a subject of the very greatest importance to you in your professional education, but at the same time it is one that is very apt to be overlooked. Students are in the habit of seeing in the hospitals the physician or surgeon in charge of cases write down one of the hospital diets, with or without, as the case may be, certain extras, such as stimulants, essences, eggs, and so forth, and they do not pay very great attention to the subject. Moreover, in medical text-books and treatises on general medicine, the subject of diet and regimen is not now treated in the elaborate, and I might say pedantic and prolix way in which you find it frequently treated in the writings of the older physicians; and therefore it happens not infrequently when men go out into practice, and are thrown upon their own resources, that they feel themselves somewhat abroad as to the directions that they should give as to diet and regimen of their patients. Of course the subject is far too large a one for me to go over any but a very small portion of it in a single clinical lecture and therefore I am afraid that my remarks, as I have said already, will be rather general and discursive.

All of you know that perhaps the oldest writer upon dietetics with whom we are acquainted was Hippocrates, who wrote a complete book on the diet and regimen of acute disease. That book was thought so highly of by the great physician who flourished several hundred years later, Galen, that he wrote a book which was a commentary upon Hippocrates' book. Another great physician of classical times, Aretæus, also wrote on the therapeutics of acute disease. I am not going to

advise you to read these, because, considering the amount of modern knowledge which you have to get up for your examination, it is perfectly impossible for you to do so, but at the same time if in any leisure minutes you can look at them you will find a good deal that is curious and interesting, and not a little which is very instructive in those old treatises. The subject of diet, or perhaps one might put the two subjects of diet and regimen together, have been favourite subjects for medical authors to write upon ever since the times of the revival of learning, and I should not be doing any good, neither do I wish to trouble you by mentioning a tithe of the authors who have written on the subjects. I will, however, draw your attention to two works, for very different reasons. One you probably all know by name, and that is 'De Conservandâ bonâ Valetudine,' the maxims of the school of Salernum, which contains a great deal of interesting and somewhat amusing information; and the other is a book which was well known at the beginning of last century, on 'Diet,' by Dr. Arbuthnot, and I mention the latter because it might interest you to know that it was from this book that Dr. Johnson took, to a very great extent, his explanations of the medical terms which appear in his dictionary. If any of you ever have the curiosity to look out any words in Johnson's Dictionary, you will constantly find that in connection with words which were used in medicine there are passages from Arbuthnot in illustration of their meanings. I mention those two authors as ones with which you ought to be acquainted, in name at least. But when I say two authors, that is improper, because of course the maxims 'De Conservandâ bonâ Valetudine' were not the work of any single person, but a collection of wise saws put together by various teachers and philosophers of the School of Salernum, that being, as you know, the oldest Medical University with which we are acquainted in what one might call the Middle Ages, founded by Robert Guiscard, at Salernum, in Italy.

My object to-day is not to draw your attention to the various musty tomes which I have mentioned, but rather to urge you to use your own observation, and form your own judgment when here, early in your careers, as to the way in which you can best diet your patients. And I must say here in passing, that it is astonishing what an

amount of information you can get if you simply keep your ears and eyes open in general society, and listen when people are talking about eating and drinking. I do not mean to say that what you learn there will be of very great use to you as far as the directions you can give are concerned, but they will be of very great use in picking up information as to the whims and fancies of the general public and society with regard to articles of food and drink, and you will get from that source information which you do not receive within these walls, nor find written in any books. In this way you will learn many things which perhaps it is improper to say are "wrinkles," meaning by wrinkles anything which is valuable; still you will learn a great deal about the whims and fancies of general society. You must remember, also, that when you are in practice the man who can order palatable food is preferred very much to the doctor who relies wholly upon his pills and potions and one or two standard dishes.

The increase of our knowledge, especially of physiology and physiological chemistry, of late years has placed us on surer grounds with regard to food-stuffs. We are in a position which our forefathers were not in; and although we are still very ignorant of the whole process of digestion and assimilation, we have a considerable measure of knowledge on the subject. And I would remind you here that food-stuffs are divided into three great classes—proteids, fats, and carbohydrates. I shall have to allude to these again. Those three great divisions of food-stuffs vary not only in their chemical constituents, especially the proteid or nitrogenous bodies, but they also vary in the ease with which they can be digested and assimilated into our system. And it is necessary for you to have that clearly before your minds when dealing with both health and disease. Almost all writers from Hippocrates downwards, in treating of diet and regimen, have been in the habit of dividing the subject into two divisions, into which it very naturally falls, namely, the diet and regimen for those in health for the purpose of preserving health, and that required for the infirm. Now the marvellous adaptability of man to his surrounding circumstances is so great that, as you all know, we can keep our health under most diverse circumstances and in most dissimilar climates; and,

moreover, we can keep healthy when making use of most dissimilar food,—such, for example, as that of the Esquimaux who inhabit the Arctic zones, and that of the Hindoos in torrid regions. The Esquimaux never or but rarely taste any vegetable food; their food consists almost solely of animal food at all times of the year. Some of the inhabitants of cold climates, such as the Lapps, get milk, but the Esquimaux scarcely ever get either vegetable food or milk; and the same is true of some natives of South America, the Guachos, who live almost entirely upon animal food. On the other hand, the inhabitants of other parts of the world, especially large portions of the population of India, hardly ever taste flesh from one year's end to the other, and yet they are able to live in perfectly good health upon such diet as they have. So that, regarding the diet of the healthy, I might refer you to the very remarkable words with which Celsus commences his book on medicine, in which he says, "*Sanus homo qui et bene valet, et suæ spontis est, nullis obligare se legibus debet; ac neque medico, neque iatro-alipta egere;*" which may be very briefly paraphrased by "a healthy man need tie himself by no laws, but may live on what he meets with, and that he ought to have no need for the physician." But then in his second chapter he goes on to say that that statement of his in the first chapter does not apply to the infirm, and he proceeds with this very remarkable passage: "The infirm embrace nearly all the dwellers in big cities, and almost all others who are given to literary studies." Now that is a very remarkable and interesting passage, because it shows that in the time of Celsus, that is in the Empire, the inhabitants of Rome suffered from improper food and want of air and sunlight, and were infirm, and differed from the robust rustics, just in the same way as the inhabitants of this overgrown metropolis do; but I hope that what he says in the latter part is not true now, that all literary students also suffer in their health. But it is extremely interesting to find that even in that day the inhabitants of cities were, I suppose, notably more pallid and less robust than their brethren in the country. And so it undoubtedly is now.

When we are dealing with the diet of healthy people it always seems to me that the main point to consider is the quantity rather than the quality. The healthy man can digest and assimilate almost

anything in reason; it is the quantity which is generally in error. And I am reminded of the remark which was made by Sir Charles Scarborough, who was one of the leading physicians at the end of the seventeenth century and the beginning of the eighteenth century, to one of Charles II's favourites, the Duchess of Portsmouth, who consulted him upon her health; he said, "Madam, you must either eat less, make use of more exercises, or take physic, or be sick." And that is true.

I do not intend to say anything more about the diet or the regimen of the healthy to-day, although it would be an interesting subject; but I will remind you of the maxim of the school of Salernum, which says, speaking of supper, which in those days answered to our dinner, "*Ex magnâ cœnâ, stomacho fit maxima pœna. Ut sis nocte levis, sit tibi cœna brevis*"—which means "you must dine lightly if you wish to have a good night."

Now I turn to disease, and I might here say in the short time that remains that the most interesting part of dietetics is in connection with the dietetics required for chronic disease. I am afraid I shall hardly have time to enter at all upon that subject.

In any acute disease—and I mean by acute disease those cases of disease in which the temperature is raised, whether it be raised as a result of a specific poison or what we still speak of as inflammatory action—one of the first and leading symptoms is anorexia, or loss of appetite; and that loss of appetite is certainly accompanied by, if not consequent upon, a disordered condition of the stomach and the whole of the digestive and assimilative organs; they sympathise with the general condition, and they exhibit the fact that they are out of order by the anorexia which is an almost ever-present symptom during the time that there is continuous elevation of temperature. Now I have already said that Hippocrates wrote a book upon the regimen and diet of those suffering from acute disease, and another book was written by Aretæus on the therapeutics of acute disease, which also goes at considerable length into the regimen. I will not ask you to read those books, but they are very valuable and interesting. We have to thank Sydenham for the return in modern days to a rational system of regimen and diet in acute disease. Sydenham, with his great powers of

observation, and the use he made of them at the bedside, together with the independence of his mind, was able to throw off all the mists and fogs which had gradually grown up around mediæval medicine, and he reverted to a very great extent to the principles which guided Hippocrates and his school. We have to thank him for what I may call the rational treatment of acute disease, so far as diet is concerned. If you turn to Sydenham—and he is not so musty that I do not think you might not some of you turn to him with great advantage—you will find that his treatment of fevers, with the exception for his partiality for bleeding, was very nearly the same as that which we now adopt. He agrees with Aretæus, who laid down the law that “liquid food is proper in all febrile diseases, and that abstinence from food should not be prolonged. Food should be liquid, scanty, and frequently administered; for food soothes the soul, and the proper time for giving it is during remissions, both of the fever and of the delirium.” You could not have better directions than those.

And now I must just revert for a moment to the difference there is in the various classes of foods. The proteids, fats, and carbohydrates differ in their ease of digestion and assimilation. As you all know, the easiest to be broken up and get into an assimilable form are the carbohydrates. Fats, if tolerably easily digested, do not appear to be assimilated well in states of pyrexia; and proteids, if administered in the ordinary solid form, are hardly digested at all. I wish you to bear that in mind. Sydenham reverted to the doctrines of the ancients, and showed how necessary it was that liquid food only should be given in fevers. Of all liquid foods, Hippocrates lays very great stress upon a decoction of barley—in fact, our barley water,—and on the whey of milk. These, he says, should be given as ordinary drinks. He also gives directions with regard to other starchy foods which may be given, but these are out of date. At the present time our great sheet-anchor in all acute diseases is milk, which is the only liquid food in which all the aliments that we require for the due nourishment of the body are contained. In milk you have as nearly as possible a perfect food, containing all that is necessary for supporting life. Occasionally you meet with instances in which milk is either not borne or the patient thinks he cannot take it. Such cases many of you must

have seen, and in the common practice of this hospital you have noticed how useful it is only to give the whey of the milk, which is obtained by first coagulating the milk and pressing out the whey from the curd. This whey may be mixed with other substances, such as cream, the yolk of eggs, &c., to make it more nutritious if you think that during the fever milk is not well borne. It is very curious to remember that among what one may call a certain school of physicians there was a prejudice against the use of milk in acute disease, especially fever. Heberden says, “Milk and eggs, I know not from what cause, are by some forbidden in all fevers, for it seems to me they can be safely taken in the very worst, provided they are agreeable to the sick.” That is, of course, what we act upon now. It is only in those cases to which I have alluded, in which milk is not tolerated, that you have to depend on use of other food. Animal food, broths, beef-tea, essences of beef, and the like, are of course required, and they require to be well made. Nothing is more distressing to the patient than ill-made animal extracts or broth. It seems to me that it is almost immaterial of what meat they are made so long as it is good—beef, mutton, chicken, veal, rabbit. Remember that a change is always very grateful to your patient. I much prefer myself to have these fresh made, to using any of the various patented beef-teas and animal broths which are upon the market. I do not mean to say that some of them are not extremely good, still I think one is much safer in using extracts thoroughly well made at home. Certain of the patented extracts are undoubtedly valuable, such as Liebig’s and Valentine’s meat juice, but they are of use more as stimulants than food.

Let me now say just a few words about peptonising food. Our modern knowledge has led us to be able to partly digest outside the body the food-stuffs which we wish to introduce into it, and in that way, when the digestive and assimilative powers are weakened, much good may be done. But I think we are rather in the habit of overdoing the administration of these peptonised foods now, for I am sure it is much more grateful to most patients not to have peptonised food if their stomachs can digest sufficient to carry them on. Remember that peptonising food, unless it is properly done, renders it very distasteful and bitter.

It is undoubtedly valuable sometimes, especially in the case of children and in long-continued cases of fever,—that is to say, in cases which run on for many days and weeks, such as we often see in typhoid fever.

I would wish to make another suggestion with regard to animal broths. It is very desirable that you should mix with them vegetable juice, and this can easily be done if, in the preparation of them, you put chopped-up vegetables—carrots, turnips, potatoes, spinach, or any other forms of green into a muslin bag. Let that stew with the beef-tea or with the broth, and before administering it squeeze the bag. In that way you get the vegetable juices without getting the irritating vegetable fibre.

In the few remarks I have made I have not spoken of the use of alcohol in acute disease. Well, our time is almost at an end, so I will say nothing more about the use of alcohol than that, strong as I am as an advocate of its use in disease, I believe that as an habitual article of diet it is quite unnecessary for the healthy man; a perfectly sound and healthy man is better without the habitual use of any form of alcoholic drink. In sickness I would only say that alcohol should be used exactly like any other medicinal drug, and should never be administered except in measured quantities and at stated intervals.

Neither have I said anything about the quantity of fluid food which you have to administer during acute disease, as in fevers, typhoid especially.

Speaking roughly, one may say that in cases of acute disease, as Hippocrates said, and as Aretæus said, the food should be scanty, frequently administered, and the times for administration should be when the fever is lowest. As a matter of fact one finds that food should be administered every two or three hours during the twenty-four, and that two to three pints of fluid food, according as to whether you administer it every two or every three hours, is about the quantity which the system can generally, during those conditions, make use of. I cannot help feeling that our forefathers were in the habit of not allowing sufficient food during fevers, whereas now, if anything, we are inclined to over-feed rather than under-feed our patients.

CEREBELLAR LOCALISATION.

A Lecture delivered at University College Hospital by

J. S. RISIEN RUSSELL, M.D., F.R.C.P.,

Assistant Physician to University College Hospital, and
Pathologist to the National Hospital for the Paralysed
and Epileptic, Queen Square, London.

GENTLEMEN,—In this lecture I propose to call your attention to some of the main facts that have been the outcome of modern researches, conducted with a view to determine the functions of the cerebellum. In so doing I shall deal chiefly with those phenomena which serve as our best guides when we attempt to elucidate one of the most difficult problems in clinical medicine, viz. the diagnosis of the probable seat of a lesion in the cerebellum. Moreover, I hope to guard you against certain fallacies which are apt to arise when the clinical problem is under discussion; fallacies due to the fact that in cerebellar lesions in man we have, as a rule, to deal not only with defects consequent on destruction of cerebellar tissue, and which are strictly comparable to the effects which follow ablation of parts of the organ experimentally, but also with others the result of increase of intra-cranial pressure. The effect of increase of intra-cranial pressure is to distort the clinical picture by adding to it extraneous phenomena in no way dependent on the destruction of cerebellar tissue. A study of our subject from the clinical standpoint, however, affords us evidence that this increase of pressure, oftentimes responsible for so much confusion, is not entirely an unmixed evil, but that under certain circumstances some of its effects are important aids to diagnosis.

While there are certain phenomena common to cerebellar lesions in general, we are able to distinguish a group of signs which indicate that the lesion is in one or other lateral lobe, as opposed to its being in the middle lobe of the organ. All that I propose to do on the present occasion is to deal with the signs which indicate the existence of a lesion of one lateral lobe of the cerebellum, and to reserve for consideration at some future time those which point more especially to an affection

of the middle lobe. The point which is of most practical importance for us to be able to decide is which side of the cerebellum is affected in any case of disease of this organ.

Phenomena Characteristic of a Unilateral Lesion of the Cerebellum.

Inco-ordination is the most striking feature noted after experimental ablation of one half of the organ, and manifests itself in various ways.

Rotation.—At first all other modes of its manifestation may be subservient to rotation, the direction of which is best described in terms relating to a screw. In a right-sided lesion the rotation of the subject is in the direction of a right-handed male screw entering an object, while in a left-sided lesion the direction of rotation corresponds to that of a similar screw coming out of an object. This phenomenon does not always occur after experimental lesions, and is only exceptionally met with in man, a fact that is not surprising in that it is much less pronounced in the monkey than in the dog.

Titubation is always obvious when the tendency to rotation is sufficiently in abeyance to make sitting up or standing possible. The unsteadiness of the head on the trunk resembles closely that seen in disseminated sclerosis, and is increased by any attempt at voluntary movement, *e. g.* feeding. This phenomenon may be well marked in clinical cases, but not so much so as inco-ordination or ataxy of the limbs, best seen in the disturbance of the gait in man. The subject walks with the lower extremities far apart, on a wide base, moving the limbs forward in an unsteady manner, it may be tending to stamp the heels unduly, and evincing more inco-ordination of the limb on the side of the cerebellar lesion than of that on the opposite side. Oftentimes this difference is only very slight in degree, and will only be appreciated on most careful observation. Similarly, the inco-ordination may sometimes be demonstrated as the patient lies in bed, by asking him, with his eyes closed, to attempt to touch the knee of one leg with the heel of the other, or if with his eyes open he be asked to touch with his toes some object held above his bed. In this way not only may inco-ordination of the lower extremities be detected, but it may be obvious that the unsteadiness is more pronounced on the side of the

lesion than on the opposite side. Inco-ordination is also present in the upper extremities, and is most marked in the limb on the side of the lesion; both the unsteadiness which you have probably seen most typically in disseminated sclerosis, and the ataxy with loss of sense of position which you may have seen well marked in locomotor ataxy may be evident, if the patient be asked to touch the point of the nose with the forefinger of either hand, his eyes being closed during the attempt; or it may be that such defects of co-ordination are only detected in the limb on the side of the cerebellar lesion.

The defect may be only very slight, while at other times the most pronounced inco-ordination of the superior extremity may be met with. In testing the power of co-ordination of movement of the upper extremity the patient should be made to support the limb, from the shoulder, at a right angle to the body, and to slowly bring the forefinger forward so as to touch the nose. I have seen this sign missed in consequence of the patient being allowed to rest the upper arm on the bed, while the forearm and hand were alone employed in executing the movement.

Reeling is another phenomenon which is much more constant as a result of experimental lesions than as one of the effects of cerebellar disease in man, and, moreover, while its direction indicates with great certainty the side of the experimental lesion, few of the signs on which we rely for the localisation of the seat of disease clinically are less reliable than is this one. Experimentally the subject reels so as to fall away from the side of the lesion, but clinically we sometimes find the patient reeling to the side of the lesion, while in other instances he reels to the opposite side, as in the case of the experimental lesions. This difference, no doubt, depends in some way on the increased intra-cranial pressure present in addition to the destructive lesion of the cerebellum in most of the cases that we meet with clinically. But even in the experimental cases it requires some knowledge of the conditions to be able to distinguish a fall to the side opposite to the lesion as a result of reeling in this direction, as opposed to a fall to the same side as the lesion, which sometimes occurs, and which is due to giving way of the limbs on the side of the lesion in consequence of, as we shall subsequently see, a certain degree of loss of power in

these limbs, but no doubt also in part due to the inco-ordination which they exhibit.

When attempting to localise a cerebellar lesion clinically I do not allow myself to be influenced by the side to which the patient lurches or tends to fall, unless this sign is in accord with the other phenomena present on which a diagnosis is being based. So much do I mistrust this sign that there are few others on which one would not rely with greater confidence than on it. In short, where other signs present point to one side of the cerebellum being affected, while the direction of the reeling indicates the other, I prefer to disregard the latter sign, and to be guided by the indications which the others afford.

The attitude is very characteristic. The trunk is curved with the concavity to the side of the cerebellar lesion, and the side of the face is approximated to the shoulder on the same side; but in addition to this, in animals, there is a curious rotation of the neck, so that the side of the face corresponding to the cerebellar lesion is turned upwards, and at the same time the chin is directed to the affected side. This attitude has been present in several cases that I have had opportunities of seeing, including a case of mine operated on by Mr. Godlee last week. There has been this difference, however, in the attitude in the cases in which one has seen this sign in man, as compared to what one has found in animals,—the chin appears to be directed to the opposite side instead of to the side of the lesion. Otherwise the attitude is the same as after experimental lesions; there is the same lateral inclination of the head to the side of the lesion, so that the side of the face is approximated to the shoulder, and there is the same lateral curvature of the spinal column with the concavity of the curve to the side of the lesion.

Rigidity due to spasm of the muscles of the limbs on the side of the lesion, and to some extent of those of the opposite posterior extremity, is a constant feature after experimental ablation of one half of the cerebellum. That the back muscles share in the spasm is evident, in that any attempt to straighten the spinal column passively meets with distinct resistance; and, moreover, as soon as the extending force is taken off, the trunk once more assumes the attitude already described, in which the spinal column is curved laterally,

with the concavity of the curve to the side of the lesion.

I do not remember ever having seen this muscular rigidity in an uncomplicated cerebellar lesion in man. Dr. Hughlings Jackson has, however, recorded instances in which rigidity of the trunk and limbs occurred in association with tumours of the middle lobe of the cerebellum; but it has been contended that the rigidity in Dr. Jackson's cases could be explained by pressure of the tumours on the pons rather than by supposing it to be due to any direct effect consequent on the lesion of the cerebellum—an argument that it is not easy to controvert.

The influence which the cerebellum exerts on the muscles in regard to tonus is altogether a very complicated one, for Mr. Victor Horsley and Dr. Max Lowenthal* have shown that when both cerebral hemispheres are removed, and as a result active extensor tonus of the limbs obtained, faradic excitation of the upper surface of the cerebellum at the junction of the vermis and lateral lobe caused immediate relaxation of the tonus, which, however, became re-established as soon as the current was shut off. These observers further showed that in the upper extremity the active relaxation which occurred in the triceps was accompanied by a powerful contraction of the biceps, that one limb could be influenced by excitation of either side of the cerebellum, but that the effect is very much greater from excitation of the same side. In five of their experiments they found that excitation of one side of the cerebellum resulted in relaxation of the biceps of the opposite limb. When no "acerebral" tonus had been established they found that excitation of the cerebellar cortex caused tonic contraction of either the triceps or biceps, or of both together. Synchronous excitation of the cerebellar area for the fore-limb and of the cerebellar focus resulted in an addition to the tonus elicited previously from the cortex cerebri.

Convulsions.—Closely related to the question of muscular rigidity in connection with cerebellar lesions is a form of general convulsion that has been described in man, and which has also been observed to affect the muscles of the limbs on the side of the cerebellar disease alone. These con-

* 'Proc. Roy. Soc.,' 1897.

vulsive attacks, as a rule, resemble tetanus seizures rather than convulsions, the result of discharges having their origin in the cerebral cortex.

I have not myself made any observations with regard to excitation of the cerebellum in animals; but the results obtained by Dr. Ferrier* are in accordance with the condition of things that has been occasionally met with in man, for this observer found that movements of a sudden and spasmodic character occurred in the limbs on the same side as the hemisphere of the cerebellum excited.

An objection has been raised, which appears to me to be a fair one, against the muscular rigidity and convulsions that have been met with clinically in cases of tumours of the cerebellum being regarded as of cerebellar origin, and it has been contended, with much reason, that these phenomena may result from pressure on the pons. When rigidity or convulsions exist on both sides of the body, it must be admitted that no satisfactory argument can be adduced against the view that they are thus secondarily caused by pressure on the pons if the situation of the tumour in the cerebellum is such as to allow of its exerting pressure of the kind. The same, or even more may be said when rigidity or convulsions involve the opposite side of the body in a unilateral lesion of the cerebellum; for in such cases there can be little doubt that these symptoms are consequent on pressure on, or infiltration of the side of the pons corresponding to the side of the cerebellar lesion. But it is otherwise when, with a lesion limited to one side of the cerebellum, we meet with rigidity or convulsions involving the muscles of the same side of the body as that on which the cerebellar defect exists. I am convinced that in such cases these phenomena are direct effects of the cerebellar lesion, and that they are not indirectly induced by pressure on or infiltration of adjacent structures, for such secondary damage could only result in the symptoms being general, or if unilateral, then limited to the opposite side of the body, unless we assume that a rare anatomical defect is present in the shape of non-decussation of the pyramidal tracts at the caudal end of the medulla oblongata. With the pyramids decussating normally at the caudal end of the medulla any

pressure or infiltration of one half of the pons which gives rise to any motor phenomena in connection with the limbs of one side of the body must of necessity affect those on the side opposite to the pontine lesion.

The tendon-jerks furnish us with one of the most striking differences of what obtains in man as opposed to the experimental results in animals. In the latter the tendon-jerks are increased; and while this is true of both knee-jerks, that on the side of the lesion is the more exaggerated. Though a similar state of things is met with in man in some instances, in others there may be no difference on the two sides, while in others both knee-jerks are abolished.

What happens after experimental lesions is this: while the animal is still under the influence of the anæsthetic the knee-jerk on the side of the ablation of the cerebellum is greatly exaggerated, while the knee-jerk on the opposite side is distinctly diminished. A curious alteration in the condition of the two knee-jerks may, however, be observed if the anæsthetic agent (ether) be pushed to a stage of narcosis which closely precedes that in which they are abolished by ether, for the knee-jerk on the side of the lesion, which was formerly increased, now becomes diminished; while that on the opposite side, which was formerly diminished, becomes exaggerated.

Immediately after the operation of removal of one lateral half of the cerebellum the knee-jerk on the side of the lesion is much increased, while that on the opposite side is greatly diminished, if not abolished. By the following day the diminished knee-jerk of the opposite side to the lesion becomes exaggerated, and by the second day it may be impossible to say which of the knee-jerks is the greater. As time goes on, however, the knee-jerks become less and less active, that on the side of the lesion always remaining brisker than normal as long as the subjects were kept under observation (three months), while the knee-jerk on the opposite side usually returned to or about normal, so that at this late stage it is usually possible to determine inequality of the knee-jerks, that on the side of the lesion being increased and greater than that on the opposite side.

That increased activity of the knee-jerks may persist for a long time after a cerebellar lesion is recovered from in man was well illustrated by a

* 'The Functions of the Brain,' 2nd edit., p. 190.

case in which Mr. Dean* successfully evacuated an abscess in the right side of the cerebellum, for both knee-jerks remained exceedingly active two years and a half after the operation; but Mr. Dean was unable to decide which of the two jerks was the more increased.

As has already been said, while exaggeration of the knee-jerks occurs in cerebellar disease in man, it is as common to find them diminished or abolished. This diminution or abolition of the knee-jerks in cases of cerebellar tumour in man must in some way be associated with the increase of intra-cranial pressure present in these cases. This is suggested by the following two considerations:—In the first place, in experimental lesions in animals where pressure is out of the question, apart from the temporary depression of the knee-jerk on the side opposite to a unilateral lesion of the cerebellum, the rule is, as we have seen, that the knee-jerks are increased. Secondly, tumours of the cerebral hemispheres attended with great increase of intra-cranial pressure, especially if the pressure be rapidly increased, may show great diminution or absence of knee-jerks, just as in these cerebellar cases that we are now considering. Moreover, where diminution or abolition of the knee-jerks is met with, variations in their exact state of activity from time to time not uncommonly occur.

Clinical evidence does, however, in some cases undoubtedly support the experimental findings. In such patients, while both knee-jerks may be increased, that on the side of the cerebellar lesion is the more active, so that in any case of cerebellar disease in man where we meet with increased activity of the knee-jerks, and the one more so than the other, provided there be no reason to suspect secondary pressure on, or infiltration of one side of the pons, we regard the greater knee-jerk as evidence of a lesion situated on that side of the cerebellum.

Motor Paresis.—I have next to speak of a sign which is constantly met with in slight degree experimentally, but which is only to be detected exceptionally in man, viz. loss of motor power. It assumes the form of a hemiplegia in which the limbs on the side of the lesion are affected and the face escapes; or perhaps it would be more correct

to speak of it as a triplegia, in that experimentally a slight degree of paresis is also present in the posterior limb of the opposite side. Apart from the fact that this motor paresis is only exceptionally met with in man, while it is a constant phenomenon after experimental lesions, another notable difference exists between the quadruped and man, in that it appears to be the posterior extremity on the side of the lesion that suffers most in the former, while it is the superior extremity on the side of the lesion in which the paresis has been most evident in cases of the kind which have come under my observation in man.

I cannot leave the subject of loss of motor power without referring to the back muscles. It is exceedingly difficult to determine whether there be any loss of power in these muscles or not in the quadruped. The attitude which I have described to you as being so typical of a unilateral lesion of the cerebellum, and which has been noted both in man and in the lower animals, is characterised, as we have seen, so far as the trunk is concerned, by a lateral curvature of the spinal column in which the concavity of the curve is directed to the side of the cerebellar lesion. Now it has been assumed that this attitude is the result of weakness of the back muscles on one side. It may be so at least in part, but of this we have no very satisfactory evidence. Dr. Hughlings Jackson has long contended that paresis of the erector spinæ results in lesions of the middle lobe of the cerebellum, an opinion which was also held by Niemeyer. I have had the honour of publishing a case in conjunction with Dr. Jackson* in which we determined such weakness of the back muscles during the patient's life, and in which we found a cystic condition of the cerebellum post mortem. The greater part of the middle lobe of the organ had been destroyed, in addition to some of the contiguous parts of each lateral lobe. I have, however, never been able to determine with certainty any weakness of the erector spinæ on one side in a unilateral lesion of the cerebellum by means of any of the known tests on which we rely for the detection of any such defect, nor am I aware that any one else has proved the existence of unilateral weakness of the back muscles under such circumstances by these means. It is, of

* 'Lancet,' July 30th, 1892.

* 'Brit. Med. Journ.,' Feb. 24th, 1894.

course, not easy to judge of the condition of the trunk muscles in most animals, the monkey alone affording us an opportunity of studying the effects of ablation of the cerebellum on the state of the back muscles; but even in this class of animal it is by no means easy to come to any definite conclusion on the subject. A monkey deprived of the whole of its cerebellum is certainly unable to sit up, a state of things that may depend on weakness of the back muscles; but we cannot, of course, satisfactorily exclude the possibility that the animal refrains from so doing in consequence of a subjective sense of giddiness which may be present.

Whether the attitude be due to weakness of the back muscles on one side or not, this much is certain in the case of animals, viz. that spasm of the back muscles on the side of the cerebellar lesion is at any rate in part responsible for the lateral curvature of the spine which is present. The proof of this lies in the fact that if an animal presenting this phenomenon in any degree has its spinal column passively straightened, resistance to the attempt to straighten the spine can be distinctly felt, and, moreover, directly the extending force is taken off the spine again assumes the abnormal position of curvature, with the concavity of the curve directed to the side of the cerebellar lesion; and this, be it remembered, in a quadruped which has not the same need to use the back muscles to keep the trunk straight in the way that man has to use them to maintain the erect posture. The existence of this spasm does not, of course, exclude the possibility of weakness of the muscles so affected, so that all the experimental results prove is that, at any rate in the lower animals, spasm is an important factor in bringing about the characteristic attitude of the trunk.

The explanation of the way in which the motor paresis is produced by a cerebellar lesion is not quite clear. It is tempting to assume that it is the result of some direct cerebellar influence removed from the muscles of the limbs on the same side of the body, an assumption which seems warranted when we remember that the face is not included in the hemiparesis, for the escape of the face under such circumstances is only what would be expected. On considering whether any path exists by which motor impulses may reach the muscles from the cerebellum

through the spinal centres, we find that such a path undoubtedly does exist, though there is a difference of opinion among observers as to whether it is a direct one from the cerebellum to the spinal cord—a view held by Marchi, who has been supported by Biedl and more recently by Thomas; or whether, as I have contended, it is an indirect path through Dieter's nucleus—an opinion shared by Drs. Ferrier and Turner, and by Dr. Mott. From the practical standpoint it matters little which of these views is the correct one, for all observers are agreed that an efferent tract in the spinal cord exists at the periphery of the ventrolateral region; and that it probably is concerned with the conduction of efferent impulses from the cerebellum, there seems to me little question. We are all aware that the paths by which afferent impulses reach the cerebellum meet with more than one break in their course from the periphery to that organ; e. g. impulses which travel by way of the direct cerebellar tract meet with a break at the cells of the dorsal root ganglia, and again in the cells of Clarke's column, so that there would be nothing remarkable in efferent impulses from the cerebellum meeting with a break at Dieter's nucleus, as well as in the cells of the ventral horns of the spinal cord before they reach the muscles on which they exert their influence. Division of a dorsal nerve-root on the distal or peripheral side of its ganglion does not result in degeneration of the fibres of the direct cerebellar tract, nor does section of the dorsal root on the proximal side of the ganglion between it and the cells of Clarke's column result in such degeneration; it is only when the cells of the latter column are destroyed, or their axons severed from them, that such a degeneration results, so that there is nothing surprising if an efferent tract from the cerebellum to the muscles exists, and yet does not degenerate after a lesion of that organ itself, but only after destruction of the cells of Dieter's nucleus.

In introducing this part of my subject I told you that some doubt exists as to the way in which this motor paresis is to be explained; this is in part due to the fact that one half of the cerebellum undoubtedly exerts an inhibitory influence over the cells of the opposite cerebral hemisphere. This is evidenced by the fact that after ablation of one half of the cerebellum the excitability of the cortex of the opposite cerebral hemisphere

is increased to electrical stimuli, and by the further fact that general convulsions evoked after such ablation of one half of the cerebellum are of such a character, that those of the muscles of the limbs on the side of the lesion are much in excess of those of the muscles of the opposite limbs; or, in other words, the discharge from the opposite cerebral hemisphere is much in excess of that from the cerebral hemisphere on the side of the cerebellar lesion: for it is scarcely necessary to remind you that the bulk of the fibres of the paths by which motor impulses reach the muscles from the cerebrum (the pyramids) cross at the caudal end of the medulla oblongata, so that impulses originating in the left cerebral hemisphere, for instance, are directed chiefly to the muscles of the right limbs. It is possible, then, that this altered state of excitability of the opposite cerebral cortex may be responsible for the motor defect met with in the limbs on the side of a cerebellar lesion; but it does not seem very likely, in that all the evidence points to excess of action of the cells of the opposite motor cortex, there being no diminution of excitability, such as might be expected to account for a paresis such as that which we are now considering. It thus seems more reasonable to regard the paresis as the direct effect of some cerebellar influence removed from the muscles, rather than to assume that the effect on the muscles, is brought about in an indirect way through the agency of the motor cortex of the opposite cerebral hemisphere.

I do not propose to do more than mention that another explanation that has been advanced to account for the loss of power in the limbs on the same side as a cerebellar lesion in man, is pressure exerted by the tumour on the pyramidal fibres on the same side below the level of the decussation of the pyramids at the caudal end of the medulla oblongata. Personally I do not believe that any mechanism ever exists in these cases by which pressure can be so exerted by a tumour of the cerebellum on the pyramidal fibres on one side below the level of the decussation.

Ocular defects are some of the most constant phenomena which result from ablation of one half of the cerebellum, and squints are among the more important of these defects. It is, however, all-important that you should clearly recognise that the position assumed by the globes while the

animal is still under the influence of the anæsthetic is not that which persists after all the effects of the anæsthetic have passed off, at least in so far as the position of the eye on the same side of the cerebellar lesion is concerned. The position assumed by the eyes while the animal is still unconscious may be what has been described as the "skew deviation;" but it is not constant, and rarely persists for any length of time after consciousness is restored. This "skew deviation" consists in an upward and outward displacement of the eye on the side of the cerebellar lesion, while the opposite eye is displaced downwards and outwards. When the "skew deviation" is absent, and only one eye departs from its normal position, it is always the eye on the side of the cerebellar lesion which is not displaced, while the opposite eye is directed outward, with a varying degree of downward rotation added.

After the effects of the anæsthetic have passed off, what is observed is a displacement outwards and downwards of the globe on the side opposite to the cerebellar lesion, while the globe on the same side as the lesion may show little or no departure from the normal position. According to Luciani* this eye is directed inwards, so that there is a condition of conjugate turning of the eyes away from the lesion; but while I have never failed to detect the outward displacement of the eye on the side opposite to the lesion, I have never been able to satisfy myself that the inward displacement of the eye on the side of the lesion exists. The abnormal position of the globes which has just been described is so rapidly compensated for, that it at first sight seems highly probable that it belongs to the category of irritative phenomena connected with the cerebellar lesion, with regard to which the older physiologists made so much. Defects classed as the result of irritation are supposed to be due to irritation, by the destructive process at the seat of lesion of parts of the cerebellum left intact, instead of being regarded as due to a loss of function consequent on the absence of some influence formerly exerted by the part removed. That ocular displacement is not dependent on irritation, but that it is a true paralytic phenomenon, is, however, I think, proved by the following consideration. After one half of

* 'Il Cervelletto,' Firenze, 1891.

the cerebellum has been removed the ocular displacement which occurs is sooner or later recovered from, so that the globes return to their normal positions as before the operation. Now if such an animal be placed under the influence of an anæsthetic, after such recovery has taken place, at a stage of the narcosis preceding that at which the globes diverge in deep coma, the displacement noted as an immediate effect of the cerebellar ablation is reproduced. This can only mean that the nerve centres governing the movements of the eyes to one side succumb to the influence of the anæsthetic before those on the opposite side, and that it is, therefore, a phenomenon that can in no sense be regarded as irritative, but must rather be looked on as paralytic in nature.

Clinically we rarely meet with the turning of the opposite eye outwards and downwards, a position which is so characteristic of a unilateral ablation of the cerebellum; but I have been fortunate enough to see some well-marked instances of such ocular displacement, both in the case of abscess and of tumour of the cerebellum. On the other hand, in consequence of the existence of increased intra-cranial pressure, which is so common an accompaniment of cerebellar lesions in man, we meet with displacements of the globes which are quite different from that characteristic of a unilateral cerebellar lesion. Such displacements may not only confuse us, but they may actually mislead us if we do not recognise their true significance. A clinical fact, for which I can offer you no satisfactory explanation is that the opposite sixth nerve appears more prone to suffer from pressure in these cases than is the corresponding nerve on the side of the cerebellar disease, so that weakness of the external rectus of the opposite eye may be present, and the globe may accordingly turn more or less inward, *i.e.* to the side of the cerebellar lesion. We thus see that the displacement of the eye which occurs is in the opposite direction to that which is characteristic of a unilateral cerebellar lesion where pressure is out of the question, and where we have only to deal with a defect consequent on destruction of cerebellar tissue. This phenomenon has, to my knowledge, been responsible for errors in localisation, owing to its being assumed that the sixth nerve on the side of the cerebellar disease is more likely to suffer from pressure

than is its fellow of the opposite side, so that weakness of one external rectus has been taken to mean that the tumour (the most common condition present in these cases) is situated on the same side of the cerebellum. Those of us who are aware that ocular displacements may occur as a result of cerebellar defects quite apart from any question of pressure, may possibly be misled by supposing that the displacement of the globe is of this character, and not the result of pressure on the sixth nerve: and this is the more likely to occur when the weakness of the external rectus of the one eye is associated with weakness of the internal rectus of the other eye; both eyes then turn to the side of the lesion, but in that, according to Luciani, both eyes may turn away from the side of the lesion where the defect is due to loss of function on the part of the portion of cerebellum destroyed, it is not unnatural that the displacement of the globes should lead us to locate the morbid condition on the wrong side of the cerebellum. Happily there are several considerations which may save us from this error. In the first place, it must be remembered that in a displacement of the eyes the direct result of a cerebellar lesion it is the opposite eye which is at any rate most displaced; that, according to my own observations, the eye on the side of the lesion is not displaced inward, and that the direction of the displacement of the opposite eye laterally is outwards. On the other hand, the displacement of the globes consequent on pressure exerted on the ocular nerves is such that the eye that turns inwards is usually that most displaced; and indeed it may be alone displaced, as the one sixth nerve may alone suffer from the effects of the pressure.

In attempts at intra-cranial localisation* in general we have to accept with caution any evidence of the seat of lesion supplied us by parietic conditions of parts presided over by cranial nerves, in that one or other nerve may suffer from the effects of general increase of intra-cranial pressure. Certain combinations of defects occur which leave no question in our minds as to the significance and importance of the affection of cranial nerves met with; while in other instances, as I have already said, we have to accept with the utmost caution any evidence thus supplied to us. In no instance is such reserve more necessary than in

paralysis of one or both sixth nerves, for their small size and long intra-cranial course render them particularly prone to suffer from the effects of increase of intra-cranial pressure. Nevertheless weakness of the external rectus of the opposite eye has been sufficiently frequently met with in unilateral lesions of the cerebellum in man to make the sign of some clinical value, taken in conjunction with other more reliable indications of the probable seat of the morbid process. As has already been said, this weakness of the external rectus of the opposite eye with consequent internal strabismus may be the only external ocular defect present, if we exclude nystagmus. On the other hand, it may be associated with a similar weakness of the external rectus of the eye on the side of the lesion, so that convergent strabismus results; but even in these cases the defect of the external rectus of the opposite eye is more pronounced, as a rule, than is that of the corresponding muscle of the eye on the same side as the cerebellar lesion in the cases that I have myself observed or have seen recorded. So, too, instead of the external rectus on the side of the lesion being the muscle which suffers in conjunction with the external rectus of the opposite eye, it may be the internal rectus of the eye on the side of the cerebellar lesion that is so affected. The displacement of the globe consequent on the weakness of the internal rectus on the side of the cerebellar lesion is the only one that should cause us any real difficulty, for such a displacement of the globe outwards may, as we are aware, result from ablation of the opposite half of the cerebellum, or, as we shall possibly see on some subsequent occasion, of the lateral half of the middle lobe on the same side as that to which the eye turns. Inward displacement of a globe does not, so far as my observations go, result from ablation of one half of the cerebellum. As has already been said, Luciani certainly speaks of displacement of both globes to the side opposite to the lesion; but while I find this true in so far as the opposite eye is concerned I have never been able to satisfy myself that the displacement of the globe on the side of the lesion amounts to much if it occurs at all; so that inward displacement of either eye to any extent if met with clinically ought not, it appears to me, to be regarded as a direct effect of the cerebellar lesion present, but rather as an indirect

effect consequent on pressure on the sixth nerve concerned. Moreover, as has already been said, if any weight is to be given to the existence of a convergent strabismus of either eye, the cerebellar lesion must be regarded as situated on the side towards which the squinting eye turns,—that is to say, on the side of the cerebellum opposite to that of the eye whose external rectus is weak. My object is not so much to advocate the value of this sign as an aid to localisation in clinical cases, as to guard you against two fallacies: one is that of regarding inward displacement of one eye as indicating that the lesion of the cerebellum is situated on the side corresponding to the weak external rectus; and the other fallacy is that of supposing that if the displacement is the result of weakness of the external rectus consequent on pressure on one sixth nerve, the nerve more likely to suffer is that on the same side as the cerebellar lesion,—for the reverse is, as a rule, the true state of things.

Another ocular defect which is of great importance in the localisation of cerebellar lesions is nystagmus, either spontaneous, or evoked when the eyes are moved from their resting position. Nystagmus commonly occurs in association with the displacements of the eyes that have already been described, both when these are the direct results of the cerebellar lesion, and when they are secondarily induced by pressure on ocular nerves,—or this phenomenon may be present when no abnormal position of the globes can be detected. When the nystagmus is associated with ocular displacements consequent on pressure on one or other of the nerves which supply the muscles of the eyeballs, the special features which characterise nystagmus, the direct result of the destruction of one half of the cerebellum, can as a rule no longer be recognised. Where no ocular displacement occurs at all, or where the displacement met with is that characteristic of ablation of one half of the organ, and where it is not necessary to invoke secondary pressure on any of the ocular nerves to account for the condition, the nystagmus has very special characters which it is important that we should recognise. In the first place the nystagmus is lateral; and in the next place when not present spontaneously it may not be evoked at all, or only in slight degree when the globes are voluntarily directed away from the side of the lesion, whereas it is pronounced when the

eyes are voluntarily turned to the side of the lesion, *i. e.* in the direction opposite to that in which the opposite globe is displaced as a direct result of the unilateral ablation of the cerebellum. Even in animals it is comparatively easy to determine this, and in some excellent examples which I have met with clinically it has been possible to determine the characters of the nystagmus with precision. If spontaneous nystagmus exists, voluntary turning of the eyes to the side of the lesion intensifies it, and the nystagmus may become coarser, the range of movement of the globes becoming greater. If no spontaneous nystagmus exists, voluntary turning of the eyes away from the side of the lesion may evoke little or no nystagmus, and in any case the nystagmus evoked on voluntarily turning of the eyes to the side of the lesion is much more pronounced than that evoked by turning of the eyes to the opposite side. It is as if the voluntary effort to keep the eyes directed to the side of the lesion has to struggle with some opposing force which resents the interference of volition, and which is bent on pulling the eyes back from the direction to which they have been turned voluntarily, at one moment the one force, at another the other gaining the ascendancy, so that the globes are constantly being pulled from side to side in a lateral direction. Or, interpreted otherwise, it is as if the muscles concerned with turning of the eyes to the side of the cerebellar lesion are weak, and unable to keep up a sustained action sufficiently powerful to overcome the normal tonic influence of the muscles concerned with turning of the eyes away from the side of the lesion. The rate of nystagmus varies in different cases, and when present clinically when the eyes are directed to either side, both the rate and the degree of coarseness of the nystagmus may differ according to the side to which the eyes are turned.

Proptosis and narrowing of the palpebral fissure are other phenomena which may be met with, about which, however, I prefer to say nothing definite at present, in that further observations seem necessary to determine their true significance.

Facial paralysis.—In speaking of the hemiplegia which results in some cases of cerebellar disease I told you that it was characterised by affection of the limbs on the side of the lesion, but that the

face escapes. Now this is true only in so far as any direct effect of ablation or destruction of the cerebellum is concerned, for the face may become affected secondarily in consequence of pressure exerted by the tumour, connected with the cerebellum, on the facial nerve at the base of the brain in its course from the pons to the internal auditory meatus. In a recent discussion at the Neurological Society Dr. Beevor brought forward evidence to show that implication of the facial nerve in this way, together with the auditory, which we shall presently see may be similarly affected, occurs in cases of tumour of the posterior fossa, while when the tumour occupies the part known as the cerebellar fossa both the facial and auditory nerves may escape, so that there may be neither facial paralysis nor deafness. The facial paralysis has of course the characters of one of peripheral origin, so that the upper part of the face suffers equally with the lower part; there is no difference between the results of emotional and volitional movements, and the electrical excitability of the affected muscles may show the changes characteristic of a peripheral lesion.

This facial paralysis may occur without any evidence of hemiplegia in so far as the limbs are concerned; but if hemiplegia directly due to cerebellar defect chanced to be also present, the affection of the face and limbs would be on the same side of the body as in an ordinary hemiplegia of cerebral origin,—with this difference, however, that the facial paralysis would present the features which characterise one of peripheral origin. On the other hand, it is conceivable that a tumour connected with one side of the cerebellum may be responsible for facial paralysis of peripheral type on the same side as the tumour, and of paralysis of the limbs on the opposite side of the body in consequence of secondary pressure on or infiltration of the pons.

Deafness.—As has already been said, tumours connected with the cerebellum may cause deafness of the ear on the same side as a result of pressure on the auditory nerve in conjunction with the facial. The deafness is perosseal, so that a tuning-fork placed on the mastoid is not heard on the affected side.

These two signs, *viz.* facial paralysis of peripheral type and nerve-deafness, are of the greatest possible value as aids to localisation in clinical

cases, and point to the lesion (usually tumour) being situated on the side on which these defects are detected. It was these two signs more especially that I had in mind when, in speaking of the way in which increase of intra-cranial pressure distorts the clinical picture, I said that pressure was not entirely an unmixed evil. Be it observed, however, that these effects on the facial and auditory nerves are not the result of general increase of intra-cranial pressure (our chief bugbear), but of direct pressure of the tumour.

Sensory phenomena.—Despite the difficulties which attend the testing of sensibility in animals, a large number of observations have convinced me that a certain degree of anæsthesia and analgesia results after ablation of one lateral half of the cerebellum, and that the distribution of this sensory defect corresponds to the distribution of the motor defect, so that it is met with in connection with both limbs on the side of the cerebellar lesion and in the opposite posterior limb. The limb which suffers in greatest degree is the posterior limb on the same side as the cerebellar lesion, and it is in this limb that the sensory defect persists for the greatest length of time. In common with most other defects consequent on cerebellar ablation, the anæsthesia and analgesia rapidly recover, but the recovery takes place in connection with the other limbs before the hind limb on the side of the lesion. I would ask you to carefully note, however, that these sensory defects have only been met with as a result of experimental lesions in animals, and that no instance of defect of sensibility, a direct result of a cerebellar lesion in man, has come under my observation, nor am I aware of any recorded case of cerebellar affection in which such a defect of sensibility has been noted clinically.

Neuropathy in Relation to Congenital Cardiac Lesions.—Dr. Riberolles ('Progrès Médical,' July 24th, 1898), after a careful examination of a large number of neuropathic subjects in whom cardiac lesions were found, has rejected coincidence as an explanation of the frequency of the co-existence of neuropathy and the cardiac defect. He concludes that there exists a cardiovascular heredity related to anomalies of the circulatory system which causes the subject to manifest, under stress, neuropathic disorders, generally intermittent or slight, but capable by frequency of developing definite nervous lesions.

Medicine, October, 1898.

CLINICAL LECTURES ON MUSCULAR TREMOR AND CLONIC SPASM.

Delivered at the Hospital for Diseases of the Nervous System, Welbeck Street, by
THOMAS D. SAVILL, M.D.Lond., D.P.H.Camb.,
Physician to the Hospital.

LECTURE II.

Cases of Localised Clonic Spasm.

GENTLEMEN,—At our last meeting we were engaged in the consideration of the various diseases giving rise to choreiform and other clonic spasms. I was fortunately able to show you several interesting cases of St. Vitus' dance, rhythmical or hysterical chorea, and post-hemiplegic chorea. It now remains for us to consider, before passing to the subject of tremors, or small, rapid, and usually rhythmical movement, a group of disorders attended by localised clonic spasm—habit spasm, facial spasm, wryneck, and fatigue spasm; together with the general questions of the prognosis and treatment of clonic spasmodic disorders.

6. *Habit spasm.*—Habit spasm, from a clinical point of view, is the first cousin of chorea, and consists of intermittent twitchings, which only differ from chorea in that the movements are always of the same kind and in the same locality. They may affect any part of the limbs or trunk, or even the respiratory muscles, but they are most usually met with in the face or arm. The "grimaces" of children form the most familiar illustrations of the condition. They nearly always start in children, and may be regarded, in point of fact, as an exaggeration of the restlessness of that age-period. They are unaccompanied by any pain or any paralysis. This boy, aged 17, whom I show you, was affected for eighteen months with an inconvenient and inelegant spasmodic jerking backward of the head, due to clonic spasms of the muscles at the back of the neck. It is always advisable in such cases to seek for any chance source of reflex irritation, and upon careful investigation I found that this

boy had granular pharyngitis, and a considerable enlargement of Luschka's tonsil. My colleague, Dr. Dundas Grant, very kindly saw the case a few weeks back, and has cured the throat condition by appropriate measures. The correctness of the surmise that the habit-spasm was in some way associated with the throat irritation is confirmed by the fact that the spasm at once disappeared on relieving the throat condition. The movements have slightly returned lately owing to an injury which he received from a fall on the back of his head; but I hope that by rest and small doses of bromide he will soon be well again.

Heredity plays an important part in the production of these cases,—a fact which this case illustrates. An inherited irritability of the nervous system is undoubtedly in operation in the majority of these cases. But an attack is often determined by "imitation." That is why it is so frequent in children, who unconsciously imitate the movements they see. Then, once started, any movement more readily recurs because the nerve-path travelled by the impulse becomes a path of less resistance. These considerations are important from the point of view of treatment; and the remarks I shall have to make *apropos* of facial spasms apply with equal force here.

7. *Clonic facial spasms*.—The facial muscles are sometimes the seat of persistent clonic spasms, accompanied or not by a certain degree of tonic contraction. The condition is variously known as convulsive tic, tic non-douloureux, and "mimic spasm," but the last name is an unsuitable term. This condition, like several others we have to consider, is only a symptom, and the causes of facial spasm, gentlemen, like the causes of pain or neuralgia,—in nearly all positions—may be grouped under three categories: organic, reflex, and functional or constitutional, and these should always be kept in mind in the investigation of any case of facial spasm. *First*, organic lesions of some part of the facial nerve or tract may be attended by facial spasm, more especially if the lesion be of such a kind that it damages *without completely destroying* some part of that structure. Here again, then, we have confirmation of the pathological data mentioned at our last meeting, and I need not do more than mention the fact in passing. Twitchings are not uncommon at the

commencement of Bell's paralysis, and they are sometimes present in the later stages of these cases as the nerve gradually recovers. In this way any of the lesions which give rise to Bell's paralysis may be a cause of facial spasm, but certain of them are more liable than others. Thus an aneurysm of the vertebral artery, any tumour pressing on the trunk of the nerve, or an embolic lesion of the cortical facial centre, are three examples of lesions which damage but do not—for a considerable time—destroy the nerve-fibres. These are, as a matter of fact, much more liable than other causes of Bell's paralysis to give rise to facial twitchings. Organic causes of this spasm may be recognised by the presence, or the history, of a certain amount of paresis; by the paralyses usually being on one side only; and by the intermittent twitchings being usually attended by a certain amount of tonic spasm as well. Moreover, this form of facial spasm may occur at any time of life, whereas the other two groups of causes are practically unknown in childhood.

A *second* and much rarer cause of facial spasm is some reflex irritation, especially of the trigeminal nerve, which is the associated sensory nerve of the facial. In this way, exposure to cold, decayed teeth, irritation of the skin of the face, injury to the fifth, and various ocular defects, especially errors of refraction, may any of them give rise to facial spasms. Facial spasm due to causes of this nature is usually on one side only, it may be clonic, or tonic, or both. Practically these causes can only be differentiated from the others by finding the source of the reflex irritation and removing it, as we did in the patient I showed you with a habit spasm.

The *third* group of causes are of a functional or constitutional kind, and how they produce facial spasm is at present unknown. Various general conditions,—such as neurasthenia, hysteria, and especially the climacteric state, or general debility—may be attended in some obscure way by facial spasm. In all of these there is a general irritability of the nervous system, and especially of the spinal cord—that condition, in short, which we have seen may be called "strychninism," and usually evidenced by an increase in the deep reflexes if the leg centres are also affected. An unusually localised chorea must not be mistaken for facial spasm: but here the age of the patient, the chorei-

form character of the movements, the absence of any tonic spasm in the facial muscles, and lastly a thorough examination of the limbs for choreiform movements, will generally enable one to come to a conclusion. Epilepsy is sometimes preceded or followed by facial spasm, but in such cases it is usually tonic, and the history of the attacks of either minor or major epilepsy reveal the condition.

Finally, it must not be forgotten that facial spasm may also grow out of, or result from, a persistent habit spasm. In such cases it is always clonic in character, though sometimes tonic as well. Here is a young woman, E. H—, æt. 21, whose case probably belongs to this class. As a child, up to the age of fourteen, she was the subject of blepharospasm—eye-blinking. After that, up to the age of twenty, she was troubled with frequent attacks of twitching of the nose, and since the age of twenty she has been subject to twitchings of the face and mouth. These attacks seem to come on without cause, and the movements distort the face very much, so that she cannot keep a situation in which appearance counts for much, though otherwise she is very comely. They are always much worse, she tells us, at the catamenial period, and whenever she is in the least degree “out of health.”

The best treatment for such cases, for habit-spasm in general, and for cases of facial spasm arising from functional causes, is to remove them from home. This method cannot be too much insisted upon. Habit-spasm is of very frequent occurrence, at any rate in the out-patient department here, yet the method of dealing with it is not sufficiently described in books, and the condition is sometimes very troublesome to get rid of. If possible I always take them into hospital, for they hardly ever get well at home. The parents correct and scold them for the habit, and the more they scold the worse the habit becomes. Sometimes the offer of a reward will help the child to check it; in other words, they are more amenable to a system of “voluntary” than of “obligatory” morality. But the great thing is to get them away from their parents for a time, not only for the reasons just mentioned, and not only because it is important to remove them from the conditions under which the disease arose, but also for the fact that in these nervous subjects, as in cases of hysteria, there are very often irritating surroundings

in the home which do not obtain elsewhere, which act as a continual source of annoyance. I have a patient at the present time under my care who is subject to clonic spasms which I believe to be of hysterical origin, although they are of seventeen years' duration, on and off. But as I cannot get her to leave home she will probably never make much improvement. Those who study the social surroundings of their patients become aware that the “angularities,” if I may use the word, of different members of the same family, and especially mother and daughters, often jar very much upon each other, even apart from any incompatibility of temper. And if, as in another case which I shall shortly mention to you, we can only get them right away from the uncongeniality of home, they get well directly.

8. *Wryneck* may be of two kinds; first, the congenital, which is a fixed torticollis, due to a persistent or *tonic* muscular spasm (often accompanied by organic contracture of one or more muscles), and which is the consequence very generally of injury during birth. This form does not concern us here; but, secondly, there is another kind, which is spasmodic or acquired torticollis. Here the spasm may be clonic or tonic, or (more usually) both; and because in a large number of cases no organic lesion is discoverable, it is usually classed among the functional disorders. This arises under many of the same conditions that facial spasm does, and our inquiries should be directed on the same lines.

Injury I believe to be a not infrequent cause of this condition, and it is interesting to note that the spasm may not result in wryneck until some time has elapsed after the accident. A gentleman, aged sixty-five, now under my care in private, fell upon his head in a coach accident in 1894, and in falling incurred a severe blow on the chin. He suffered only a moderate amount of pain in the neck at the time, though he was laid up six weeks with a bad leg. It was not until the long period of six months had elapsed that he began to have severe pain in the region of the occiput—to use his own phrase, he suffered “the torments of the damned”—and thereupon his neck began to twitch towards the right. The twitchings now come on whenever he attempts to write, and are so severe and painful that he is precluded from letter-writing

altogether. I believe in his case that the spinal accessory nerve was in some way injured at the time, and as a possible means of relief I have suggested division of nerve trunk or the sterno-mastoid tendon. Whether it would be of much use is a question which cannot be definitely answered. He has derived partial relief from galvanism.

Fatigue spasms and other muscular twitchings.—

The clonic spasms to which I refer here must be familiar to most of us as we are dropping off to sleep at night after an exhausting day of muscular exercise. The legs twitch, sometimes the arms also; but if the exercise has been walking, it is only the legs. There can be little doubt but that these are due to the toxic condition of the blood resulting from the increased metabolism, combined with the irritability of certain parts of the motor tract produced by over-functioning. These same twitchings may also be observed in dyspeptic and gouty persons, where probably the former cause alone is in operation. "Occupation tremor" is an instance of the latter cause acting through a long period of time.

Twitching of the limbs as the patient goes to sleep is an early sign of peripheral neuritis, a fact which we should always bear in mind when we are consulted for this symptom. You will observe that it occurs at a stage when the damage is not sufficient to destroy the motor nerve fibres, but only enough to irritate them.

You will remember also that paraplegia, due to complete transverse lesions which leave a healthy area of the cord below, is attended by spontaneous twitchings of the legs. This is generally explained by supposing that the "inhibitory influence" of the higher centres is cut off. But, gentlemen, does it seem more probable to us who have been studying the subject of clonic spasms in its wider aspect, that it may be caused by the irritation of the reflex centres of the legs in the spinal cord due to their involvement in the irritative zone or fringe which we know exists at the lower border of the lesion?

The *prognosis* of clonic spasm is as a rule much more hopeful than in cases of paralysis of the same parts, at least if we except the organic cases. This follows from our conclusions as to the pathology. Whereas paralysis or tonic rigidity arise

from complete destruction or irritation respectively of a motor strand, tremors and clonic spasms are an evidence that the motor tract is only the seat of *partial* or temporary destruction or irritation. Some part of the motor strands must remain healthy for the clonic spasms to occur. The course and clinical history of all the conditions we have been considering is in keeping with the view I have enunciated. Some of them, like chorea, tend to run a definite course towards spontaneous recovery. Many of them—some cases of habit spasm and hysterical spasms, for example—are only temporary; and nearly all yield to appropriate treatment, if we except organic affections such as post-paralytic clonic spasms.

In none of the conditions we have been considering is there any immediate danger to life, excepting a few rare cases of chorea. The prognosis of chorea depends largely on the age of the patient and the violence of the movements, the condition being much more intractable as age advances. The presence of complications also enters into the question, cardio-valvular disease occurring in something like one third, and leading to a chronic condition just as rheumatism does. Pregnancy is another serious complication of chorea (about 30 per cent. of such cases, according to Barnes, being fatal). Such cases, when the movements are of great violence and insomnia intractable, are very unfavorable.

Treatment of clonic spasms.—The rational treatment of muscular spasm follows in like manner from the pathology. The indications are to remove the cause and to restore the damaged structures, and the latter readily ensues on the former in most cases. The treatment of the individual diseases has for the most part been dealt with in each case. In chorea, arsenic, gradually increased to toleration, seems to be a specific; but if arsenic fails we have many other similar nervine tonics to fall back on. The salicylates and iodides are of undoubted use in many cases which resist other remedies, a fact which gives it another link of association with rheumatic affections. Chloral and chloroform are useful in severe cases.

Tremors and spasms of hysterical origin must be treated on general lines as in other hysterical affections. In these cases, and in those of functional origin, reflex irritation should be carefully

inquired for, such as irritation of the fifth nerve by decayed teeth, or some error of refraction, as well as uterine, stomach, and other visceral troubles. Opium and morphia are permissible in some severe cases. Strychnine, as a rule, in my experience is contra-indicated in nearly all clonic spasms. Bromides are undoubtedly of great value in habit spasm, facial spasm, and for some hysterical tremors; and if these fail, cannabis indica, conium, physostigma, and other sedatives may be tried. Galvanism, applied either to the limbs, or still better as a descending current to the spinal cord, gives as much relief to post-paralytic cases as anything I know of. Warm baths and Turkish baths are useful palliatives. The application of heat in the form of hot water internally and externally is one of the best remedies for clonic spasm that I know of. The very simplicity of this remedy renders it of all the more value.

THE USE OF LARGE PROBES IN STENOSIS OF THE LACHRYMAL DUCT.

By MELVILLE BLACK, M.D.

IN the presentation of this subject no reference is made to such mild cases of lachrymal stenosis as can be relieved by probing the canaliculi with small probes, followed by syringing, nor to cases dependent upon closure of the lower opening of the duct by pressure from the inferior turbinated body. The class to which I desire to call attention is that of true stenosis of the lachrymal duct, in which mild measures and nasal treatment are of no avail.

In order to pass a large probe down the lachrymal duct it is first necessary to slit a canaliculus. It is very important that the canaliculus of lesser function should be selected. The punctum and canaliculus of the lower lid unquestionably have greater function in draining the palpebral aperture of fluid than have the corresponding parts in the upper lid. Once the canaliculus is slit and its punctum destroyed, its suction power is lost, and it ever after acts in an indifferent manner. If the lower canaliculus is slit, probes passed, and the duct otherwise treated until it is thoroughly patent, this eye will never again be free from epiphora

when exposed to inclement weather. On the other hand, if the upper canaliculus is chosen as the means of gaining access to the lachrymal duct, it will be found that after the passage is rendered patent the lower canaliculus will be able to keep the eye free from tears. I deem this point of great importance, and believe that the many failures in the cure of lachrymal duct stenosis are due to the destruction of the suction power of the lower canaliculus.

If probing the lachrymal duct for the relief of stenosis is to be of any advantage we must observe the same rules as in treating stricture of the urethra. The duct must be dilated *ad maximum*. It is my custom to pass, at the first sitting, the largest probe that can be introduced. The canaliculus in the upper lid is slit with a Graefe cataract knife, with the aid of a Bowman's grooved director. The director is passed through the canaliculus, then turned to the vertical and passed to the bottom of the lachrymal sac. The Graefe knife is used with the director in the vertical position. The director is now removed and a graduated probe introduced, to determine the size of the duct. If a marked stricture is encountered, an Agnew's knife is passed down the duct and the stricture cut. If no stricture is found, the Agnew knife is not used. The size of probe that the canal will admit can be fairly well judged by the passage of the graduated probe. I find that it is rarely that a No. 10 Theobald probe cannot be passed. If the probe first passed is loose in the canal, one after another of larger size should be tried until one is found that is passed with difficulty and completely fills the canal. Strong pressure should be exerted on the mucous membrane throughout the entire length of the canal. It is my custom to examine the nose to see that the probe is well down in the inferior meatus. If it is not, this or a smaller probe is forced down until it can be seen or felt beneath the inferior turbinated body. The probe is allowed to remain *in situ* about ten minutes. It is then withdrawn, and the lachrymal passage syringed with a saturated solution of boric acid until bleeding ceases. The patient is then sent home, and instructed to apply lead and opium wash on pledgets of lint which have been cooled on a block of ice. This should be done almost continuously for twenty-four hours. At the expiration of this time I inject,

with a lachrymal syringe, into the duct a few drops of 10 per cent. solution cocaine. A probe, a size smaller than the largest one passed the day before, is then passed. The negative pole of a galvanic battery is then connected with the probe, the positive pole being placed in the patient's hand. Then from two to three milliampères of current is turned on for ten minutes. When the probe is removed it will be found to be very loose, owing to the electrolytic action of the cathodal current. The lachrymal passage is syringed free of blood, as on the day previous. There is usually a little bleeding after the removal of the probe for the first four or five days. The patient is instructed to use the lead and opium wash for four hours in the same manner as on the day previous. On the third day the same probe is passed, unless it is found to be loose, in which event a size larger should be used. The 10 per cent. cocaine solution should have been previously used to relieve the pain. The probe is allowed to remain in the passage ten minutes. After its removal the syringing is practised as before. A few drops of 25 per cent. solution ichthyol in water are now syringed into the passage. The probe is passed daily for about ten days, accompanied by the syringing of saturate solution of boric acid and ichthyol solution. The largest probe that the duct will take should be used. At the end of the first week the battery should be again used in the manner described. After the first ten days the above procedures should be carried out three times a week for two weeks, then twice a week for two weeks. After this it should be done according to indications until all epiphora ceases. The use of electricity should be practised every week or ten days for the first six weeks, and after that according to indications. It is impossible to lay down hard and fast rules in these cases. I generally say to my patients that a cure may be expected in two months. I am frequently able to dismiss them sooner. My experience has been that a No. 10 probe can be passed at the first sitting, and that in a few days a No. 13 or 14 can be passed. I have passed the No. 16 probe in a few cases.

When blennorrhœa or phlegmon is present I expect complete disappearance of these symptoms during the first week. When superficial necrosis of the lachrymal bones exists treatment is some-

times more protracted, and has to be persisted in until the necrosed bone is absorbed. In cases of fistula with sequestrum of bone it is necessary to cut down and remove the sequestrum.

The three points that I desire especially to emphasise are the opening of the upper canaliculus, the use of the largest probes possible of passage, and the electrolytic action of the negative pole of the battery.—*Journal of the American Med. Assoc.*, October 8th, 1898.

The Repair of Wounds of the Brain.—

Tschistowitsch ('Boston Med. and Surg. Journal') finds that in the process of healing and of filling up losses of substance the connective-tissue elements of the pia and of the blood-vessels play the chief part. The neuroglia takes a very insignificant part in the process, and that consisting only in the formation of a sclerotic zone about the cicatrix or foreign body when there has been long-continued irritation. He thinks it likely, by a more gradual destruction of the brain tissue than was possible in his experiments, this growth of the neuroglia may become relatively more extensive, as in progressive degenerative processes in brain disease. No regeneration of nerve cells was observed. With regard to the regeneration of nerve fibres the author is in some doubt, but does not deny that it may occur. He found that the epithelium of the ventricle had little power of regeneration.—*Maryland Med. Journal*.

Neuralgia.—

R Chloral,
Menthol,
Thymol āā 1 part.
Camphor 3 parts.
M. S. Apply locally.

NORTH-WEST LONDON CLINICAL SOCIETY.

THE Annual Address, by Mr. VICTOR HORSLEY, F.R.S., on "The Rational Treatment of Goitre," will be given on Thursday, October 27th, at 6.30 p.m., at the Café Royale. The first Annual Dinner of the Society will take place at 7.30 on the same evening at the same place.

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A CLINICAL LECTURE ON HERNIA.

Delivered at the Westminster Hospital, Oct. 11th, 1898,

BY

CHARLES STONHAM, F.R.C.S.

GENTLEMEN,—As I have several cases of hernia in the wards at the present time, I will take the opportunity of making some remarks upon the subject.

Hernia is an exceedingly important condition; in the first place because it is so common, and in the second because it is very common amongst children, and is very liable to make a child grow up more or less handicapped for life. The possible dangers are most important; for example, incarceration and strangulation, even if there were no others, would be sufficient cause for regarding the malady seriously, since acute intestinal obstruction is most commonly due to the latter condition.

The term hernia is generally meant to imply the escape of some part of the contents of the abdomen through an opening in its wall. Of course there are other forms of hernia, such as hernia of the lung and of the brain, and so on, but the word hernia alone is always meant to imply the abdominal variety.

First with regard to the general anatomy of hernia. In the great bulk of instances you find the hernia escapes through some potential canal in the abdominal wall, such as the crural or inguinal. Sometimes the hernia escapes through an abnormal opening in the abdominal wall, such as may be formed by the yielding of a cicatrix after an operation, or consequent on disease. You also find that abnormal openings or weak spots are sometimes made in the abdominal wall as the result of great muscular strain, especially if the muscles have been previously weakened by over-distension of the parietes; this is especially noticeable in the case of women who have had

several pregnancies. Again you may find that the escape of any hernia is dependent upon certain congenital defects. Of course it may be very well said, and with a certain amount of justice, that the formation of hernia at the inguinal or crural canal is due to what is a congenital defect. Well, that is true in one sense, and not in another. If the boundaries are of normal strength the inguinal or crural canal is properly safeguarded, and there can hardly be said to be a congenital defect in those regions.

Now with regard to the sac of a hernia; this merely consists of the peritoneum which it pushes in front of it. Occasionally it is totally absent, *e. g.* cystocele. The sac consists of neck, body, and fundus. The condition of the sac at the neck is a matter of great importance with regard to the prospects of cure, and also with regard to the position of constrictions in strangulation. This can be best illustrated with a ring and a pocket handkerchief. Let us suppose that the ring represents the inguinal or crural canal, and the handkerchief represents the peritoneum; at the early stage of the hernia of course the sac is quite small, and if you look at it from inside you see merely a little depression. But as the protrusion gets bigger the peritoneum is further pushed through the ring. The obvious result is that the peritoneum must be folded at the neck; these folds are in close apposition one to another. As this sac begins to pass down more and more, it gradually begins to expand at the fundus, but the folded condition of the peritoneum at the neck becomes more pronounced. In quite the early stage of the affection the folds can all be undone; you will see that as I pull my handkerchief out of the ring they immediately disappear. But very soon, when a hernia is more fully formed, you will find that with irritation of the neck of the hernia these folds become agglutinated, as the result of adhesive inflammation; and with this is associated an irritative overgrowth of the cellular tissue outside the sac, the net result of the two conditions being, that if you can keep the hernia up while that irritative overgrowth and gradually adhesive inflammation in the neck of the sac continues, you may get a radical cure performed by nature, but this only occurs in children. But, on the other hand, the gradual contraction and narrowing of the neck may occasion strangulation of the hernia.

Here I may remind you that in the case of strangulated hernia, in the bulk of instances the point of strangulation is at the neck, and very often in it. When a hernia escapes from the structures among which it first lies, it begins to alter its shape. If you take the inguinal canal, you find that as a hernia passes along it the sac is shaped like the finger of a glove, because it is surrounded and supported by the structures of the abdominal wall, but directly it gets out of the ring and into the loose scrotal tissue it begins to expand; the neck is in the inguinal canal, but the fundus is in the scrotum, the hernia being somewhat flask-shaped. But some herniæ you will not find this shape. For instance, we have a child in the hospital now with a congenital hernia through the triangle of Petit, in which the mouth of the sac is as broad as the fundus. This is obviously an advantage, for as long as the mouth of the sac remains as broad or nearly as broad as the main bulk of it strangulation is impossible, but directly the neck of the sac begins to narrow, and especially if the narrowed part be long, strangulation can very easily happen; you have only to force down an extra piece of omentum or another coil of bowel, and strangulation occurs at once. I should mention that occasionally you get what is known as an hour-glass sac; that is, the same process of adhesive inflammation that occurs at the neck may also occur in the body of the sac, and if it does the hernia becomes constricted in the middle like an hour-glass. That is not a matter of very great importance provided you are acquainted with the fact when you have to deal with a strangulated hernia. It is important then because you might attempt taxis with a strangulated hernia, and push the contents from the lower into the upper part of the sac, and go away contented with the idea that you had reduced the strangulated hernia, whereas you would have done no such thing. The hour-glass sac, however, is not a common form.

Next with regard to the contents. In the great bulk of instances a hernia contains ileum—usually the lower end; but you will find that, as a rule, incarcerated hernia contains colon, to which I shall refer presently. But you may find practically anything in the abdomen within the sac of a hernia; even a pregnant uterus has been met with in ventral hernia, which occurs particularly in women who have had many children. Occasionally you

find, but only in a case of femoral hernia, that a portion only of the gut is constricted; this is known as Richter's hernia. Generally the symptoms are not quite as severe as they would be were the whole calibre of the gut affected; but the danger is even greater, because if the ring is so narrow that only a little piece of gut can come down, you may be certain that the constriction is very tight indeed, and therefore gangrene is very liable to occur; another powerful factor in the matter is that a Richter's hernia is necessarily a very small one, and therefore is very likely to escape one's attention until perhaps even gangrene has set in. Just before I went away I had in the ward three cases of Richter's hernia in succession. Fortunately they were all seen pretty early, and all got well; in one, however, a small piece of the bowel gave way, the fistula healing spontaneously.

There is a hernia which is known by Littre's name. I have once seen it in the operating theatre, and for a short time it puzzled me as to its nature. It consists of a hernia of Meckel's diverticulum, and forms a long sausage-shaped tumour consisting of a single tube. The case in which I met with it was a femoral hernia on the left side. I have only seen one other, and that in a specimen which I dissected some years ago. Another thing which you may find in the sac of a hernia is omentum. Omentum is very commonly met with; indeed, it is almost invariably present in large herniæ, especially umbilical. When omentum is present you will find that it always retains its normal position in relation to the gut,—that is to say, lies in front of it. This is important to remember in operating for strangulation, for you may cut down upon it and see nothing but a mass of black discoloured omentum, but if you lift that up you will probably find a knuckle of gut underneath it, but never in front.

Another thing which one ought to mention in the anatomy of the sac, although I suppose it is strictly the pathology of the sac, is the presence of adhesions. They form in hernial sacs by a very gradual process of constructive inflammation, and consequently there are usually no symptoms, and no history of any inflammatory trouble; but occasionally the sac is known to have inflamed. The adhesions may form between the coils of intestine and the walls of the sac, or between one coil of intestine and another, or with a piece of omentum,

or between one part of the wall and another. In any of these cases the adhesions are dangerous. When the coils of gut are matted together the hernia is irreducible, for although it is not bound down in the sac, yet its very bulk prevents it being got back. Supposing that the coils of gut are adherent to the wall, that naturally makes the hernia irreducible, because the sac is itself always irreducible. On the other hand, supposing there is a bridle of adhesion stretching from one part of the wall to another, that bridle may act as a constricting band to a piece of gut that comes underneath it.

Another matter to consider in the anatomy is the coverings of a hernia. You often find in books on surgery and anatomy elaborate accounts given you of the coverings of the different forms of hernia. They are very good, no doubt, as anatomical exercises, but to an operating surgeon they are practically useless. The coverings naturally differ with the situation of the hernia, and they consist of course of the structures between the sac and the skin. In inguinal hernia there is only one of these coverings that you can identify,—namely, the cremasteric fascia; and you are enabled to do so because you can see the strands of cremasteric muscle lying in it. I have often seen people operating on hernia, and demonstrating every layer of the coverings, and it has often struck me why they do not demonstrate a greater number, because it is perfectly easy to do so. There is no practical use in trying to differentiate the various coverings, because you cannot by any means recognise the sac by that. You really recognise it by the fact that a little fluid escapes when it is opened; that is the practical way.

Next we come to the ætiology of hernia. Herniæ are either congenital or acquired. By congenital hernia we do not necessarily mean that the hernia was present at birth (indeed, most are not), but that some congenital condition favouring hernia persists after birth. For instance, this boy, who has double congenital inguinal hernia, says he had it when he was very young, but was not born with it. Although a young person may have the conditions favouring the development of hernia, it may never occur, or may not do so until the patient has perhaps reached adult life, and then it may quite suddenly make its appearance in the already defective spot.

With regard to the acquired form of hernia, you will find that the causation of it is by no means as simple as may at first sight appear. Mr. Lockwood some time ago took a great deal of trouble to work at the condition of the mesentery in cases of hernia, and he found, which any of you can certify for himself, that if the mesentery is normally attached (*i.e.* from the level of the second to the lower border of the fifth lumbar vertebra), it is quite impossible, when the belly is opened, to pull the coil of intestine below Poupart's ligament. Therefore it is proof positive that in cases of hernia the mesentery must be either abnormally elongated, or it must be prolapsed so as to let the gut come out. Nobody denies that in a case of hernia the mesentery is lengthened, that is quite certain; but at the same time that does not by any means prove that the formation of that hernia was primarily due to such elongation, because if the mesentery is of normal length, and it gets into a hernia, the constant traction on it as the hernia lies outside the abdomen would gradually tend to stretch it. There is no doubt that the initial condition is prolapse of the mesentery,—that is to say, that the mesentery is attached lower down than normally. This enables the coil of gut to slip outside the belly, and the constant traction then tends to increase the length of the mesentery, and to increase the prolapse as a secondary consequence. In some cases of very large hernia you may find that the whole mesentery and intestine is outside the belly and in the scrotum. That, of course, is a very marked prolapse of the mesentery. But even given the fact of prolapse of the mesentery, and given the fact of elongation of the mesentery, it does not by any means follow that you are bound to get hernia; it is quite clear there must also be some weakness of the abdominal wall. If the abdominal wall were perfectly strong, you might have the mesentery as much prolapsed and as long as you chose, but still it would remain in the belly.

The causes of weakness of the abdominal wall I have already mentioned.

With regard to age, you will find the two extremes of life are those in which hernia is most common. In the first place, young people are subjected to hernia because the congenital conditions producing or favouring congenital forms are present in them. Old people get hernia because

their abdominal walls are likely to become more or less lax and pendulous, and the mesentery and the abdomen generally become full of fat, so that the intra-abdominal tension is increased. In the case of women added conditions also play a part, and in them ventral hernia is due to the fact of repeated pregnancies, and yielding of the linea alba.

Next with regard to sex. You will find that hernia is about five times more common in men than in women. This is due to the fact that inguinal hernia is much more common than any other form; in fact, it is more common than all the other forms put together, and inguinal hernia is (for obvious reasons) seven times more common in men than in women, whereas femoral hernia is rather more common in women than it is in men. Umbilical hernia (except the congenital form) is almost confined to women, as is ventral hernia; but at the same time the proportion of these forms to the aggregate number of herniæ is so small that the proportion of five men to one woman is about correct.

My next point refers to the general pathology of hernia; it may be reducible, irreducible, or strangulated. The irreducible form may also become incarcerated or inflamed.

First of all we will take the reducible hernia. We find that nearly all herniæ remain reducible throughout, and this is especially the case nowadays, when the safety with which radical cure can be performed has naturally diminished in numbers cases of irreducible and strangulated hernia. When reducible hernia first develops it merely causes a bulging at the spot where it comes through the abdominal wall, and is only noticeable if the patient stands up, or during some forced expiratory act; if you put the patient upon his back probably the whole thing will disappear, and the part resumes its normal outline. But as the hernia begins to increase in size, when it escapes from the surrounding tendinous structures and gets into the scrotum it assumes its characteristic shape. It will, perhaps, be most convenient to describe the symptoms as you meet with them in an inguinal hernia, that being the most common form. You find a tumour that is pyriform in shape with the base downwards, and the apex extending up to and involving the external abdominal ring. Remember that that is the important thing in

examining a supposed case of hernia. First, you must put your hand close to the abdominal ring, and see if it is free. If it is free, and the tumour is below, it cannot possibly be a hernia; it must be something in association with the testicle or tunica vaginalis, or possibly in connection with the scrotum itself. The tumour is generally soft, and the percussion note is usually dull, but it may be distinctly tympanitic. There is usually practically no pain, and all that the patient complains of in connection with the hernia is that if it is a large one there is a little dragging about the groin and round the loin. Pain is quite rare. There is a distinct impulse on coughing, which is perfectly characteristic of hernia. It is quite true that if you put your hand against the abdominal wall, and cough in the ordinary way, you will get an impulse. But the impulse of hernia is something entirely different. Occasionally, if the hernia is very large, the patient complains of dyspeptic symptoms and more or less trouble with his bowels. If the hernia contains small intestine the trouble with the bowels is very slight, for the simple reason that, although the bowel is outside the abdomen, the contents, being fluid, are easily swept along. On the other hand, if the colon is prolapsed, then the trouble with the bowels may be marked, and it is exactly in these cases that trouble sometimes arises, leading, perhaps, to actual incarceration. The crucial test of the nature of a hernial swelling is its reduction by taxis. Occasionally, directly a man lies down upon his back, back goes the hernia without being touched; in other cases you have only to lift up the scrotum and you see the swelling gradually disappear. But usually the hernia requires more or less kneading and manipulation before you can get it back to the abdomen at all, and when it does go back it goes with a slip and a gurgle, but if it is omentum the gurgle is not heard. You may find it is impossible to completely reduce the hernia; for example, given omentum and bowel in the sac, you may get your bowel back but not your omentum. This is sometimes spoken of as a partly reducible hernia.

The treatment of reducible hernia resolves itself into either that by a truss or treatment by operation.

Now with regard to the truss. You may be pretty sure that a truss will not cure a hernia unless the patient be a child. In children you may

under certain conditions cure a hernia with a truss, but in an adult you can never do more than keep it up, and very often you cannot do even that. Therefore of late years, since the adoption of antiseptics, the radical cure has become more and more common, and it is a perfectly safe operation. It has been performed on quite little children, and, indeed, at every age. There is practically no risk in suitable cases, and no reason why any accident should occur during or after the operation. At the same time it is wrong even to infer that the operation should be done indiscriminately; I do not think that. I think there are certain cases where it is doubtful whether you should undertake the operation, and in others it is quite certain that you should not do so. In the case of little children who have intelligent parents, I think you should give them the chance of cure by a truss. The application of a truss to a child undoubtedly requires a certain amount of supervision and intelligence. But in hospital practice I do not think it is the least use to give patients a truss with the object of curing the hernia of young children. In the first place, the parents in the poorer classes do not take the pains to put the truss on properly. Again, the child is too often uncared for, and, moreover, the parents are frequently totally incapable of grasping the object in view, and the consequence is failure. I am strongly of opinion that a hernia should not be operated upon in very old patients unless there is some very definite cause for getting rid of it. You can usually easily keep the hernia up with a proper truss, and it does not appear to me to be quite fair to submit an old man to an operation if it can be avoided. In the case of young men I should urge an operation. The hernia is always a source of danger, and in addition it does to some extent handicap a man in the race of life. An operation becomes an absolute necessity for a man who wants to lead an active life; it is, of course, a necessity if he wishes to enter the Services, and I believe, but am not quite sure, that it is necessary for postmen.

In very large scrotal herniæ an operation should not be undertaken. In the first place, these are usually met with in old and very fat patients; and in the second, even if you try to replace the hernia by operation, failure to do so is likely, for the hernia having been left long unreduced, the abdomen has

become filled with fat, and there is no room for it to go back.

Now, with regard to trusses, there is no one truss that will fit all cases of hernia in the same region. In some cases a perfectly simple truss will keep up an inguinal hernia, while in others you may have to try half a dozen different forms before you get one which will act efficiently. It is necessary to give the patient certain directions when you supply him with a truss. In a case in which your object is to cure a hernia in a child you should give the patient's friends directions in writing, and these are the directions you should give:—(1) The truss should always be put on before the child gets up in the morning; (2) the attendant must be assured that the hernia is up; (3) the truss is only to be taken off when the child is lying down at night; (4) it is never to go on with the hernia down; (5) if the rupture comes down beneath the pad it must be returned and the truss re-applied; (6) if at any time the hernia has come down and cannot be got back again, the patient must be seen by a doctor without delay; (7) the bowels must be kept acting regularly. For the purposes of bathing and for night wear it is well to give these children a light truss of vulcanite or celluloid. In the case of infants, if you think it wise to supply them with a truss, have it made of vulcanite, because it can then be kept clean and dry, whereas a leather one soon gets foul with urine. You should also impress on the friends that on no account whatever are they to try to get a child to bring the hernia down, because if this occurs the treatment starts again exactly where it began. For instance, if treatment has been going on for six months, and the parents have not seen the hernia during that time, and they bring the child to see you, do not try to bring it down, for by so doing you would be putting the case six months back. If you are giving a truss with the simple object of keeping the hernia up, you will give the patient very much the same directions: he must wear his truss whenever he resumes the erect posture, never put it on with the hernia down, keep the bowels acting regularly, and if at any time the hernia cannot be got back he is to apply for relief. Those are the main points which you have to impress upon patients.

Next let me speak of irreducible hernia. A hernia may be partially or wholly irreducible. By partial irreducibility one means that only some

part of the contents can be pushed back, but that part can be pushed right back; it does not mean that the whole can be pushed partially back, say from the lower to the upper part of the sac.

The causes of irreducibility are various. It may be and usually is dependent on the presence of adhesions in the sac. Irreducibility may also be due to thickening of the neck of the sac, but not of sufficient degree to strangulate. Some herniæ are naturally irreducible because they possess no sac, and the protruded viscus is adherent to the surrounding structures. Sometimes irreducibility is dependent on the fact that the hernia has been down for a long time, the patient has neglected it, and fat has accumulated in the omentum or mesentery. There is a specimen of this in the museum, which I removed from a man twenty-eight years of age who was putting on fat very fast. He did not take any trouble with his hernia, which was irreducible. I removed three and a half pounds of fatty omentum from the sac, but there was not a single adhesion.

The symptoms of irreducibility are generally perfectly clear. A patient tells you he knows he has got a hernia, which he could at one time put back, but the difficulty of so doing became greater and greater, until at last it has become impossible. If you get a history like that the diagnosis is clear. But it occasionally happens that the patient is so foolish or ignorant that no reliable history can be obtained, and under such circumstances you have to make a diagnosis by the result of your own examination. You will find that there is a hernial sac of the shape I mentioned just now, that it extends up to the ring and into the canal; that there is a distinct impulse on coughing, and that the hernia may gurgle when squeezed. If it contains intestine this may be reducible. With regard to the percussion note, that varies; it may be dull or it may be tympanitic. You will find also that straining on the part of the patient may sometimes render the hernia rather more tense.

The treatment of irreducible hernia must depend upon circumstances. In very old people, and in the obese with very large herniæ, no operation should be undertaken; but the patient should be very carefully dieted, and warned of the possible occurrence of incarceration, the nature of which had better be explained to him. The

hernia must be supported and protected by an accurately moulded and fitted truss. If you have a youngish patient and the hernia is not very large, your best plan is to do a radical cure, and in doing this you must be very careful how you go to work, because if your hernia is rendered irreducible by adhesions, when you cut down upon the sac you may divide with it the adhering bowel, an accident which will gravely complicate matters and may endanger the patient's life. Therefore be careful in exploring the interior of the sac. If irreducibility is dependent on narrowing at the neck, the plan is to divide it sufficiently and push the intestine back; if it is due to the presence of adhesions they must be dissected off; if to large masses of fat in the omentum this must be cut away. The treatment of adhesions is sometimes difficult. You may find the adhesions are merely long bridles which are easily divided after double ligature; or they may be very broad, and intimately unite the surfaces. If you find that it is better not to attempt to separate the gut, but simply to cut out the piece of sac which is left adherent to the intestine, and push the whole thing back together, that is by far the safest method, and has the advantage of being quickly performed. As soon as the hernia has been completely returned the operation is finished by a radical cure.

When the intestine becomes distended, either by gas, fæces, or undigested food, so that its lumen is blocked, it is said to be incarcerated. Incarcerated hernia is much more commonly met with in women who are the subjects of umbilical hernia, and such herniæ nearly always contain the transverse colon. If you have the small intestine in a hernia it is not likely to be incarcerated, for the simple reason that the contents of the small intestine are fluid. But where you get the colon with its solid contents, incarceration is very likely to occur.

You will very often find that the determining cause of incarceration is a mass of undigested material following a heavy meal. Unfortunately the very women who are most likely to get incarcerated hernia from being fat, and having a large umbilical hernia, are the very people who indulge too freely at the dinner-table, and are often chronically constipated; they try in every possible way to produce incarceration. Incarceration itself is not dangerous, but it may be mistaken

for strangulation. A patient tells you that she is suffering considerable colicky pains in the abdomen, and that the hernia has suddenly become tense, and the bowels for some time may have been giving considerable trouble. You will find on palpation that the tumour has got a feeble impulse, but nothing like that which you usually associate with simple hernia; it is tender and swollen, and it may be tympanitic if it is full of gas. In other cases you find that instead of being tympanitic it is dull, and may be easily moulded on pressure, showing that the gut is distended with fæces. The patient is not collapsed; she may of course feel ill, and is troubled with nausea and vomiting, but she has not got the typical symptoms of acute intestinal obstruction. She is probably anxious and apprehensive, but otherwise there is nothing very noticeable about her general condition. Very often you will find that she is passing fluid motions, and perhaps mucus, because there is a certain amount of intestinal irritation as well as incarceration of the hernia. She probably has a feeling of nausea, and perhaps actual vomiting, but the vomit never becomes stercoraceous; the last is a very strong diagnostic point.

With regard to the treatment, that is simple enough. All you have to do is to get the intestine to empty itself. Now, supposing that you are entirely convinced that strangulation is not in the case, you may perfectly well give the patient a purge by the mouth; but if you have the slightest doubt about it you ought not to do any such thing. You may give in addition, or separately, a large enema of olive oil, or one of glycerine. Manipulation of the tumour sometimes cuts short the condition almost at once; by so doing you may free one loop of intestine that has come down in the neck of the sac, and so give plenty of room for the fæces to pass on; or you may push on an obstructing scybalous mass, and so re-establish the passage. After the patient has got over the incarceration it is extremely important that you should carefully regulate the diet and bowels, and I should recommend you in all cases to write your instructions out. The instructions should be to live on very easily digested food, and avoid all things like vegetables, new bread, nuts, and similar indigestible things. Heavy meals must be interdicted.

With regard to the local treatment of the hernia

itself, it may be advisable in some cases to do a radical cure, but unfortunately these patients are the very ones in whom the radical cure does not prove very inviting; they are too fat, they are too old, and the hernia is usually much too large, and in other ways they are not suitable patients for surgical operation. In a large irreducible hernia which has become incarcerated, and a radical cure seems to be out of the question, the best way is to apply a bag truss; this is made by taking an accurate cast of the hernia, on which a metal pad can be made so that it accurately fits the hernia. This keeps up pretty considerable pressure on the hernia, and according to some, continued pressure on an irreducible hernia will eventually make it reducible. I have seen it tried a good many times, and I must say that I have never seen it succeed. A truss such as this has this advantage; it prevents the hernia getting any larger, and protects from injury.

There is still another condition of irreducible hernia, and that is the inflamed form. When we speak of inflamed hernia we mean that there is local peritonitis. That may be dependent upon the irritation of an ill-fitting truss, on injurious manipulation, or perhaps upon direct injury. It is very rarely a serious condition, although cases are occasionally met with in which the peritonitis extends to the general peritoneal cavity. Of course if the peritonitis is dependent upon disease of the bowel itself, as might be the case in typhoid fever, for example, the condition may then be very serious. As a rule the inflammation clears up, and there are no other ill effects than resulting adhesions, which will render the hernia still more difficult to reduce.

The symptoms of inflamed irreducible hernia are very similar to those of incarceration, but you find that the pain is usually more superficial, that the tumour is probably not so tense, nor is it so tender. The patient very likely feels ill, and the temperature is perhaps 100° or 101° , or even 102° , but the absence of fever makes it by no means certain that inflammation is not present, because we know that even wide-spread peritonitis may be associated with a normal or subnormal temperature. The absence of signs of strangulation and of vomiting are of course important.

The treatment of inflamed hernia is practically that of peritonitis. You should put the patient to

bed, keep him perfectly quiet, and give him a saline purge or open his bowels by an enema. With regard to food, milk and beef tea and similar articles of liquid food should be given. Locally you may apply the ice-bag, or, if they are more grateful, hot fomentations. When the inflammation has subsided, the question will have to be considered as to whether a radical cure shall or shall not be performed, and this must be determined on the lines I have already indicated in speaking of irreducible hernia. I hope to discuss the subject of strangulated hernia with intestinal obstruction generally at some future time.

Indicanuria.—Dr. Charles E. Simon, in the 'American Journal of the Medical Sciences,' concludes as follows: (1) The gastric juice possesses antiseptic and germicidal properties. (2) These properties are referable to the presence of free hydrochloric acid. (3) A subnormal amount of free hydrochloric acid will call forth an increased degree of intestinal putrefaction. (4) The conjugate sulphates form an index of the degree of intestinal putrefaction. (5) The increased intestinal putrefaction in cases of subacidity and anacidity of the gastric juice is largely referable to an increased formation of indol. (6) The elimination of indican in the urine may be regarded as an index of the amount of free hydrochloric acid present. (7) A normal acidity of the gastric juice is never associated with increased indicanuria. (8) Cases of ulcer of the stomach apparently form an exception to this rule, an increased indicanuria being usually associated with hyperchlorhydria. (9) In other cases of hyperchlorhydria a subnormal or normal amount of indican is eliminated. (10) Simple constipation is rarely accompanied by an increased elimination of indican. (11) Diarrhoea referable to a catarrhal condition of the colon, often following a previously existing coprostasis, as well as diseases of the colon in general, is not associated with an increased indicanuria. (12) In the differential diagnosis between ileus and coprostasis, a small amount of indican excludes the former condition. (13) In cases of an achlorhydria with much lactic acid the indican is not necessarily increased.—*Medical Record.*

CHAPTERS FROM THE TEACHING OF DR. G. V. POORE.

No. XI.

GENTLEMEN,—To-day I begin to deal with poisons *seriatim*, and the first class I shall speak of are irritant poisons. The most irritant poisons are the corrosive poisons, and the most corrosive poisons are the mineral acids—sulphuric acid, nitric acid, hydrochloric acid. Now these have been used for poisoning accidentally and intentionally again and again. That which has been most frequently used is sulphuric acid or oil of vitriol. I may remind you that this acid is used a good deal for manufacturing purposes. I need not go through all the uses to which this acid is put, but we must remember that it can be easily obtained. An acquaintance with the fluid will remind you that it may be mistaken for other oily or syrupy fluids; and sometimes oil of vitriol has been taken by a drunkard in mistake for something different. It would be difficult to murder a man with these strong acids unless he were insensible at the time. Of course it is only the strong acids we deal with. Strong sulphuric acid combines with water with great energy, as you know, and generates a good deal of heat as it does so. It has the faculty of charring all kinds of tissues; all tissues containing carbon are blackened by strong sulphuric acid. When a dose of strong sulphuric acid is taken the first symptom is intense pain in the throat and gullet, and a little later a pain referred to the stomach. Apparently the pain is of an awful burning character, which burning is not only due to the action of the acid, but to the vigorous combination with water which takes place, and to the heat which is generated on that account. When sulphuric acid is swallowed it occasionally has happened that the suffering is not so immediate as you might expect. I think that holds good with all these very strong acids; and the reason is that the mucous membrane can, when the need arises, secrete mucus with most wonderful rapidity. You know that if something gets into your eye the eye waters copiously and immediately, and washes it out. Also if something is irritating your throat, almost immediately there is an accumulation of

mucus, and you cough the substance up. So it is in the stomach; when these substances are taken into the stomach, amongst the vomited matter is a large amount of stringy mucus. The power which the mucous membranes have of protecting themselves is very remarkable. A slug is one of those creatures which wears its mucous membrane outside, and those of you who have watched a gardener killing slugs with quicklime will have noticed that one sprinkling with the quicklime is not enough, because the mucus secreted throws off the lime and the slug escapes; but it cannot do that twice. So it is with the strong acids in the stomach; sometimes a quick secretion in the stomach will protect the organ in a way that is simply marvellous. Next there is vomiting, and the vomited material consists of the contents of the stomach and mucus. The vomited matter is intensely acid, and if the patient vomits on a limestone pavement effervescence will take place. That is important, because limestone pavements are ubiquitous, and effervescence under such circumstances will give you a clue instantaneously. Now with regard to the pain, that does not always seem to be instantaneous or anything like it; and there is a case on record of a suicide taking a dose of oil of vitriol in the street, tossing his head back during the process, just as you may have seen people do when they take a dose of castor oil, and hailing a cab, getting into it, and telling the man to drive him to the nearest hospital. That will probably enable you to remember that the symptoms are not always instantaneous, and for the reason which I have given you.

Soon after the poison has been taken the patient vomits and is collapsed. What I understand by being collapsed is that he is pale, has an exceedingly weak pulse, and that he can hardly do anything. He is not insensible, and I hope you have all got a clear distinction in your minds between a state of collapse and a condition of coma. Collapse is the condition to which a patient is brought by a severe injury; coma is the condition to which he is reduced by something which poisons the brain, or injures it so as to cause insensibility. Usually when sulphuric acid is taken there is constipation, but it has been known to cause excessive action of the bowels. Usually there is diminution of urine, and the urine has often been albuminous. The patient is very apt to get some of the corrosive upon

his lips, leaving marks upon them, and he is likely to get some corrosive fluid on his clothing, leaving red marks or rotten holes. Then the person who takes a corrosive may injure his mouth or tongue. Then as the fluid goes backwards he may get his glottis and his epiglottis attacked, in which case there is swelling of those parts, and you may get as a consequence occlusion of the glottic aperture, and death from asphyxia. It is very important to remember that. If any of the corrosive fluid goes down the windpipe, you not only get a whispering voice from the state of collapse, but you get in addition aphonia from the injury to the vocal cords and the larynx itself. After the extreme stage of collapse a reaction may set in, and then you get rise of temperature and fever, and the spots of corrosion on the mucous membrane begin to swell.

There are exceptional cases on record where casts of the intestine have been passed in sulphuric acid poisoning. That is not so common in sulphuric acid poisoning as it is in nitric acid poisoning. After all these things there may be nervous symptoms. A man may be convulsed, and the convulsions will probably occur just in proportion to the youth of the subject. It is said that you may get all sorts of nervous troubles if the patient lives, sensory troubles among others; and whether they are reflex or not it is difficult to say.

Post mortem, as a rule, you get dirty yellowish-brown stainings about the angles of the mouth, and you often get similar spots on the tongue and gullet. The stomach very often contains a black tarry fluid. Now you might be ready to assume that as sulphuric acid chars very markedly, the effect is always to produce blackness and charring. That is not the case. I have here some specimens of gullets, and I cannot tell from their appearance whether they are cases of sulphuric acid poisoning or not. I see by the catalogue that this comparatively light-coloured one is a case of sulphuric acid poisoning; I should have thought by the appearance of the gullet it would have been nitric acid. If you were shown these specimens, and asked to speak about them, you would be right in saying that the condition was due to a corrosive fluid, but you could not say much more with certainty. The specimen which is the deepest yellow is a case of poisoning by nitric acid. In the latter stages you may get stricture of the

oesophagus and cicatricial tissues formed wherever the sloughs have previously been.

As regards the treatment of cases of poisoning by sulphuric acid, you have to remember that the patient has taken a very corrosive fluid into the stomach, and it is exceedingly common in these cases to have perforation of the stomach, and that being the case you must not use the stomach-pump. What can you do for the patient? You must neutralise the acid. Here, however, you are in rather a difficulty, because if you give a carbonate you cause such a sudden and voluminous evolution of carbonic acid that you may burst the stomach. Therefore, if you can get them, you must use the alkalies, and not the carbonates of the alkalies. If your surgery is handy you will use a weak solution of potash, or calcined magnesia stirred up in a little water. Of course, if you have not alkaline earths handy, you can give carbonates and run your risk. These cases of mineral poisons are just those in which the classic direction to scrape the ceiling with a fire-shovel and give him plaster is applicable; you can get a little lime whitening off most walls. There are alkalies in most houses, and in the kitchen you will find carbonate of soda for cooking purposes or for washing purposes. If you cannot get anything of which you know the composition with any certainty, I take it you would be right in using baking powders, because I dare say most of them have alkalies in them. Then you may give oil or gruel, which text-books say does a certain amount of good. If the amount of acid taken has not been very great you probably would be right in giving water. Of course, if the amount of water is not enough to copiously dilute the acid, but only enough to generate heat, the effects would be very painful. Therefore you must give large quantities of water if you give any. In regard to poisons you are asked what is a fatal dose. The fatal dose of sulphuric acid depends very much on the degree of concentration. One is not prepared to say that a drop of sulphuric acid, if it gets on the right spot, would not burn a hole through the stomach or oesophagus and kill a man, but that is not very likely to be the case. A teaspoonful has undoubtedly killed a person, and people have recovered after taking larger quantities. Then with regard to the fatal period, that is very uncertain; indeed, to expect a definite answer as to the fatal

dose and the fatal period is somewhat nonsensical. The fatal period depends upon the dose. Usually it is thought a man lives about ten hours after a draught of sulphuric acid, but I do not see what good a statement of that kind is. That the mortality is very great indeed need hardly be said.

There are certain points about sulphuric acid which are of interest. First of all there is the heat which it produces. It has been attested that sulphuric acid was put into cold water in mistake for whisky. But the generation of heat is so tremendous that it would be detected. Again, the charring is an important matter, and there is a case on record of a child being killed with oil of vitriol, and the question was whether it was a case of murder or of accident. The defence was that the oil of vitriol was given instead of aniseed; that the woman put a lump of sugar in a teacup and poured accidentally oil of vitriol upon it instead of the preparation of aniseed. In answer to that it was stated that this was impossible, because the strong sulphuric acid would have blackened the sugar immediately, and she must have seen it turn black. I have here a piece of ordinary loaf sugar, and I will pour a little oil of vitriol upon it, and I ask you to note the time which the change occupies. You see it is an appreciable time before the sugar turns brown, and that it is some minutes before it turns black. Therefore it is possible for the liquid to have dissolved the sugar before anything in the nature of a blackening was noticed.

Before I take the chemistry of these substances I will go to the next mineral acid—nitric acid. *Aqua fortis*, red spirit of nitre, or crude nitric acid, differs from sulphuric acid in that it fumes. The nitrous fumes which are given off by nitric acid are very important. If you take a strong dose of nitric acid into the stomach the symptoms are practically the same as when you take a strong dose of sulphuric acid, so that I shall not take the trouble to again describe to you the vomiting and the pain and the collapse. But there are certain differences with nitric acid which it is well for you to remember. First of all there is the fuming quality of nitric acid, and the action of these fumes is exceedingly insidious. It is a peculiarity of nitric acid that if people inhale these fumes they die from a form of bronchitis, and usually the end comes in twenty-four to thirty-six hours. You will

remember that it is very dangerous to be exposed to the fumes of strong nitric acid. There is a case on record in which a Scotch Professor of Chemistry—Mr. Stewart—and his janitor, as they call the porters in Scotland, broke a large carboy of nitric acid, and instead of leaving the room they stopped in it, and tried to mop up and collect the acid. They thus inhaled the fumes, which were immediately diffused. Mr. Stewart returned home, unconscious of the mischief which had been done. After an hour or two difficulty of breathing came on, and in spite of every medical effort to save his life he died ten hours after the accident. His janitor suffered from similar symptoms, and died the day following. Then I find another case. Four persons exposed to the fumes while trying to remove a broken carboy of acid. No. 1 was laid up with pneumonia, Nos. 2 and 3 had bronchial trouble. No. 4 after the exposure did not feel right; five and a half hours afterwards he had cough and pain, the pulse failed, and he died thirty hours after the accident. Post mortem there were catarrh of the larynx, trachea, and bronchi, oedema of the lungs, and ecchymosis of the heart. The post-mortem signs were those of asphyxia. It is very important to remember that the onset of the symptoms in nitric acid poisoning may be insidious. Again, as we all know, nitric acid stains the skin yellow. It will also stain mucous membrane yellow. Another property which it has is that of coagulating albumen. It is one of the tests for albumen in urine. Owing to nitric acid coagulating albumen, it makes the tissues which it touches hard and firm. Therefore perforation of the stomach, as a result of swallowing nitric acid, is not so common as with sulphuric acid. In the case of nitric acid poisoning the tissues are first of all coagulated, and then, of course, necrosis takes place and a slow process of sloughing. Indeed, in some of these cases of nitric acid poisoning which have lived some considerable time it has been found that, instead of the stomach, there is a cavity of the shape of the stomach, formed of inflammatory exudation and adhesions of the neighbouring viscera and cellular tissue. There is a case described by Puchelt, of Heidelberg, in which a man swallowed nitric acid, and died on the twenty-third day. The acid was not very strong. It was only on the fifteenth day that he began to vomit, and on the seventeenth day he vomited a mem-

brane, which, when spread out, was a foot in breadth; that is to say, he had vomited the whole of the coats of the stomach, which had been first coagulated and then separated from the living tissue beneath.

I remember making a post-mortem examination on a case of nitric acid poisoning, and in doing so and opening the gullet the first thing that struck us was that the muscles around the top of the pharynx and at the back of the windpipe were exposed, and we could plainly see the posterior crico-arytænoids and the constrictors; they were laid bare as though they were a dissecting-room preparation. The mucous membrane was gone, and when we opened the stomach we found in it a perfect cast of the gullet and the back part of the glottis. It had got loosened, and had subsided into the stomach. That might have been vomited had the patient lived. In the same way casts of the bowels have been passed.

With regard to the fatal dose of nitric acid, again we must say that it depends on the strength of the acid; it depends also upon the extent to which the patient has been exposed to the fumes. It is not so dangerous as sulphuric acid, and two drachms seems to be the smallest dose which has certainly killed. In one of these cases death resulted in one and a half hours. The treatment is practically the same as for sulphuric acid.

The next acid I have to deal with is hydrochloric. That is an uncommon poison. Again I would remind you that hydrochloric is an acid which is found habitually free in the contents of the healthy stomach. That is an important matter. The fumes of hydrochloric acid are particularly fatal to plant life, and the fumes of hydrochloric acid may cause more or less inconvenience, just as the fumes of nitric acid do, but to nothing like the same extent. Post mortem there are signs of corrosion, and perforation is not very common. It does not seem to be so dangerous as the other two. A case is recorded from King's College Hospital in which half an ounce killed a man in eighteen hours.

I think I may also mention hydrofluoric acid, but I can only find the record of one case of poisoning by it.

This is reported by Dr. Robert King in the twenty-fourth volume of the 'Pathological Transactions' (1873). The patient, accustomed to the use of hydrofluoric acid for etching glass, was a

drunkard aged forty-six. He drank about half an ounce of the poison from a gutta-percha bottle. He died in thirty-five minutes in agony with retching, cold clammy perspiration, pulse small and rapid, pupils contracted, inability to swallow. It was noticed that breathing continued after the radial pulse and heart had apparently failed.

Post-mortem.—Mucous membrane of mouth white and softened, and denuded in parts of epithelium, which hung in dirty shreds. The stomach contained a large quantity of thick blackened material resembling treacle; when this was removed and the stomach washed, a reticulated surface was seen, the branches of which were perfectly black and slightly raised, the intermediate sulci being highly injected and speckled with minute ecchymosis. There was no perforation.

Thyroid Gland in Hæmophilia.—Dr. Combe-marle ('*Médecine Moderne*,' April, 1898) at the recent Congress of Internal Medicine in France, reported the case of a thirty-eight-year-old woman in whom the least cut gave rise to severe hæmorrhage. She suffered, moreover, from enteric, uterine, laryngeal, and nasal hæmorrhage at irregular intervals. She frequently had hæmorrhages into the skin and from the gums. In ten days after treatment with thyroid tablets there was very decided improvement. The purpuric spots disappeared, the gums became firm and ceased to bleed, and there was marked constitutional improvement. Ten days later her menses became normal, and all other hæmorrhages ceased.

Medicine, October, 1898.

Tracheotomy under Local Anæsthesia.—B. Franckel ('*Berl. klin. Woch.*,' June, 1898) has performed twenty-three tracheotomies under cocaine during the past three years. Many of these have been performed under circumstances in which it would have been dangerous to have given a general anæsthetic. His plan has been to inject hypodermically a 20 per cent. solution in two places near the site of operation, or a 10 per cent. solution in four places. In children the 10 per cent. solution was always used. In adults the amount injected was .04 grm. of cocaine.

Patients dreaded the cocaine less than the chloroform. One of the advantages of operation under local anæsthesia is the removal of all necessity for undue haste.

• *Canadian Practitioner*, September, 1898.

CLINICAL LECTURES ON MUSCULAR TREMOR AND CLONIC SPASM.

Delivered at the Hospital for Diseases of the Nervous
System, Welbeck Street, by
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LECTURE III.

Tremors.

GENTLEMEN,—At our last two meetings we were considering the subject of intermittent muscular spasm, and you will remember that for clinical purposes we divided these intermittent muscular movements into clonic spasms or movements of large range, and tremors or oscillations of small range. It is the latter which still remain for us to study this afternoon.

You will have noticed already amongst the clonic spasms that this large and interesting group of maladies are very largely represented in the so-called functional (or constitutional) disorders,—that is, diseases not yet associated with definite organic lesions. We shall find, gentlemen, that the same remark applies to the tremblings and small movements which will occupy our attention to-day. Indeed, if you will look at the list of diseases which I have written on the black-board (see list annexed), you will see that if we except disseminated sclerosis, general paralysis of the insane, and some of those diseases in which the trembling is a subordinate feature (group 10), none of those are definitely associated with an organic lesion of the nervous system. They are all manifestations either of some functional derangement, or some constitutional, blood, or toxic condition.

List of diseases giving rise to TREMORS or movements of small range, of which Paralysis Agitans may be regarded as the type :

1. Paralysis agitans.
2. Senile tremor.
3. Disseminated sclerosis.
4. General paralysis of the insane.
5. Hysterical tremor.
6. Alcoholic tremor.

7. Metallic tremor.
8. Other toxic conditions.
9. Nervous or neurasthenic tremor.
10. Other diseases in which tremor is not always a prominent feature, viz. :

- (1) Tumour of the brain under certain conditions.
- (2) Lateral sclerosis.
- (3) Friedreich's hereditary ataxy.
- (4) Amyotrophic paralysis.
- (5) Graves' disease.

It is unnecessary for me to repeat the clinical features of such cases which require investigation for purposes of differentiation. They are the same as in cases of clonic spasm.* Permit me only to remind you that the smaller movements we are about to consider differ from the group of clonic spasms not only in their size, but also in the fact that the oscillations are in a good many instances of a more *regular* rhythm. Moreover, being of smaller range, they are, as you would expect, generally more rapid, though in neither of these respects does it do to draw a hard and fast line. I should like in passing to draw your attention to this question of *rapidity*. If you observe a patient when he is under usual or normal conditions—when he or she has got over the initial nervousness which troubles him on appearing before “the doctor”—you will generally find that the rate of the oscillations in paralysis agitans, senile tremor, and disseminated sclerosis are slow when compared with those of alcoholic tremor, metallic tremor, general paralysis, and Graves' disease.

If the tremors be recorded on a drum—which can easily be done by causing the patient to hold a closed caoutchouc bag connected with a steel box like that of an aneroid—it will be found that whereas the rate of the first three is something like 200 or 300 beats per minute, the rapidity of the second group amounts to some 500, or 600 per minute.†

Among the cases of tremor which I wish to show you to-day it will be advisable that we should first consider Paralysis Agitans, which is the type of the whole class.

1. *Paralysis Agitans* is certainly far the

* Vide ‘Clinical Journal,’ Oct. 19th, 1898.

† Charcot, ‘Leçons cliniques des Maladies du Système nerveux,’ tome iii.

commonest cause of tremor in persons of advanced life. Another name for this disease is Parkinson's disease, but I only learnt this when I was studying in Paris. In France it is very generally called by this name even in the present day, and it was in France, gentlemen, where I learnt that this disease was first described by, and named after, a countryman of my own nearly a century ago : * another illustration that a prophet is without honour in his own country.

The first patient I have to show you exhibits very well the three cardinal symptoms of the disorder—the tremor, the festination, and the characteristic age, for it is practically unknown before fifty. He is sixty-five years old, and for seven years he has been affected with a tremor of the limbs such as you now see. There are about 200 oscillations to the minute, they are rhythmical, continuous, and they do not cease when the limb is supported, that is to say in the absence of voluntary muscular action. It is general in its distribution, but it does not affect the head and neck. Secondly, you will notice that he bends forward while walking ; and he complains that he has a feeling as of falling forward. His speech is slow and indistinct, and his expression is fixed and placid.

In the later stages of the disease the tremor tends to disappear, and to be replaced by a gradually increasing weakness. I want you particularly to bear this feature in mind because it is one of the clinical data on which I base my view of the pathology of these cases, viz. that the tremor in paralysis agitans is the result of a very slowly progressive degeneration (probably of a senile nature) of the nervous system ; and when this degeneration becomes more complete it gives rise to paralysis.† The restlessness and strange sensory disturbances which trouble some of these patients in the later stages are an evidence that the sensory tract is also involved in this widespread degeneration. I had a great many cases of paralysis agitans under my care at the Paddington Infirmary, and I remember quite well one patient who was continually calling out "nurse" all day long, because no sooner was he placed in one

position than he wished it to be changed. The final stage, when all signs of tremor have disappeared, consists of rigidity and contracture such as usually supervenes in all paralysed muscles.

These later stages are only to be seen in a workhouse infirmary, and that, gentlemen, I venture to think is the reason why such erroneous views as to the pathology of paralysis agitans have been suggested. But the long clinical history of these cases is only consistent with the view I have just mentioned, viz. a slow, scattered, and partial degeneration of the nerve structures throughout the body. Just as we find in one person the circulatory system first shows signs of decay, so in another it is the nervous system, and the partial degeneration of the motor tract and peripheral nerves is manifested by tremor.

Here is another case, a woman named E. B—, æt. 61, who has been affected with the tremors for four years. In her the head and neck are tremulous, but the other characteristics of the case are quite sufficient to place it in this group. The third case is that of a man named E. F—, æt. 60. In him the weakness—which began seven years ago, after an attack of influenza—has been a prominent feature throughout. All three of these cases are quite in the early stage, for the disease runs a prolonged course of many years, and the patient generally dies of some intercurrent malady.

The first two cases have derived great benefit from tincture of hyoscyamus, twenty drops thrice daily, and now and then hyoscin hydriodate one hundredth grain hypodermically. The last case has certainly improved under strychnine. This is a remedy which, as a rule, is contra-indicated in all tremors and spasms, but it is *par excellence* the tonic of old age, and as the weakness was the most marked feature in his case I prescribed it for him ; and, as the event has proved, with advantage. You will observe I have shown you two men and one woman ; and, as a rule, this is about the proportion in which the two sexes are affected.

2. *Senile tremor*.—Senile tremor is really part of the mumbling, fumbling, and tumbling condition of old age ; but when the tremor amounts to a definite disorder I must confess that I have not always been able to satisfy myself of the differences between it and paralysis agitans. There are, however, two features which are said to aid in this

* 'Essay on the Shaking Palsy,' Parkinson, London, 1817.

† Dr. Maguire and I have found incipient degeneration of the nerve-trunks in these cases.

differentiation: first, the head and neck in senile tremor are the parts that are specially involved, whereas these parts are generally—though not always—exempt in cases of Parkinson's disease; secondly, the paresis and rigidity of paralysis agitans are said to be usually absent from the later stages of senile tremor, but in my experience this is merely a question of time. The concluding stages of both of these diseases are rarely, if ever, seen in hospital; to study them we must go to a workhouse infirmary.

3. *Disseminated sclerosis*.—The anatomical lesion of this disease was discovered by Cruveilhier many years ago, but the clinical features of the disorder were not satisfactorily described and coupled with the anatomical lesion until Charcot described the disease and established its association with the changes described by Cruveilhier. It is in connection with this disease that the term "intention or voluntary tremor" has most meaning, because this disease is typically an "intention tremor."* It differs diametrically from paralysis agitans in this respect. The tremor is generally of rather larger range; it may be considerably larger, though, like paralysis agitans, it affects all parts of the body, including the head and neck. In the early stages, and for a very long time, the tremor is only present during voluntary muscular action. Here is a patient, a man named P—, forty-one years of age, who has been affected with a tremor for nine months, and you will see that there is not a vestige of movement when he is at rest. The movements start, however, the moment he grasps the spoon, and as he raises it to his lips the tremor gradually increases. But the spoon nevertheless reaches its goal in a manner quite different from that noticed in the clonic spasms of chorea. You will notice also that he has the characteristic expressionless look in his face, and his speech is syllabic. Again, he has well-marked nystagmus, and he has also had attacks of vertigo—a symptom which is complained of by about 75 per cent. of patients. He is a trifle older than these patients usually are, for the disease mostly commences under thirty or thirty-five. Here, for example, is a boy of only sixteen years old, who presents all the symptoms of the disorder.

The prognosis in both of these cases is grave,

but for different reasons. In the case of the man the sclerosis is very extensively scattered throughout the cord, for, amongst other reasons, his knee-jerks are absent. This is always an untoward circumstance in cases of disseminated sclerosis, for it almost invariably indicates involvement of the grey matter of the cord. The prognosis in the case of the boy is unfavourable, partly because the disease has made rapid progress in a comparatively short time, and partly because the brain as well as the spinal cord is evidently involved. He has lately become dull, stupid, obstinate, and bad-tempered, and it will shortly become a question whether they will be able to manage him at home. Both of them are taking iodide and bromide with but very little benefit so far. This, I am bound to admit, is a very hopeless disease.

4. *General paralysis of the insane* is characterised, as you are aware, by three groups of symptoms. First, the general weakness; and secondly, a mental disturbance, which usually takes the form either of a melancholic tendency or a grandiose mania in which the patient thinks, for instance, that he is wealthier or stronger than all other men. Thirdly, there is usually present a very fine rhythmical tremor, affecting chiefly, and sometimes only, the lips and the tongue. In point of fact this is a more constant symptom than weakness or paralysis. The patient who is now coming in, a man named H—, is thirty-five years of age, a clerk. He contracted syphilis at the age of twenty, and eighteen months ago began to become nervous and excitable. Six months ago he is said to have had an attack of "influenza," and ever since then has been excitable, irritable, and forgetful. The tremors which you will observe in the tongue, lips, and hands first appeared one month ago. I should like you particularly to notice the aspect of this young man, for it is clearly indicative of "cortical irritation," a condition which it is not always easy to appreciate during a single short interview, but which is clearly written in this young man's face. The aspect of the patient in these circumstances is very characteristic, but it has not—so far as I am aware—been described. Anxiety is depicted on his face, and you will notice how he looks furtively from side to side, and that he is constantly on the move. The appearance cannot well be described in words, but it is nevertheless readily

* *Vide* 'Clinical Journal,' Oct. 19th, 1898.

recognisable and not easily forgotten. He has several other symptoms of general paralysis of the insane, upon which there is no time for us to dwell—pupillary, mental, and other symptoms; and there is general weakness, though no actual paralysis anywhere. He is improving slowly under iodide, but his prospect of recovery is not great.

5. Now that we have considered three very well-marked types of tremor, we may turn to *hysterical tremor*. Hysterical subjects are liable to very many different kinds of intermittent muscular contractions. So many and varied are these that Charcot attempted to classify them in a tabular fashion.* You will, perhaps, think it very pre-

Hysterical tremors	A. Tremors not exaggerated by voluntary movements. Constant.	1. Oscillating tremor, slow —3 to 6 per sec.	Imitates paralysis agitans, or senile tremor.
		2. Vibratory tremor, quick, 8, 9, or more per sec.	Imitates Basedow's disease, alcoholic or general paralytic tremor.
		3. Intentional tremor, number of vibrations intermediate between 1 and 2.	Imitates sclerose en plaques, and still more perfectly mercurial tremor.
	B. Tremors existing, or not, in rest, set up or exaggerated by voluntary movements, which do not quicken it, but increase the amplitude of the vibrations.		

sumptuous of me, but I cannot say I have been able to identify all the three classes which he makes, and I think that they may be conveniently classified into small movements or tremors, and large movements or clonic spasms. The latter were fully discussed on a former occasion;† it is the small tremulous oscillations with which we are now concerned. Here is a young woman of twenty-seven years of age, who has been under my care for various kinds of hysterical manifestations, and who is at the present time afflicted with weakness and rigidity of the left side, and also with small, fine, very rapid, rhythmical tremors in this arm and leg. These symptoms have existed, though with variation in severity, for nearly three years. In this young woman they come on, you

will observe, only during muscular action, but this is not a constant feature. Authors variously describe these hysterical tremors as being regular and irregular. The fact is, as you can see in this case, there may be a regular and an irregular tremor in the same patient. Here there are small continuous regular tremors, and now and then irregular jerks. Another feature which enables us to identify this condition is that the advent is often sudden, and dates, as in this case, from some emotional shock, such as a disappointment in love or a fright. A third feature is that they vary considerably in severity from hour to hour, and day to day. These, combined with the age and sex of the patient, and the presence of the four hysterical stigmata, which I have referred to on former occasions,* generally enable one to arrive at a correct diagnosis.

Nevertheless, gentlemen, I am bound to admit that at times such cases present the utmost difficulty, and it is only by the nondescript character of the case that one is enabled to hazard a conclusion. Hysterical tremors, like the diseases which they imitate, may be partial, general, hemiplegic, paraplegic, or monoplegic. The hysterical stigmata may be absent; and men I have found are relatively more often affected with hysterical tremor than some other hysterical defects.

I remember one case of tremor in which a clue as to its origin was obtained from the fact that the shaking could be re-started by pressure in the inguinal region. Sometimes the diagnosis is aided by the abrupt cessation of the trembling without apparent cause. The duration of such cases is no guide; they may last for hours, weeks, months, or years. Charcot mentions a case which lasted "several years," and in the patient before you the tremor has existed for nearly three years with varying severity. The conditions which these tremors most resemble are paralysis agitans, disseminated sclerosis, alcoholic, general paralytic, and metallic tremors. Their treatment differs in no way from that of other hysterical affections, and I would once more remind you of the efficacy of isolation, as in the case of Penelope G—, whom I showed you last lecture.

6. *Alcoholic tremor*.—Here is a patient named L—, æt. 40, who is a commercial traveller in the

* Prof. T. M. Charcot's classification of hysterical tremors, slightly modified ('*Maladies du Système Nerveux*', tome i).

† '*Clinical Journal*,' Oct. 19th, 1898.

* *Vide* '*Clinical Journal*,' May, 1898 (a lecture on "Hysterical Paralysis").

wine trade, and furthermore he had the disadvantage of contracting syphilis at the age of twenty-five. He has had various syphilitic manifestations, from which he has now practically recovered. But the tremors which you see, affecting his lips and hands when he makes any voluntary movement, are probably of alcoholic origin. They have made great improvement since he has diminished his alcoholic consumption. Alcoholic tremors may be diagnosed by being small, rhythmical, "intentional," and worse in the morning; by their affecting the hands, lips, and tongue by preference; and by their being accompanied by alcoholic dyspepsia and other evidences of alcoholism.

7. *Metallic tremor*.—Tremor is apt to arise in chronic poisoning by four metals—mercury, lead, zinc, and arsenic. It is not a frequent manifestation of poisoning by any of these, and it seems to arise more often when the metal is introduced through the lungs than when it is introduced into the system by other means. I have heard it stated that lead poisoning does not produce tremor, but lead tremors are not uncommon in the Cornish and Cumberland miners who breathe the fine dust in the course of their work; mercurial tremor occurs in the thermometer makers and water-gilders; and zinc and arsenic tremors occasionally arise among the workers in brass foundries. They all have the same characteristics—being usually very small (though sometimes of wider extent), rhythmical, and "intentional." Apart from these features they can be identified only by the other signs of poisoning by the metal in question.

8. *Other toxic conditions*—e.g. diabetes, ague, influenza, and various pyrexial states—may be attended by tremor, which is a small, rapid, fibrillar tremor, and rhythmical. It is indistinguishable from the preceding excepting by a history of the cause, and the circumstances under which it occurs. It is highly probable that neurasthenic tremor, the tremor frequently present in cases of neurasthenia, belongs to this category. Clinically they have a close resemblance.

9. "*Nervous tremor*" is a very frequent condition among the out-patients here. It often arises in association with neurasthenia, and in cases of general debility from any cause. Belonging to this category, and presenting the same clinical

features, there is also the tremor localised in certain muscles or groups of muscles in association with certain occupations, notably violinists, pianists, and clerks. These "occupation neuroses" may be manifested as a flaccid paralysis, a tonic rigidity, or a tremor of the over-used muscles. The first is the most frequent, but the second and third sometimes arise.

It is worth while noting in this connection that attacks of muscular tremor, which may even amount to a general shivering, are apt to arise in several conditions in which we know the sympathetic system to be involved. The simplest illustration of this is to be found in the general trembling after the application of cold to the surface of the body. Here other evidences of vasomotor and sympathetic disturbance are to be found in the pallor of the skin and the irritation of the pilo-motor system ("goose-flesh"). We may also get shivering at the end of what my colleague, Dr. Harry Campbell, calls so appropriately the "flush storm."* In neurasthenia also there is, as I have elsewhere pointed out,† abundant evidence to show that the sympathetic system is affected, and that there is a toxic derangement of the blood. Lastly, in Graves' disease, in which a fine tremor is frequently observed, we now know that the cardio-vascular and nervous symptoms are intimately connected with, if not actually due to, sympathetic disturbance. But we have not time to pursue these interesting speculations further, excepting to note the intimate connection between the kind of tremor under consideration and sympathetic disturbance, and with toxic states of the blood.

10. There are several *other diseases* in which tremor occurs, though it may not be a very prominent feature. Among these may be mentioned the following:

(1) *Tumour of the brain*, under certain conditions, gives rise to tremulousness in the limbs corresponding with that part of the motor tract affected by the tumour. It is specially common in cases of cerebral tumour in children, and particularly if the tumour presses on without actually destroying some part of the motor tract. As Gowers points out, the tremor in such cases

* 'On Morbid Blushing and Flushing,' Harry Campbell, London, 1892.

† 'Clinical Journal,' 1897.

is so much like disseminated sclerosis as to give rise to frequent mistakes—mistakes which are sometimes cleared up only in the post-mortem room. Another condition under which cerebral tumour gives rise to tremulousness is when the growth is situated over the parietal region of the cortex or near the optic thalamus. Here, again, organic disease in this situation is more likely to produce partial damage than actual destruction of the motor centres or the motor tract.

(2) *Lateral sclerosis* is sometimes associated with tremor in addition to rigidity and increased deep reflexes, which are its more constant associates. This tremor may be dependent on or greatly increased by voluntary muscular action.

(3) *Friedreich's hereditary ataxy* is a disease usually arising in several children of the same family. It presents symptoms resembling a combination of tabes dorsalis and disseminated sclerosis. Amongst the latter we get tremor. It is, fortunately, a rare condition, and is found in children who seldom reach middle life.

(4) In various forms of *amyotrophic paralysis*, and particularly in "progressive muscular atrophy," fibrillary tremors of the muscles are not uncommon.

(5) Among the symptoms of *Graves' or Basedow's disease* fine fibrillar tremor is one of fairly constant occurrence. You will remember that this disease gives rise to four groups of symptoms which usually come on in the following order:—(i) Neurasthenic symptoms; (ii) cardiovascular symptoms; (iii) thyroid enlargement, and (iv) proptosis. Now the tremor belongs to the first of these groups. Here is a woman named M—, æt. 43. She has a long history of nervousness. From the age of twenty, when she began to have children, she has always been very nervous, especially after each confinement, and her convalescence was always very tardy. At these times she had "nervous fears," was easily startled, and dreaded to be left alone, especially out of doors (agoraphobia). However, it was not until nine months ago that she began to have cardiovascular symptoms in the shape of palpitation and shortness of breath. Her pulse has of late been very rapid, 134 to 160 to the minute. It was about this time, *i. e.* nine months ago, that the tremor which you can now observe came on. It is a very fine, perfectly rhythmical, very rapid

tremor, affecting chiefly the hands. Finally, only six weeks before she first came under my care, she noticed her neck was getting large, and her eyes prominent.

When she first came to the hospital three months ago, I immediately put her upon thymus, five grains of the fresh extract twice a day. She has made very marked improvement under this treatment, and in the course of three months she now not only professes herself very much better in every respect, but the neck measures one inch smaller than it was. It is only right to mention that for part of this time, when the palpitation was very severe, she took ten grains of bromide of ammonium and four minims of tincture of digitalis thrice daily for a short time; but she has not taken it for some weeks now, and yet the improvement has continued, and latterly the amelioration has been so well marked that there is no doubt in my mind that it is the thymus which has produced the good effect.

This completes the list of tremors or movements of small range, and for the most part regular rhythm. The general principles which guide us in their treatment are those already mentioned when I was dealing with clonic spasms. But, in order that our therapeutic measures may have a rational basis, it will be well to review the whole subject from a pathological standpoint. To this end I will attempt to classify the several conditions according to their pathological causation.

I think, gentlemen, that I have adduced abundant evidence to show that tremors and clonic spasms are due to a partial or incomplete damage (or imperfect recovery) of some part of the motor tract. It is obvious that this partial damage may be produced in various ways; and I will endeavour to classify the different diseases we have been considering under the several agencies, thus:

1. *Autotoxic or heterotoxic blood-states*.—A little consideration will show us that altered blood-states cause a gradual, and for a long time only partial damage to nerve structures. Among the diseases probably thus explained are the following:

Chorea (possibly a heterogenic bacterial toxin).
Metallic, alcoholic, and other toxic conditions.
Fatigue spasms.
Athetosis (possibly, in my belief).

Carphology, and the subsultus tendinum of pyrexia.

Graves' disease (considered as a disordered internal secretion).

2. *Slowly progressive, degenerative, or destructive changes.*

Paralysis agitans, and senile tremor.

Disseminated sclerosis, and Friedreich's disease.

Lateral sclerosis, and post-paralytic spasms.

General paralysis of the insane.*

Progressive muscular atrophy.†

3. *Localised organic disease* (producing partial damage):

Cerebral tumour (certain cases).

Certain cases of facial spasm.

„ „ of torticollis.

4. *An increased reflex irritability*, resulting from such causes as over-use, fatigue, long exhausting illnesses, general debility, anæmia and similar blood defects:

Occupation tremor.

Fatigue spasm.

Neurasthenic tremor.

Habit spasm.

Clonic facial spasm (certain cases).

Among the predisposing causes which may act concurrently with these conditions are heredity, long exhausting illnesses, shock (which is also an exciting cause), debility, and the strumous diathesis.

Such are the agencies which I believe to be in operation. The remedies selected must be appropriate to the one which happens to be present. I feel persuaded you will find, in treatment carried out on these lines, confirmation of the suggestions I have ventured to put forward. It is not possible at this late hour to enter further into the matter than has been done when speaking of the several conditions. There is, however, one remedy which seems to be suitable to them all—rest, taken in its widest meaning, a remedy, upon the value of which I am never tired of insisting.

* Now known to be due to malnutrition of the cortical cells of the cerebrum.

† Tremors here possibly due to partial destruction of the muscular fibres themselves.

THE TREATMENT OF ANGINA PECTORIS.

LYON writes in the 'Revue de Thérapeutique Médico-Chirurgicale' on this theme. For the treatment of the attack itself, rest, the inhalation of five or six drops of nitrite of amyl and a hypodermic injection of $\frac{1}{100}$ of a grain of nitro-glycerine are to be resorted to. To overcome the syncope ether, caffeine, or camphorated oil, the last in 10 per cent. strength, is to be employed. Friction should also be applied to the limbs, and, should there be evidences of pulmonary involvement venesection must be practised, while if respiration fails rhythmic tractions of the tongue must be performed. Injections of morphine are contra-indicated in such cases. In those cases in which the neuritis is apparently due to involvement of the cardiac plexus morphine may be admissible. Fifteen to forty-five grain doses of antipyrin may be given by the stomach or by rectal injection, or smaller amounts of phenacetin may be used, and to the point of pain chloroform of ethyl spray may be applied.

For the treatment between the attacks care should be taken that exercise does not immediately follow a meal, and that sudden motions are avoided. Mild exercise should be taken, but cold baths are not advisable. Smoking should be refrained from. Massage and friction of the right chest with alcoholic liquids may be resorted to. In regard to the diet, the patient should refrain from all rich dishes and fermented drinks, and tea, coffee, and alcohol, and should live largely upon milk, eggs, green vegetables, and properly cooked fresh meats. Water should be taken at each meal. For two or three weeks out of every month 30 or 40 grains of iodide of potassium should be taken a day, and for the remaining days of the month $\frac{1}{100}$ of a grain of nitro-glycerine may be similarly taken. Sometimes it is wise to increase the dose of the latter drug. It is also suggested that counter-irritation should be applied in the form of a hot iron over the precordial region every eight days.

Where there is feebleness of the heart due to myocarditis a combination of digitalis and nitro-glycerine is of value.

For the treatment of false angina or cardialgia the medication should consist in nitrite of amyl,

antipyrin, bromide of potassium, and applications of ether or chloride of ethyl vapour to the pericardium.

A useful prescription is one composed of Hoffmann's anodyne, tincture of valerian, tincture of digitalis, and tincture of belladonna, of each one drachm. Ten to twenty drops of this are taken at the beginning of the attack.

For the treatment of the cause hydrotherapy is to be resorted to in the form of hot baths; faradisation of the painful region and the local application of cold by chloride of methyl spray is useful. Should the attack be due to hysteria the same treatment may be instituted. If to dyspepsia, a milk diet with hydrotherapy is useful. If due to the excessive use of tobacco, this drug must be prohibited and nitro-glycerine given as in the case of coronary angina. In the angina due to gout, diabetes, and malaria, relief must be given by remedying as far as possible the arterio-sclerosis, chiefly by the use of the iodides.—*Therapeutic Gazette*.

Treatment of Pertussis (R. B. GILBERT).—

The moment the disease is recognised I order an average dose of the tincture of belladonna, given once every eight hours, to be increased one drop daily until the full physiological effect is obtained, viz. widely dilated pupils, flushed cheeks, dry fauces, &c.

The maximum dose being reached in five or six days, it is continued until there is decided lessening of the severity of the cough, which may be confidently expected within ten days from the beginning of treatment. In addition to the belladonna I give every three hours during the night full doses of potassium bromide combined with phenacetin, which insures prolonged tranquil sleep and fewer coughing spells.

For a child one year old I write thus:

℞ Tr. belladonnæ ... 3j.

Sig.—One drop every eight hours, increasing one drop each day until the tenth day.

Also—

℞ Potass. bromid. ... 3ij.
Phenacetin ... grs. x.
Mucilaginis acaciæ,
Aquæ puræ ... āā 3j.

Ft. sol. Sig.—Teaspoonful every three hours during the night.

Pediatrics, October, 1898.

A SPECIMEN jar of the new patent rubber-oil enamel has been received from Messrs. May Roberts and Co. for our consideration and opinion. This preparation meets the long-felt want of some ready means whereby, in cases of ulceration of the gums from loosening plates, the soreness and distress may be speedily alleviated and the irritating condition removed. The obvious gain to a patient in thus being able to properly masticate his food only needs to be mentioned to be appreciated.

WE have received from Abrolis Limited samples of a high-class natural sparkling mineral table water called "Abrolis, the World's Table Water." A noteworthy advantage in this table water is the fact that no salt or other ingredient is added—the water is bottled in its absolutely pure natural state as it rises from the spring. The spring from which the water is taken is the Tonnisteiner Brunnen, so widely known on the Continent for the dietetic properties of its water, the principal components being carbonate of soda, chloride of sodium, and carbonate of magnesia. An exceptionally valuable feature is the presence of an unusual quantity of free and semi-combined carbonic acid, rendering this table water highly effervescent from natural causes. The taste of "Abrolis" is pleasant, and is entirely free from any trace of metallic harshness, indicating the absence of any artificial addition of carbonic acid gas. A further point which tells in favour of this table water is that it perfectly blends with spirits, wines, and fruit syrups without either hiding or impairing their flavour, and the objectionable soapiness so noticeable in some natural table waters is obviated in Abrolis by the fortunate circumstance that it contains a fair percentage of chloride of sodium. Considering the composition of this water it is not difficult to understand the reputation it has secured in medical treatment, acting as it does moderately upon the bowels and correcting acidity of the blood. It is justly claimed for this table water that it is not only an aid to digestion, but that it acts also as a preventive to such disorders as dyspepsia, rheumatism, and gout. Exceedingly moderate in price, exceptionally good in quality, and capable of being kept in capital condition for years, "Abrolis" undoubtedly ranks as the "World's Table Water."

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* Specially reported for *The Clinical Journal*. Revised by the Author.

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CONSTIPATION AND ITS MODERN TREATMENT.*

BY

GEORGE HERSCHELL, M.D.Lond.

IN selecting constipation as the text of my remarks this evening, I am influenced by the consideration that a discussion upon a common affection will be productive of more real help to us in the daily practice of our profession than an academic dissertation upon a more abstruse subject. And my object is more particularly to point out to you that constipation is not a symptom to be lightly regarded, but that every case which comes under our notice is worthy of careful study, and deserves even from a purely scientific standpoint, a more elaborate treatment than the routine prescription of a laxative.

But first of all it is necessary to have a clear idea of what we mean by the term constipation. It has been asserted by some writers, and it will be found stated in most text-books, that constipation is a purely relative matter, and that whilst some people have a daily motion of the bowels, others may have relief at much less frequent intervals without any detriment to their health. I must say that I do not agree with this doctrine, and believe it to be as erroneous as it is pernicious, because—

(1) It is more than probable that in the cases where the patient is supposed to be in perfect health with infrequent actions of the bowels, a more careful investigation would have established the fact that there were slight symptoms of auto-intoxication present. Such phenomena, usually supposed to indicate minor degrees of neurasthenia or nervous exhaustion, were not ascribed to their true cause or associated with the constipation.

(2) Absence of a daily evacuation will sooner or later infallibly produce in a purely mechanical manner changes in the bowels themselves, such as dilatation or permanent faecal retention.

* A Paper read before the West Kent Medico-Chirurgical Society on November 5th, 1898.

(3) In many of these cases there is already a morbid condition present in enlargement, either congenital or acquired, of the sigmoid flexure, and the explanation of the constipation is that, as this takes longer to fill, the action of the bowels will take place at longer intervals than normal. Therefore I think that we should define constipation in the following terms :—*Constipation is a condition in which a normal daily amount of food is taken, and its equivalent of débris is not daily discharged naturally from the body without the aid of artificial means.*

In pure constipation all the *débris* is eventually discharged from the body. When this does not occur there is *fecal retention*.

Very few cases of pure constipation are met with in practice, as they are nearly always complicated with some degree of *fecal retention*. This may be temporary and able to be removed by purgative medicines, or it may be permanent.

In the present paper I do not include the consideration of cases where the total amount of food taken by the mouth does not reach the duodenum, as in pyloric stenosis, but only those in which the normal amount leaves the stomach. I also shall not discuss acute constipation, which is rather to be studied under the head of obstruction of the bowels.

In order that we may rightly comprehend how constipation may arise it is necessary that we should study the normal act of defæcation. We shall then be in a position to appreciate the different factors which may interfere with and hinder its proper performance.

After the partially digested food leaves the stomach and enters the duodenum, its further progress along the alimentary canal may be divided into three stages, and in order that we may properly understand the mechanism of constipation they must be studied separately.

(a) First stage. The passage of the chyme along the small intestine. This ends with its propulsion into the cæcum through the ileo-cæcal valve.

(b) Second stage. The passage along the large intestine, and its final storage in the sigmoid flexure.

(c) Third stage. The actual expulsion of the *feces* from the body.

(a) *Passage of the Chyme along the Small Intestine.*

From the moment when the chyme enters the duodenum, it moves slowly and more or less continuously through the channel of the small intestines. During this time further digestion is progressing, and absorption of soluble matters is taking place. This onward motion is effected by what are known as the peristaltic movements of the intestines. These peristaltic movements are contractions of the circular and longitudinal muscular fibres of the bowel, co-ordinated in such a manner as to force the semi-fluid intestinal contents continually onward in one direction. To understand how this takes place we may imagine the intestine to be divided into an infinite number of transverse muscular rings, each independent of the others. The circular fibres in the ring at the spot where the peristaltic movement commences, contract, and thus lessen the calibre of the bowel at that point. The next ring will then contract in its turn, and the next, and so on. After a few rings have thus contracted the one which was first affected will dilate, and this dilatation will involve the second ring, and then the adjoining ones in succession. We have thus a length of contracted bowel, the distal end of which is advancing by involving fresh rings, whilst the proximal one is resuming its natural size. Thus we have a zone of contraction passing along the bowel. The effect of this upon the soft contents of the intestine will be precisely the same as if, having filled a thin wide tube with some pultaceous material, we were to pass over one end an inextensible ring of a smaller diameter. If we were to pull the tube through the ring we should drive its contents onwards. Such is the effect of the peristaltic movements of the bowel upon the contained chyme. At the same time the longitudinal fibres of the bowel will contract, and by shortening the tube in front of the advancing constriction materially assist the onward passage of the food. A peristaltic movement usually occurs in a short length of bowel only, and many are taking place simultaneously in different portions of the intestine. These muscular contractions are produced automatically by the presence of the contained chyme acting reflexly upon the local nerve mechanism of the bowel; and other things being equal, the more

irritating the chyme, the more violent and extensive will be these movements. In the normal condition about four hours should elapse before any given particle of food will traverse the whole distance from the duodenum to the ileo-cæcal valve.

(b) *Passage along the Large Intestine.*

Although the large bowel is much shorter than the small intestine, the passage of the food residues will occupy at least ten hours. Peristalsis is much less active. It is necessary that such should be the case since, during its stay here, all the available remaining nutriment has to be absorbed. The facility with which the food will pass along the large intestine depends very much upon the amount of cellulose which it contains. Not only is the presence of this substance essential to excite peristalsis, but also, like the straw which the ancient Egyptians put into their bricks, necessary to bind the mass into such a consistency that it can be dealt with by the intestine. So much is this a fact that it has been found experimentally that if an herbivorous animal, such as a rabbit, be fed upon a diet from which cellulose is completely absent, death shortly ensues. The fæces cling to the sides of the bowel, and the peristaltic movements are insufficient to dislodge them. It is also probable that up to a certain point the presence of flatus in the large intestine will assist the onward passage of the fæces. By the time the bowel contents have reached the sigmoid flexure they have assumed the familiar character of fæces, and they are stored here until the time arrives for the next act of defæcation. In a healthy man the sigmoid flexure holds the exact amount of fæces which are discharged at any one evacuation. When it has been emptied it is refilled from the contents of the colon, and the contents of the bowel will, so to speak, move down another peg. Thus our evacuation of to-day represents the *débris* of the meal of at least the day before yesterday. It is important to bear in mind that except immediately before an evacuation the rectum is normally quite empty. Nothing enters it until the commencement of the act of defæcation.

(c) *The Act of Defæcation.*

The first part of the act of defæcation is the passage of some fæcal matter into the rectum. This is effected by powerful peristaltic movements,

reflexly induced either by the accumulation of fæces in the sigmoid flexure, or by the stimulus of food introduced into the stomach. The presence of the fæces in the rectum stimulates the sensory fibres distributed to its mucous membrane, with the result that the ano-spinal centre in the lumbar cord is excited, and motor impulses are originated which are conveyed to the muscular substance of the whole length of the large intestine. By the same reflexes the lumbar centre of the anal sphincter is inhibited. The patient now voluntarily fills his lungs with air, closes the glottis, and contracts the abdominal muscles. The result is that the contents of the rectum are expelled. Such is the mechanism of the act of defæcation.

It is obvious that any defect in any factor of this complex process may produce what is known as constipation. We may conveniently arrange these abnormalities under the following heads:

1. Defective peristalsis. The *vis a tergo* is not sufficient.

(a) Due to absence of cellulose from the food.

(b) Deficient irritability of the terminals of the nerves in the intestinal walls. This may be a pure local condition, or part of a general neurasthenic condition, or the result of over-stimulation.

(c) Muscular weakness of the intestinal walls.

It is an open question whether this ever occurs except as the result of over-distension. Most atonic conditions of the intestines are due to the fact that the innervation of the intestine is weak, not that the muscular substance is feeble.

2. Increased resistance. The work the intestine has to do is abnormally increased.

(a) From irregular peristalsis and spasm of portions of the bowel. This will include anal spasm.

(b) Abnormal distension with fæces or other materials.

(c) Abnormal character of fæces, especially dryness.

(d) Mechanical interference with the movements of the intestines by adhesions to other organs, or pressure from enlarged viscera or tumours pressing upon them, or by stricture of the bowels themselves, and abnormalities of position, such as enteroptosis.

The final effort of defæcation may be defective from—

1. Abnormality or want of excitability of the spinal centre.

(a) From local disease.

(b) By inhibition from the brain or higher centres.

2. Weakness of the abdominal muscles.

It is thus evident that in any case of constipation we have a problem which may need some considerable study for its elucidation.

A patient consults us for constipation; it is our duty to find out upon what condition it depends, and what are the factors in its production;—in short, to ascertain what is really the matter with him, and not to dismiss him with a prescription for a pill containing, perhaps, cascara and belladonna. How shall we proceed to gain this knowledge?

The first thing to do is to find out whether we have to deal with a serious condition or merely a functional derangement. That is, whether the constipation is the result of some gross lesion, or whether it is essential and primary.

1. *We go carefully into the history of the case.*—

A great deal of information may be derived from this. I need only mention the progressive emaciation and the pallor of malignant disease. The duration of the affection will tell us a great deal. If recent and progressive, it is likely to be due to some organic lesion, if of long standing; it will be more likely to be due to some mere derangement of function. In this conjunction it may be interesting to draw attention to the fact that constipation is very often met with in Addison's disease, and is a useful means of diagnosing between this affection and pellagra. In the latter there is nearly always diarrhoea.

2. *The next thing to do is to examine for retained faecal matter.*—We begin with the rectum, and shall incidentally learn whether there is anal spasm or not. This, you are aware, is frequently the sole cause of obstinate constipation, especially in women. With the finger in the rectum you will readily feel any impacted mass of faeces. The patient is then to be placed on his back with the knees slightly drawn up, and the whole course of the colon explored by palpation and percussion. It is as well to confine yourself just at present to making out whether there are any faecal masses to

be felt, and not try to ascertain whether any displacements are present. It is best to do one thing at a time, and gain your information step by step. Special attention must be paid to the sigmoid flexure, as this is often distended, and a cause of constipation whilst the rest of the large intestine is empty.

Pain and tenderness, although constant concomitants of acute faecal blocking, are generally absent in the chronic cases which we have under consideration.

The next point to be passed before our mind is the result of palpation. In nearly a half of all the cases which have been under my observation, in which the action of douches has proved the existence of faecal retention, either abnormal dulness on percussion, resistance on palpation, or an absolute lump could be made out.

As regards the diagnosis of faecal retention in the ascending or descending colon a little manoeuvre is necessary. The patient having been placed on the back, with the knees well drawn up, one hand of the physician is placed on the abdomen below the tenth or eleventh costal cartilage, while the fingers of the other hand are passed behind the patient into the posterior hypochondriac region,—that is, into the interval between the last rib and the crest of the ilium. The colon can then be pressed upwards and forwards *against* the hand on the abdomen. The secret of the whole thing is that the hand in front must be kept firm and immovable. It is only by this conjoined manipulation that it is possible to detect the slighter degrees of retention in the colon with absolute certainty, as percussion sounds may be so altered or obscured by various concomitant circumstances as to render them valueless. A little practice in this method of manipulation will soon give the requisite dexterity. However, in abnormally fat people we cannot, even by this method, arrive at definite conclusions, and must arrive at our own diagnosis by flushing the colon and seeing if any old faecal matter comes away.

It is important to bear in mind that these cases point out that, whilst the finding by palpation of faecal masses in the colon makes our diagnosis conclusive, the converse does not hold good, because—

1. The faecal masses may be of small size and encysted.

Some of the severest cases which I have seen have been where nothing abnormal could be discovered on palpation. But considerable quantities of faecal material were brought away by injections, and the origin of the affection was demonstrated by the rapid improvement in the patient's condition following its removal.

2. The abdominal parietes may be so thick and fat that efficient palpation is out of the question.

It is a remarkable fact that the abdominal cavity appears able to stow away quite a large amount of faecal material, and yet nothing shall be discovered on palpation.

It is sometimes difficult to make out whether a lump which can be felt in the abdomen is of a faecal nature or otherwise. In a great many cases we can do this by taking advantage of a sign suggested by Gersuny.* "If the tumour be firmly pressed on with the finger, the intestinal mucous membrane adheres to the sticky masses of which it is composed; when the pressure is removed the mucous membrane slowly disengages itself, and the sign consists in the recognition of this gradual separation. The only conditions necessary for its production are a certain degree of dryness of the intestinal mucous membrane and a certain impressibility of the faecal mass; the surface of the latter can be softened if necessary by an oil enema. The important factor in the separation of the intestinal wall from the tumour is undoubtedly the pressure of the intestinal gases. The characteristic sensation can be obtained by pressing the oiled or soaped volar surface of one forefinger into the palm of the other hand, and very slowly withdrawing it." I have found this sign to be of great use on many occasions.

3. *Having found no faecal masses in the colon or rectum, the next thing to do is ascertain the exact position of the large intestine.*—The only one certain method of doing this is by distending the large intestine with gas or air. This may be done either by means of an ordinary double ball such as is attached to a spray apparatus, or better by the means suggested by Illoway.†

An ordinary syphon about two-thirds full of soda water is taken, and to the nozzle is attached a piece of india-rubber tubing which terminates in a colon

tube. This latter is carefully introduced a few inches through the anus. The syphon is now inverted, and the lever depressed. The effect of this is that the gas which is disengaged from the soda water passes into the colon, and, being completely under control, can be made to distend it as much or as little as is desired. The colon can then be easily recognised by palpation, and it can be seen whether it is in its normal position or not. Except in females the transverse colon should never be found below the umbilicus. In enteroposis of course it is found much lower, and this condition can be readily recognised—

a. By the shape of the abdomen.

b. By being able to feel the colon as a transverse cord below the umbilicus.

c. By abnormally distinct pulsation of the abdominal aorta above the umbilicus, as the colon which usually lies in front of it is no longer there to cover it.

d. The frequent association of a floating kidney.

4. *We must examine for intestinal spasm.*—This may exist in a general or partial form. The former is met with in lead poisoning and in certain affections of the brain, the latter accompanies colitis, colic, hysteria, and neurasthenia. It is thus of the highest importance to know whether it is present. In general enterospasm the abdomen is retracted and scaphoid. In the partial form there is not of necessity anything characteristic to be discovered on palpating the abdomen, although sometimes the intestines can be felt as tense round cords beneath the fingers. This is especially the case with the colon. But a clue to this condition is usually afforded by the character of the faeces, which are either passed as small narrow cylinders or as scybala. Of course, one must be on one's guard not to confound the narrow faeces passed in enterospasm with the characteristic motions of stricture of the rectum.

5. *Atony and dilatation of the stomach.*—This is a frequent cause of constipation, as there is delay in the passage of the food into the duodenum. The most reliable sign of its presence is the production of the splashing sound below the umbilicus later than three hours after a meal. The diagnosis may be made absolute by the use of the gyromele or of the electric gastrodiphane, or by the discovery of food remnants in the stomach seven hours after a meal by means of the stomach-tube.

* Wien. klin. Woch., October 1st, 1896.

† Illoway, 'Constipation in Adults and Children,' Macmillan and Co., 1897, p. 122.

6. *The heart and liver must be carefully examined* for any condition which would impede the return of blood from the portal circulation, and thus produce venous engorgement of the intestines.

7. *The condition of the spinal cord must be investigated.*

8. *Weakness of the abdominal muscles must be looked for.*—One of the best tests for this condition is to make the patient try and raise himself from a lying to a sitting position without the use of the arms. This is quite impossible if the abdominal muscles are at all weak.

Having made all these investigations and found nothing abnormal, we shall be tolerably justified in regarding the constipation as functional, and we therefore now proceed to inquire into—

9. The condition of the teeth. 10. The daily habits as to exercise, abuse of purgatives. 11. Fat and cellulose in the food. 12. The hardness of the water which is habitually consumed. 13. The possibility of lead poisoning, and 14. The signs of neurasthenia. 15. In all cases it is desirable to examine, by means of the centrifuge, a sample of the fæces passed. With this we can gain valuable information, as the different substances of which it is composed will separate into layers, and we shall be able to estimate without any trouble the amount of undigested fat, cellulose, &c., which it contains.

(*To be continued.*)

Stab Wound of Thoracic Duct.—Dr. W. H. Lyne, of Richmond, Va. ('Medical Register,' August, 1898), reports the case of a twenty-four year old negro of splendid physique who received a stab wound above and behind the left clavicle and parallel with the outer border of the sternocleidomastoid near its attachment, producing a longitudinal wound of the thoracic duct. There had been considerable hæmorrhage, which had stopped, and an abundant milky fluid was steadily escaping from the wound. The wound was cleansed with a weak carbolic solution and packed with iodoform gauze. The patient was then removed to the hospital three miles distant. Seven hours later the dressing was removed, and the oozing of chyle and blood had completely stopped. The patient was allowed a light diet. His recovery was prompt and uneventful. The only untoward symptoms were some suppuration. The patient was discharged nine days after his admission, complaining only of a slight stiffness of his left arm. The patient two years later was in perfect health, weighing ten pounds more than he ever weighed before.—*Medicine*, October, 1898.

SOME DEFORMITIES OF THE CHEST WALL.

A Clinical Demonstration given at the Hospital for Sick Children, Great Ormond Street, London, October 20th, 1898,

By F. E. BATTEN, M.A., M.D., B.C.,

Assistant Physician to the Hospital.

GENTLEMEN,—I propose to-day to call your attention to some of the common deformities met with in the chest wall of children, and more especially that form known as the small chest.

But before proceeding to discuss the deformity of the chest I would, with your permission, remind you of certain points with regard to the development of the chest of a normal child.

The chest of a child at birth is in measurement nearly equal in its two diameters, so that on section it is nearly circular. During the first months of life, although the chest grows rapidly in circumference, it still retains its circular shape. This, however, in the later months of the first year of life and in the second year begins to alter, and the transverse diameter increases out of proportion to the antero-posterior, so that the proportion at ten years of age comes to be as two is to three. The chart which I hand round demonstrates the rapid growth in circumference of the chest which takes place during especially the first year, and to a lesser extent during the second year of life, after which the development of the thorax is more gradual from year to year. The proportions given above will be found to differ considerably from those usually given, and the reason of this is probably to be found in the fact that the measurements are taken at different levels; the measurements in the present case were taken at the level of the junction of the fifth rib with its cartilages. And in taking measurements of a chest there are two important points to be borne in mind: firstly, to take all measurements at a given level; and secondly, to take such measurements at the end of natural inspiration.

Methods of estimating and recording deformities of the chest:

(1) With callipers—giving the lateral and antero-posterior diameters of the chest.

(2) Cyrtometer—that is to say, pliable strips of

metal which can be moulded to the shape of the chest, giving the shape of the chest as it would be seen in transverse section.

(3) Tape measure, giving the actual measurement of the one side as compared with the other, and also the circumference.

The results of these instruments are, however, not very satisfactory, and for clinical purposes the deformities as noted by the eye are sufficient, and convey to one a better idea than any figures. In all measurements of the chest it is important to take them at the same level, namely, at the junction of the fifth rib with its cartilages.

The first chest that I propose to bring under your notice to-day is a type that is extremely common, and of which little or no notice is usually taken because it forms merely a part of the general condition of the child, viz. the small chest. This occurs in the small child, a child with small bones, small muscles, and sometimes small mental power.

If one takes measurements of such a chest one finds that it is smaller in all diameters than the normal chest, but there is no disproportion in the diameters.

In this child the circumference of the chest measures at the level above named $17\frac{1}{2}$ inches, and the child's age is three and a half. Now comparing this measurement with the measurement of the chest of the normal child, one finds that at that age the chest should in circumference measure from 21 to 22 inches. On examining the chest one notices that the ribs are more obliquely set than normal, and the result of this obliquity is that the intercostal spaces are narrower than normal, the angle formed between the ribs and cartilages is more acute, and also that the costal angle is much diminished. The lower ribs do not tend to protrude, but, on the contrary, are turned inwards, and the abdomen is retracted; the child has a tendency to bring the shoulders forward, and there is some drooping. The back is straight. The respiratory movements of the chest are slight. On looking at the child one is struck by its smallness; the child is not out of proportion, but she is in miniature, the bones are thin and fine, the musculature is very poorly developed, but there is in this child no atrophy or non-development of any group of muscles; the hands and feet are small and thin.

On physical examination one can find nothing

wrong; the heart, lungs, abdominal organs, all seem normal, and their functions are well performed. These children are generally intelligent and often exceedingly bright, and the mother brings the child because it does not "get on," or because she thinks the child is consumptive; but as a rule there is no very definite complaint. One sees from time to time a good many of these children; and though no doubt some of them may develop signs of phthisis, it is certain that the majority of them, and especially children rather older than this one, are not phthisical, and eventually develop into perfectly normal children. This condition may of course be associated with other maldevelopments. Rickets has no part in the development of such a chest as this child has.

Causes of the small chest.—The causes of this non-development in the chest of a child are numerous, and first one would mention any obstruction to the nose or upper air-passages by means of adenoids and enlarged tonsils, but in many of these children there is absolutely no evidence of any nasal obstruction. In some of these children with small chests there is a marked muscular defect, but the proportion of these is very small. In others there is a certain amount of cerebral defect, and the children are late in learning to walk and late in learning to talk, but the majority of them are children who have been under bad hygienic conditions, insufficient and improper feeding, and lack of exercise. Jenner in his well-known lecture on the subject states that this condition is due to congenitally small lungs.

With regard to the treatment and prognosis of this condition (omitting those cases in which there is definite obstruction to the upper air-passages by enlarged tonsils and adenoids), it has already been noticed that these children breathe very shallowly, and one's first indication with regard to treatment is to make the child breathe more deeply. For this purpose the child stands before her mother for five minutes every day, and breathes in and out deeply through the nose. The child should not be allowed to hold the breath long at the end of inspiration, and about ten or twelve complete inspirations should be taken in the minute. The child may perform these breathing exercises while in the recumbent position, and the weight of the shoulder falling back certainly tends to make the inspiration more effective. The child should not

be allowed to use the accessory muscles of inspiration, which it tends to do unless checked. Gymnastic exercises are useful, and are generally carried out under instruction at school. With regard to prognosis, these cases do very well; the chest increases in diameter, the ribs become less oblique, the child less anæmic, and improvement takes place in every direction; and in cases where there is no progressive loss of weight, and in which the temperature does not for a considerable period exceed the normal, one may give a most hopeful prognosis. Other general hygienic remedies should likewise be adopted.

Rickety deformity of the chest.—The next child that I bring before you presents a well-marked case of rickety deformity of the chest in association with bronchitis. The characteristics of this form are so well known to you that I will merely enumerate them—the beaded ribs, the figure-of-8-shaped chest, owing to the lateral grooves formed at the junction of the ribs with the costal cartilage, the deepest part of the groove being just outside the beads, and the sharp angles formed where the lateral and dorsal parts of the ribs unite.

The transverse groove is more marked on the right side than the left, on account of the liver which keeps the lower ribs out, and also the fact that the heart on the left side prevents the ribs on that side from falling in to the same extent as on the opposite side.

The antero-posterior diameter is increased, and there is a prominence in the dorsal region of the spine. This child has had bronchitis for some considerable time, and it is that which has kept up the rickety chest, for if the bronchitis had been cured the chest would probably have become normal by this age. All these points are well shown also in the specimen which I have here from a child who suffered from extreme rickets.

The next case is a boy æt. 4, who is rachitic, and has suffered from bronchitis off and on for the whole of his life. When I first saw him at five months old his chest was extremely rachitic, and I show him to-day in order to prove to you what a large amount of improvement may take place in a rachitic chest, even when bronchitis is present; for it is well known that the rachitic chest in a child who does not suffer from bronchitis, rapidly recovers itself, so that in course of time it is impossible to say that signs of rickets remain.

That, however, is not so with the present case, for he exhibits not only in the head but also in his legs well-marked signs of old rickets.

The next case is that of a boy aged nine and a half years, whom I have had under my care since he was six years of age, with a well-marked emphysematous chest. It is not often one sees such a well-marked case of emphysema in a boy. His chest presents the typical appearance as seen in an adult: the chest is barrel-shaped; the ribs tend to be horizontal in direction, the shoulders are raised, the distance between the chin and the episternal notch is diminished, and he has a wide costal angle. The boy has been under my care suffering from bronchitis for three winters.

The lecturer next dealt with the condition known as pigeon-breast, pointing out that the condition was caused by the impediment to the free entrance of air into the lungs, and distinguished the condition from the prominent sternum often found in rickets by the fact that in rickets the curve begins at the upper part of the sternum, whereas in pigeon-breast the prominence was only in the lower portion of the sternum.

He then considered the depression which sometimes occurs at the lower part of the sternum, and said that it was due to pulmonary or laryngeal obstruction, and was especially liable to follow whooping-cough when occurring in early life.

He next alluded to the contracted condition of the thoracic wall on one side, as seen in cases of empyema.

The same condition, he said, was sometimes met with in fibroid lung, in which there was falling in of one side. Empyemata which have been of long standing without operation often, when operated upon, gave rise to a chest of that shape, for during the continuance of the empyema the pleura became extremely thickened, and when the pus was let out the lung failed to expand, and in consequence there was a subsidence of the chest wall. A specimen was here shown of the thorax of a child, demonstrating the position the ribs assume in a child with empyema of long standing. In this case two ribs had been resected, and the whole side had fallen together as much as was possible.

Dr. Batten then showed a cast of a chest illustrating the deformity which occurs in cardiac disease. In this case there was an enormous bulging over the precordial area of the chest, such

as one saw not infrequently in children who were suffering from morbus cordis of considerable standing, and in whom considerable hypertrophy of the organ had taken place. The condition was often present in cases in which there was adherent pericardium, but it was by no means limited to this condition, occurring often in children who had much hypertrophied hearts from endo- and myo-carditis following rheumatism.

The lecturer went on to speak of congenital deformities of the chest wall, and showed a specimen and drawing as illustration of his remarks. The drawing was from a boy with non-development of the anterior portion of the ribs and the chest wall, and was taken during inspiration, so that the soft parts were sucked in. The specimen was also taken from the same condition in a child. They would observe that there was a deficiency of the anterior portion of the third, fourth, and fifth ribs, with the costal cartilages on the right side, and the space was partly closed by a membrane. He showed a puny infant with lateral curvature of marked degree, and observed that it was extremely uncommon to find such marked lateral curvature in a child so young. The child was epileptic, and convulsions could very easily be induced. There was more weakness on the left side of the body than on the right, but on the other hand the convexity of the curvature was to the right: this would seem to suggest that the muscles on the left side of the back were the strong ones. There was nothing in the chest of the child to account for its condition, and the only explanation he could offer was that it was due to weakness of the muscles on the opposite side of the body. It was also noticeable that the child's left eye was smaller than the right, it did not move its left hand readily; both knee-jerks were brisk. Probably the case was one of non-development of one side of the brain, of which there were good examples in the hospital museum. It had been argued that if it was arrest of development, and not a degenerative process, these children ought not to have convulsions, but the fact remains that where one gets non-development of the cerebrum there were often convulsions.

In conclusion the lecturer showed a boy eleven and a half years of age; he was quite well until he had measles at the age of eight. He was previously a fat child, but was nearly four years of age

before he began to walk, and could not walk properly until he was five. It would be seen that he walked in the typical unsteady manner which was associated with ataxy. Ataxy in children was met with under three conditions, namely, in Friedreich's disease, in diphtheritic paralysis, and in cerebral disease. He had lateral curvature of the spine, but that gave rise to no deformity of the chest wall anteriorly; he also had the condition of pes cavus, and already his toe was beginning to be dorsiflexed. With regard to the knee-jerks, although examined carefully many times, they could not be obtained. It would be noticed also that he had a slow deliberate speech. It was generally agreed that he was a subject of Friedreich's disease. Most cases of Friedreich's disease exhibit nystagmus, but this boy had not that symptom, but he had slow speech, ataxic swaying gait, inco-ordination of the hands, lateral curvature, pes cavus, and absent knee jerks. There was also observable in him a certain amount of mental defect, which occurs in a certain proportion of cases of this disease. There was in this case no hereditary history.

Albuminuria as a Lithæmic Manifestation in Early Life.

—B. K. Rachford calls attention to a group of symptoms from which young lithæmic patients suffer, which come and go, and are characterised by nausea, uncontrollable vomiting, slight elevation of temperature, and more or less pain in the head and in the gastric region. A study of these cases has developed the fact that there is a transient albuminuria, which continues during and for a short time after the other symptoms of the attack have disappeared. This albuminuria, he says, cannot be explained by arterial changes at this early period of life, and can only be due to the irritation of the delicate kidney structures of the child, which results from the attempt at elimination from the blood of the poisonous and irritating products, the causes of the lithæmic attacks. The comparative infrequency of lithæmic albuminuria in late childhood and early adult life is due to the better developed and more resisting structure of the kidney, and to the fact that the arterial changes found in old lithæmics have not yet had time to develop.

Amer. Journ. of Obst., October, 1898.

DEMONSTRATION OF CASES
AT THE
NORTH-WEST LONDON CLINICAL
SOCIETY,

Held at the North-West London Hospital,
October 19th, 1898.

Dr. CHITTENDEN in the Chair.

Tubercular Disease of the Epiglottis.

MR. MAYO COLLIER showed a boy, the subject of tubercular disease of the epiglottis. He thought the Society would be interested in the case because he had not previously seen one similar to it. The boy came to him (Mr. Collier) six months before suffering from irritation at the back of the throat, with symptoms of wasting and marasmus and general poor condition. He proceeded to examine him in the usual way, but found nothing in his lungs or abdominal cavity to account for the marasmus and wasting. He then examined his throat and found the following: pharynx fairly normal, but some slight congestion was present. The nose was fairly normal, but there was some slight post-nasal catarrh. There was a well-marked and thickened epiglottis, but no evidence of implication of any portion of the larynx or trachea. An epiglottis could be seen which was of remarkably good shape, but about half an inch in thickness, with a fissure between the two halves suggestive of commencing ulceration. What did the boy suffer from? That question was put to him by Dr. Campbell, and he was prepared to argue the point. The only affections from which the patient could be suffering were three: (i) chronic laryngeal catarrh, with much thickening; (ii) tubercular disease of the larynx; or, (iii) syphilis. No other portion of the larynx was affected in this case except the arytenoids, and those only superficially and not markedly. Therefore he begged that they would exclude laryngeal catarrh. The nasal chamber was free, the function of the nose was carried on, and there was no evidence of catarrh except in the larynx. Next came the question of syphilis. There was no evidence of syphilis, and he had never heard of inherited syphilis affecting an isolated portion of the larynx,

as in this case. Syphilis affected the larynx by extreme congestion, and there was often ulceration and loss of cartilage, but there was nothing of that in the present case. They were therefore thrown back upon tubercle, and he was inclined to take that view. The case had gone on for six months without any good having been done for him, excepting getting the throat and nose into better condition by irrigation and the use of a spray of chloride of zinc. Some commencing ulceration was evident in the middle line between the two portions of the epiglottis.

In reply to the chairman, Mr. Collier said he had not made a cultivation, as there had not been any scrapings taken. He did not think they were justified in scraping the larynx at the present moment, but in about a month's time they might reasonably take a scraping and examine the *débris*.

Mr. MAYO COLLIER next showed a case illustrating the effect of nasal obstruction on the growing skull. The case was one of the class to which he had endeavoured to draw attention in the last few years, showing the effects of chronic nasal obstruction on the skull of young persons. When nasal obstruction affected young persons between birth and the age of eighteen and twenty, that is to say, before the time of ossification, the manifestations were as follows:—When the patient opened his mouth it would be seen that the palate was very much pushed up, and the teeth crowded and irregular. The nose was protruded forwards, the upper lip was small and atrophied, and the lower jaw advanced in front of the upper one, giving a prognathous appearance to the face. The mechanism he took to be that from the result of chronic catarrh in youth, the turbinal bodies became enlarged, and the whole lining membrane of the nasal cavity similarly affected. The air not being able to enter the nasal cavities, oral inspiration resulted, which set up a negative pressure in the nasal and post-nasal spaces. The unequilibrated atmospheric pressure exerted its influence on the upper maxillary bones, and the walls of the nasal box were subjected to an abnormal pressure. The tension inside and outside the chamber not being equal, the nasal walls in all directions had to bear the inward pressure. The consequence was that the interior of the nose was encroached upon in all directions. Instead of

the palate being a normal arch it was elevated and V-shaped; the nose was flattened and protruded forward, and the teeth were irregular and crowded. In addition to this various abnormal conditions were found in the interior of the nose. Chronic congestion was the result of the unsupported intravascular pressure. This led to chronic engorgement of the mucous membrane, and chronic perichondritis and thickening of the septum. In many cases the turbinal bones were approximated to the septum, so much so that a small probe could hardly be passed between the two. There was also nearly always chronic deafness and post-nasal catarrh.

Mr. MAYO COLLIER next showed a case simulating cardiac asthma. He said it was sent to him as a case of possible aneurysm or cardiac asthma. From the patient's symptoms he thought it probable that an aneurysm of the transverse arch of the aorta pressing upon the trachea or bronchi, or some tumour in the upper mediastinum would be found. The symptoms were difficulty in getting breath, being always short-winded, especially at night; and on lying down a frightful feeling of impending death. At such times he would sit up in bed, breathing quickly, with rapid pulse and congested face. This condition of affairs associated with indigestion and marasmus, produced by an offensive discharge from the back of the throat, placed him in a perilous condition. Mr. Collier examined the patient very carefully, and had the assistance of a physician to examine his chest, but nothing could be found in the lungs, mediastinum, or any other portion of the thorax. The patient, however, had two enormous tonsils and very well-marked pharyngitis, post-nasal catarrh, and extreme nasal obstruction. They would be able to see a very large turbinal body pushing over the septum, and on the other side there was a large turbinal body which filled up the vacant space, and descended to the floor of the nose.

The symptoms were now thought to be probably due to irritation of the upper pharynx, due to mouth breathing,—that is to say, due to the air not being properly prepared in the nasal chamber. Mr. Collier removed the tonsils and gave the patient a wash, and prepared him for an operation on his nose. He operated on the left side only of

the nose, gave him a simple tonic consisting of quinine and iron, and the patient would tell them that all his symptoms had disappeared, including the indigestion; he could perform his duties, and his life was now comparatively comfortable.

In the discussion on Mr. Collier's cases, Dr. HARRY CAMPBELL asked, in regard to the first case, whether there were many cases of primary tubercular disease of the epiglottis on record, and what was the prognosis in such cases.

Mr. COLLIER, in reply, said that such instances were extremely rare, which was his excuse for bringing the case forward. Every one had seen tubercular disease of the larynx, but tubercular disease of the epiglottis, apart from affections of the larynx and lungs, was extremely uncommon. Dr. Sibley had reminded him that the sequel of most of these cases was the involvement of the lungs in the tubercular process as well as the rest of the larynx later on.

Mr. J. G. TURNER dissented from Mr. Collier's explanation of the deformity in the second case. He thought the obstruction to breathing did away with the normal mechanical stimulus to growth attributable to the passage of air along the nares. At the same time the mouth was kept open, and there was a constant traction of the tissues of the cheek on the alveolus, which was yet pliable. He thought the approximation of the alveolus would quite well account for the appearance of height.

Dr. HARRY CAMPBELL said that apart from the mechanism of the process by which the parts were distorted he would like to say that since Mr. Collier three or four years ago directed his attention to the association between nasal obstruction and that particular deformity, he had never yet seen it absent, but in every case of chronic nasal obstruction which he had observed there had been this high-arched palate, and the other changes which Mr. Collier had described. Conversely, in almost every case of high-arched palate with very irregular teeth there had been a history of nasal obstruction. Occasionally congenital high-arched palates were met with unassociated with a history of nasal obstruction, but that was the exception.

Dr. BOULTING remarked that not merely idiots, but people with hysterical and neurotic temperaments had high-arched palates, and he would like

to ask Mr. Collier whether he had noticed in these cases that it was almost invariably due to nasal obstruction, and what was his view as to how far the interference with the proper oxygenation of the blood and the irritation arising from obstruction had to do with the development of an explosive nervous system.

Mr. COLLIER, in his reply to Mr. Turner and Dr. Boulting, elaborated his opening remarks, and alluding to manometric measurements to prove his view of the matter. With reference to the question asked by Dr. Boulting, nothing was more common than to have a certain amount of mental ineptitude associated with nasal obstruction. Children who suffered from enlarged tonsils and chronic nasal obstruction were dull and incapable of making mental progress. On reasoning the matter out it would be seen that the mental dulness was a natural sequence of the disease. Moreover, cases had been demonstrated by Saltinger and others where epilepsy and marked neurasthenia had been caused by nasal obstruction, and where, when the nasal obstruction had been removed, the cases got permanently and completely well.

Leukoplakia.

Dr. SIBLEY showed a man the subject of leukoplakia of the tongue. He said he brought the patient forward as he had a very typical but unusual ridge on the buccal mucous membrane. He happened to have a particularly well-preserved set of teeth, and at the junction of the teeth was very well-marked leukoplakia corresponding in nature to that on the tongue. He was thirty-four years of age, a porter by occupation. Probably he had had syphilis, he appeared to have been under treatment a short time ago for secondary symptoms. For some time he had been a great smoker, but for the last three or four years he had more or less given it up. His tongue had been tender, but he would not admit it had been particularly sore. It was generally said that this condition of leukoplakia was caused by tobacco smoking, but was probably due to the irritation of the heat of the stomach rather than to the pipe, although a spot on the tongue corresponding to where a man lodges his pipe was frequently found. Dr. Sibley believed, however, that the condition was produced by the stomach. But that was not always the case. He had had a very marked case of leukoplakia in

a woman, who he was convinced had never smoked in her life.

Mr. MAYO COLLIER thought the case of great interest. He was surprised at two points, namely, that in this tongue the portions of the tongue other than the centre and side were quite healthy, whereas it was usual to find in such tongues evidence of old scars and fissures, and furrows like those in a ploughed field, with white patches at particular spots. He agreed with Dr. Sibley's explanation of the white line. He would be inclined to look upon this particular case as traumatic rather than syphilitic,—that is to say, that it was due to the constant burning of the tongue. Of course if the man had syphilis, that was sufficient to clinch the argument.

Dr. CLAUDE TAYLOR noticed that the man had paralysis of the right side of the tongue, and he was informed that two years ago he was in Moorfields Hospital with paralysis of one of the orbital muscles, apparently of the external rectus on the right side. That cleared up after a time. Therefore the patient probably had had nuclear disease of the sixth nerve two years ago, and he suggested that he had nuclear disease of the twelfth nerve on the right side now. He believed these signs made syphilis a certainty; he attributed the leukoplakia to the same disease. Moreover the patient complained of aches and pains about his body, which were probably due to tertiary syphilis.

Mr. TURNER thought the two lines on the cheeks were quite as much scar tissue as anything else, marking the site of old ulceration. The position, where the cheek was rubbed by the edges of the crowns of the teeth, was a usual one for ulceration in secondary syphilis. He had noticed it specially round the lower wisdom-teeth.

Dr. SIBLEY, in reply, said the man might or might not have had syphilis, but in any case he did not think syphilis had anything to do with the condition of the mouth, which appeared to have been of slow advent and course. There was no scar tissue on the fauces, and the patient had never had syphilitic ulceration of the fauces.

Excision of Upper Jaw.

Mr. J. G. TURNER showed a young man, aged 27, illustrating the results of excision of the upper jaw. Four years ago the man had a swelling of the right maxilla, which gradually increased until eight

months before admission to St. Thomas's Hospital, when it became much more rapid in its growth. At the time he first saw it there was a very hard swelling, so hard that the question was raised whether it was not a bony swelling. The general opinion was that it was an osteosarcoma. As there was a good deal of springiness in it he thought it was a fibro-sarcoma. The patient went away, but in the meantime the growth increased greatly, and fungated through the sockets of the molar teeth, which it pushed out, skin not being implicated. On the 14th of January he was operated upon by Mr. W. H. Battle, and the tumour removed. It was a fibro-sarcoma springing from the body of the upper jaw and pushing up the antrum, but not entering it, showing what a thin plate of compact bone was required to limit the growth of such a tumour. An obturator was made, but not too large, so that there should not be any pressure on the part. The patient's greatest inconvenience now was that he got a watery discharge from that side of the nose. No glands seemed to be involved, and the prognosis therefore seemed to be very favourable.

The deformity left by the operation was very small, showing that immediate prosthesis—that is, the insertion at the time of operating of an artificial maxilla—was quite needless.

The parts had so healed and contracted that there was only a small communication about half an inch in diameter between the nasal and buccal cavities.

Mr. TURNER also showed a case of chronic abscess of the maxilla, which he thought might be of interest, because such a condition was very little noticed in books and writings. The whole body of the maxilla in such a case might be hollowed out, excepting only that part supporting the roots of the teeth, without any external evidence except a sinus and some thickening of the muco-periosteum. Such an abscess remained limited to the maxilla in which it started, and was lined by granulation tissue. Some thin pus would be discharged, and its progress was painless, or almost so, except in those few cases where the sinus got blocked. The abscess was started by a tooth in all cases; in all the cases he had seen this had been a central or lateral incisor. Such a causation was quite explicable when the tooth was carious and the pulp dead

and decomposing, but in quite half the cases the tooth was apparently sound. On drilling into such a tooth the pulp would be found dead and stinking, and for some considerable part of the circumference of the tooth a fine steel instrument would go right up to the apex, showing that the alveolar bone had gone and the alveolar dental ligament with it. There was a pocket left between the tooth and the gum, in which pocket any *débris* might lodge and become septic. Hence in these cases there were three sources of mischief, one or all of which might be active:—(1) An extension of the same carious process which had destroyed the alveolus; (2) septic absorption from the dead pulp, which had itself probably died from thrombosis of the nutrient vessels at the apex on the advent of the chronic carious process to that region; (3) septic absorption from the *débris* in the above-mentioned pocket.

In all the cases he had seen the sinus had been on the front of the alveolus, and though in some cases he had been able to pass a probe through this sinus and feel it on the hard palate with his finger, he had never seen a sinus form in this situation. Hitherto a dental cyst had often been described as a chronic abscess, and Mr. Turner exhaustively dwelt upon the points of differentiation. The teeth in such cases could be saved by ordinary antiseptic treatment, but a big opening must be made and the granulation tissue scraped away, the opening being kept patent with packing, while the cavity granulated up from the bottom. Healing was by fibrous tissue.

Primary Tuberculosis of the Kidneys.—

In Professor Israel's operations on the kidneys 10 per cent. have been on account of tuberculosis, and in sixteen out of twenty-one it was evidently a primary affection. He removed the entire organ in twenty cases. Three died from the immediate results of the operation; five deaths occurred later, after partial or complete recovery. In regard to the final results, three of the uncomplicated cases died, but the remaining six were permanently restored to health. None of the cases with complications were cured, although some were benefited to a remarkable extent. He advises an early operation in simple primary tuberculosis, or if the bladder is but slightly involved. He does not operate in secondary ascending tuberculosis, unless to relieve serious disturbances.

Vienna klin. Woch., September 15th.

A NOTE FROM THE CLINIC OF MR. LOCKWOOD AT ST. BARTHOLOMEW'S HOSPITAL.

A Phantom Tumour.

GENTLEMEN,—To-day there are no more operations, but two cases are coming in, which to my mind are quite as interesting as any operation. Both of these are about to be anæsthetised for the purpose of clearing up the diagnosis. The first is that of a schoolboy aged thirteen years, and whilst he is being brought under the influence of the anæsthetic I will tell you something of his history. It seems that six months ago he was in a gymnasium, and says that he fell off some rings to the ground. It is not exactly known how he fell, but he tells me that he struck the pit of his stomach. We have not had the corroborative evidence of any bystander. The story, however, does not seem to be very improbable. The blow was followed by pain, which went on increasing in severity, but he says that he did not vomit nor pass any blood from the bowels. The same evening a swelling was noticed in the upper part of the abdomen by the youth, and also by his mother. This swelling has not disappeared, and is said to be getting larger. The boy has, I believe, been treated for some time in a country hospital, but has not yet been examined under an anæsthetic. When the boy's abdomen was examined in the wards of this hospital, there was a considerable protuberance at its upper part betwixt the umbilicus and the ensiform cartilage; indeed, the whole of this space is projected forwards at least an inch. The lateral limits of this swelling correspond fairly accurately with the outer edges of the recti. I myself have already become suspicious that there is no real tumour in this case. First of all I was struck by the boy's appearance which was that of perfect health, and his pulse and temperature were normal. Moreover, he seemed to be taking more interest in what happened round him than in the supposed serious condition of his abdomen. Next I thought he arched his back in rather a suspicious manner, and I was not quite sure that his breathing was regular; in other words, he seemed to me to be endeavouring to depress his diaphragm, and to

hold his abdominal muscles still. The tumour itself had no definite outline, and was curiously symmetrical, being, as I have already said, limited by the outer part of the recti. Further, the tumour was resonant throughout, and absolutely painless. If the history were true, one would naturally have supposed that an injury which was sufficient to cause a cyst of the pancreas, or a hæmorrhage into the lesser peritoneal sac, would have been immediately followed by severe shock and collapse, accompanied by vomiting. However, I do not think an actual diagnosis can be made until the boy is anæsthetised. Now that the boy is under the influence of the anæsthetic we will expose his abdomen, and as he has lost consciousness you notice that his abdomen has become flat, and that no swelling whatever can be discerned.

Nevertheless it is always wise to be cautious before expressing a decided opinion upon the entire absence of any lesion. The aorta and the vertebral column can now be felt where the swelling used to be observed, and nothing whatever abnormal is to be palpated. Thus the simple act of giving an anæsthetic has immediately cleared up a somewhat rare and obscure case. I have seen many of these phantom tumours in young females, but I cannot recollect to have met with one in a youth. During the last year or two you may have seen several difficult cases made clear in the same way. Just before I went for my holiday it may be remembered by you that we anæsthetised a middle-aged female because we suspected that she had a moveable kidney. This patient suffered from indigestion, occasional vomiting, and pain whenever she exerted herself. The pain was usually referred to the right hypochondrium. Although her abdomen was slack nothing could be felt either in the region of the kidney or of the gall-bladder, but when the patient was anæsthetised and lying on her back we were still unable to discover anything. We then resorted to what must have appeared to you at the time a rather unusual manoeuvre. The woman was held up by the pelvis, face downwards, her body was then shaken sharply, when the kidney was immediately felt to fall against the front abdominal wall, and could be easily pushed back towards its proper position. I have known this method of examination render quite clear the diagnosis in other cases which had previously presented the greatest difficulty. You

may also recollect the case of a man who related symptoms which seemed to point to some inflammatory trouble in the region of the cæcum and vermiform appendix. There was no increase in temperature or acceleration of pulse, or other symptom which threw any light upon the nature of the pain which he felt in the lower part of the abdomen. His abdominal wall was exceedingly tense, but pressure although not painful failed to reveal anything abnormal. An anæsthetic at once set all doubt at rest, because as soon as the abdominal wall was slack the mass which so often accompanies an inflammation of the vermiform appendix could be felt. It is only fair to tell you that even under an anæsthetic illusory conditions may be felt. For instance, on two occasions when patients have been anæsthetised for the purpose of exploring the gall-bladder a distinct crackling like gall-stones rubbing against one another has been felt both by myself and by other surgeons. In one of these cases gall-stones were present and removed, but they were so few and small that it seemed absolutely impossible that they could have produced the sensation which was felt. On the other occasion the gall-bladder contained no stones whatever, but was merely full of bile. My own belief is that this peculiar sensation is caused by a kind of creaking in the fat of the abdominal wall. Also, even under anæsthesia, something may be felt in the region of the gall-bladder which may be mistaken for distension of that viscus, although an incision clearly shows that the sense of touch was misleading. I am hardly alluding to such cases as that in which I mistook an abnormal lobe of the liver for a distended gall-bladder, but two cases have occurred where I myself and others distinctly felt what was considered to be a distended gall-bladder, and in which nevertheless an incision enabled that viscus to be felt, and with difficulty seen, hidden away beneath the under surface of the liver. In such cases as these it is possible that the deception is produced by the outer edge of the rectus abdominis muscle, which seems to contract when it is palpated in spite of the unconscious condition of the patient.

(The boy referred to in the first part of this note was told that the tumour was only wind, and that he was to lie in bed and not to go into the hospital square until it had disappeared, which it speedily did. The friends were warned that

the tumour might come back again, and that if it did they were to deprive the boy of any favourite article of diet till the tumour had again disappeared.)

DISEASED BRONCHIAL GLANDS, ASSOCIATED WITH MEDIASTINAL ABSCESS.

By C. P. AMBLER, M.D.

THE so-called "bronchial glands" constitute that group of lymphatics, ten to fifteen in number, situated at the bifurcation of the right and left bronchus, or upon these tubes or their primary divisions. These glands vary in size from a pea to an almond, receive their blood-supply from the bronchial artery, and the same is returned by the bronchial veins. Besides this group of large glands, the bronchi and their branches are supplied with innumerable minute lymphatic glands; but it is with this group of relative large glands that we have to do in this discussion, as it is in these glands that suppuration usually occurs.

"The central group of glands is in relation in front with the pericardium, the arch of the aorta, and the pulmonary artery; behind, with the pulmonary plexus of nerves, the œsophagus, the aorta and vena azygos. The ganglia upon the upper, anterior, and posterior surfaces of the right bronchus are four or five in number, and are smaller than those of the central group. Their situation brings them into relation with the arch of the aorta, innominate and subclavian artery, with the brachio-cephalic vein, the vena azygos, the pneumogastric nerve, and its recurrent branch. The ganglia on the left bronchus are in relation with the arch of the aorta, the origin of the left subclavian and carotid arteries, with the large veins, with the left pneumogastric nerve, and especially with its recurrent branch."

Considering the important organs in direct contact with or adjacent to these glands, one would upon first thought expect to see considerable general manifestation of even a moderate change in their normal physical relation. Such, however, is far from the case.

It frequently happens that it is only in the advanced and aggravated cases that general symptoms become at all marked.

When these glands do undergo enlargement, however, they produce vague and indefinite symptoms, usually of a reflex nature, and which, if considerable care is not exercised, will be attributed to entirely different causes.

The group is regularly enlarged in pulmonary tuberculosis, the enlargement being in many cases out of all proportion in regard to the extent or seriousness of the lung involvement.

In certain cases the glands undergo enlargement and even suppuration, while the lungs themselves may be entirely free, or at least the symptoms of pulmonary disease are so slight as to escape recognition.

We frequently meet tuberculous patients in whom marked glandular enlargement is found in the axilla, about the neck, and especially just over and about the clavicle, but too often do we lose sight of the fact that where these superficial glands are enlarged, the probabilities are that their number and enlargement is slight in comparison to the condition present in the mediastinal space.

Surgical operations are undertaken for the radical removal of such enlarged cervical glands, where, if more minute detail was given to the examination of the chest, the same operation would in all probability be indefinitely postponed.

These glands become diseased and enlarged in precisely the same way as other lymphatic glands, adjacent to certain organs, become involved when the organ in question is diseased.

It is difficult to see how a primary infection of these glands could have occurred where no other evidence of disease is found.

Pepper states, "The infection takes place by penetration of the bacilli through the lining membrane of the air-passage." Personally I believe these glands usually become infected secondarily, as is so often the case in disease of the lungs, following primary infection at some point along the alimentary canal.

The bronchial glands may be said to participate in the diseases which affect the lymphatic glands generally, and no matter whether associated with pulmonary disease or not, in the large percentage of cases the disease will prove to be tuberculous.

Quain says, "The glands are at first swollen, red, and soft as in simple lymphadenitis. On close inspection tubercles may be seen in the substance of the gland, and very soon there is

cheesy necrosis affecting part, and finally the whole of the gland. Neighbouring glands tend to fuse into a single mass, which may be surrounded by more or less resisting connective-tissue membrane," thus forming a so-called mediastinal tumour.

The disease may lead to permanent enlargement, to contraction and induration of the glandular structure with the presence of calcareous deposits, or to abscess.

If the glandular mass breaks down completely a cyst containing a puriform liquid results, the walls of this cyst encroaching toward the mediastinal space. The rupture of this cyst is followed by diminution of the pressure symptoms, and results in accumulating pus formation in the mediastinum, provided, of course, the rupture is toward the mediastinum. When the pus collection discharges into the lung or bronchi, purulent expectoration results, thus easily misleading one in the diagnosis of the case, if not seen until this stage of disease has been arrived at. When inflamed the gland or glands may form adhesions with any of the surrounding parts, and the contents be discharged by an ulcerative process into the substance of the lung, the trachea, œsophagus, or blood-vessels.

The ætiology of diseased bronchial glands will in many cases remain obscure, but it is to local irritation or inflammatory diseases in organs or tissues with which these glands have a connection that the source of the disease may be frequently traced.

As we find the submaxillary or cervical lymphatic glands, enlarged from irritation or diseases of the mouth or throat, or the axillary or inguinal glands enlarged from disease in adjacent organs or tissues, so we may find the bronchial glands enlarged temporarily or permanently from inflammatory diseases in parts, the lymphatics of which pass to these glands.

Acute catarrhal congestion of the respiratory tract frequently leads to more or less temporary enlargement, but it is in tuberculous disease of the lungs that we generally have the cause of permanent enlargement, destructive process, and abscess formations.

The glands have also been observed enlarged in certain acute specific diseases, such as scarlet fever, measles, and typhoid.

Many pathologists have observed the uniform enlargement in whooping-cough, some writers, among whom is Dr. Hugh Ley, in his excellent work upon 'Laryngismus Stridulus,' even taking the position that whooping-cough may be a disease of the bronchial glands, due to a specific microbe, similar to that which affects the parotid glands in mumps, supposing the peculiar cough to be due to pressure upon the recurrent laryngeal nerve.

It is certain that in the majority of cases where the enlargement becomes marked, and pressure upon the pneumogastric or recurrent laryngeal nerve results, certain characteristic phenomena appear, such as crowing cough, paroxysmal cough, vomiting, loss of voice, and the like.

The symptomatology of the disease is governed by the manner of involvement of the glands. When the glands are primarily involved, or when the pulmonary involvement is slight, the symptoms are paroxysmal cough, dyspnoea, simulating asthma and hoarseness; in those cases developed during the course of pulmonary tuberculosis the patient, of course, presents the usual symptoms of phthisis, but in proportion to the degree of the enlargement the cough becomes more and more paroxysmal, and may be finally quite like that present in whooping-cough. Unlike whooping-cough, however, these paroxysms may be violent during one day, and slight or entirely wanting the day following, only to recur later on with gradual increasing severity.

Of the symptoms next in importance to cough is pain. This pain is usually complained of all through the inflammatory disease of the glands as a deep-seated and increasing pain of a boring character, situated beneath the upper third of the sternum.

Tenderness upon pressure over the sternum is usually present after the disease has become marked.

Where abscess formation occurs, and rupture into the mediastinum results, the pain is temporarily relieved, but increases in greater severity as the accumulation of pus follows. If the glandular mass is considerable in size the patient is unable to lie upon the back for any continued length of time, on account of pain from pressure upon the organs beneath. Cases have been recorded where such pressure was sufficient, with the patient reclining upon the left side, to greatly interfere with the action of the heart.

In certain positions the patient will cough, in others he becomes dyspnoeic, and still in others suffers more acute pain.

Physical examination may or may not reveal dulness upon percussion, depending entirely upon the degree of enlargement, and the hearing power and skill of the examiner. If mediastinal abscess has already formed and the accumulation is large the dulness will be found both over the anterior mediastinal space and also between the scapulæ, while in all probability a change in the line of percussion dulness follows change of position.

In glandular enlargement without abscess formation the dulness will be more marked anteriorly in the region of the upper third of the sternum.

Auscultation sounds will depend upon the degree of enlargement; usually fine crackling râles are heard along the sternal border; these râles may be either from the substance of the lung or from the pleural surface.

If the patient presents enlarged glands in the cervical or clavicular region, it is fair to presume that those on the interior of the chest are enlarged more or less also.

The diagnosis, however, in the majority of cases must be made "upon the dulness with irregular boundaries, weakness or absence of breathing sounds; upon the unilateral dilatations of the thorax and its incomplete expansion; moreover, by the pressure exercised upon different nerves, such as loss of sensibility, voice, paroxysmal cough, &c., upon the constant pain, also upon vascular compression, the symptoms of which are cyanosis, œdema of the face, neck, and upper extremities, and weakness of the radial pulse, and, above all, upon the swelling of the visible lymphatics in the various parts of the body."

The symptoms mentioned above are not by any means always indicative of either diseased bronchial glands or mediastinal abscess; for instance, we get several of these same symptoms in aneurysm of the aorta; moreover, we must exclude pleural thickening, pleural exudation, and pericardial effusion.

If the case has progressed to abscess formation with rupture into the mediastinum, we here also find the patient complaining of a feeling of constriction, the same dull pain behind the sternum, laryngeal irritation, nausea, cyanosis of the face, headache, vertigo, syncope, dulness upon percus-

sion, bulging of the intercostal spaces, and loss of respiration sounds.

With the formation of pus more fever is present, usually of an intermittent type, and associated with severe rigors.

If the pus penetrates the chest wall a fluctuating tumour results, located usually to the left of the sternum at the junction of the fourth rib. The use of the needle will frequently be demanded in arriving at a diagnosis; if pus is found a free incision is always indicated.

Hertz states, "The abscess seldom bursts into the pleural sac."

If one could be certain that the disease in the glands is due to other cause than tuberculous infection, a favourable prognosis might be given; but in the face of the fact that the majority of the cases are tuberculous the prognosis must always be guarded.

Pepper says that "doubtless some cases in which circumscribed foci of tuberculous disease have been present in the lymph glands recover entirely, and calcification of the gland prevents further spread; in other cases a fibrous capsule may limit the spread of the disease for a time; but as a rule the neighbouring structures become involved, disseminated tuberculosis results from entrance of the bacilli into the blood."

In the case of mediastinal abscess the prognosis will depend almost entirely upon the source and freedom of the drainage.

Draining into the bronchi would of course not be particularly alarming unless very profuse, but would result almost certainly in auto-infection of the lungs.

Drainage through the chest wall would promise best result; and where an opening is made the same should be free, even to resection of the rib if necessary.

The treatment does not differ from that indicated in similar diseased conditions in other lymphatic structures, depending always upon the ætiology of the disease.

Cod-liver oil, iron, iodine, and particularly the syrup of the iodide of iron, are remedies of value.

Good results have been and are still being obtained from the use of the serum products. Several cases so treated and reported in the '*Med. Record*,' Feb. 8th, 1896, by the writer are still well, and have had no relapse.

In the tuberculous cases where mediastinal abscess results, drainage is everything, and if good results are to be expected the same regard must be had to fresh air, good food, a hygienic life, and the avoidance of over-exertion as is required in all other cases of tuberculous disease.

The idea of this short paper has been, not to present any particular line of treatment for the relief of such morbid conditions, but to bring out the fact that certain vague and misleading symptoms are frequently caused by mediastinal disease. Especially are we prone, in cases of pulmonary disease, to refer and take for granted that certain symptoms are caused by intra-pulmonary morbid conditions, whereas in fact they not uncommonly are caused by such extraneous causes as mentioned.

Moreover, frequently in the examination of the chest the mediastinal space does not receive its due attention; and finally, the respiratory sounds as heard in auscultation may be normal while extensive and serious morbid conditions exist in the mediastinum, particularly in the bronchial glands.—*Charlotte Medical Journal*.

Puerperal Thrombosis and Embolism.—

Singer ('*Archiv f. Gynäkologie*,' Band lvi, Heft 1, 1898) reports thirty-five cases of embolism and thrombosis following childbirth, as recorded in the Dresden Clinic during the past four years. He states that Mahler, in 1894, published a paper upon this subject, in which he quoted the cases that had occurred in the Dresden Maternity Hospital. It was Mahler who first called attention to the fact that the pulse was the most important factor to note as the danger-signal of this very grave complication—much more important than the temperature. A gradual acceleration with a graded ascent of the sphygmographic wave out of all proportion to the temperature indicated an infection of the patient, with the probability of an approaching thrombosis. Gessner, who made a large number of investigations in the Berlin Maternity Hospital, also confirmed this statement of Mahler, and found the complication especially frequent in patients who presented carcinoma and fibroma of the uterus. In addition to this symptom there are other symptoms which are very constantly present. These include severe headache, lancinating pains along the course of the

veins of the lower extremities, slight dyspnoëic attacks, pains in the pleuræ, and occasionally, when pulmonary thrombosis occurs, a blowing sound over the pulmonary vessels. All of these symptoms are of the gravest import, and, should one or more of them appear, it is most important that the patient be kept absolutely quiet. Singer claims that this is the most alarming complication of labour. He has observed all of the symptoms noted by other authors, and like them lays special stress upon the importance to be attached to the pulse-wave. In about 90 per cent. of the reported cases the quickening of the pulse occurred and preceded the other manifestations. The frequency of the pulse-rate is to be ascribed not only to the infection that is present, but to an increased circulatory resistance. It is followed by a temporary rise of temperature, and the pulse remains accelerated even after the temperature has again dropped to normal. The lancinating, lightning-like pains in the legs is also an early symptom, which is greatly aggravated by moving about. The ætiology of puerperal thrombosis is mainly inflammatory, the inflammation preceding for some time the appearance of the symptoms. Bacteria have been demonstrated in the secretions in all the cases reported. In all the patients pelvic peritonitis, parametritis, foetid lochia, and ulceration of the vulva and vagina have been noted. In twelve cases the infecting germs have been gonococci, in three cases streptococci, and in three staphylococci. A mixed infection of gonococci and streptococci were noted in a few instances. In some of the cases in which germs could not be detected, it is probable that the infection was gonorrhœal, since the absence of the gonococcus does not exclude the possibility of gonorrhœal infection. There is also, as is well known, a great tendency for the gonococcal infection to spread beyond the uterine veins and to enter the general circulation, as has been very definitely demonstrated by the investigations of Wertheim. This writer has proved that infection by the gonococcus does not remain localised, as was formerly believed, but that it shows a marked tendency to spread to the surrounding organs by means of the lymph-channels and veins. This is a very strong argument in favour of the claim that every case of puerperal infection should not be ascribed to carelessness on the part of the attending accoucheur,

but that in very many cases the septic infection is, so to speak, a form of auto-infection, due to the presence of an unsuspected gonorrhœa which has antedated for an indefinite period the birth of the child, and which has been permitted to spread rapidly because of the favourable conditions produced by the puerperal state.

University Med. Magazine.

Bromoform Poisoning.—Dr. Müller, in the 'Münchener med. Wochenschrift,' September 20th, reports a case of a child, two years old, suffering from whooping-cough, who swallowed about four grains of bromoform during its mother's absence from the sick room. A few minutes later the child appeared intoxicated, and following the administration of lukewarm water and tickling of the pharynx it vomited. Soon it fell into a deep slumber, broken by repeated convulsions, beginning with respiratory spasm and cyanosis of the face. A copious supply of milk was at once administered. Two hours afterwards the child was in a state of collapse with scarcely perceptible breathing, cyanotic face, fluttering pulse, cold hands, contracted pupils with absence of reaction to light, and muscles paralysed. Death occurred in about five hours following the ingestion of the dose, with respiratory and cardiac paralysis. The characteristic clinical picture is that of asphyxia, undoubtedly caused by the action of the blood poisoned with bromoform upon the respiratory centre. The post-mortem examination revealed the following conditions:—The pupils equal and of medium size. The stomach contained some food particles which had a faint odour of bromoform. There were only a few hæmorrhagic spots and slight congestion of the mucous membrane without loss of the lining epithelium. The duodenum and upper half of the jejunum had a deep odour of bromoform; the mucous membrane was injected; the solitary glands and Peyer's patches were intensely swollen; the upper part of the jejunum was already reddened. Farther along the reddening became less marked, the outer coat was greyish, and the mucous membrane was pale. The liver was normal in size. The upper surface was bluish-red. There were a few hæmorrhagic spots in the left lobe. Cross-section revealed a glistening bluish red surface, from which there exuded a profuse, thin dark blood. The kidneys were large,

brownish red, glistening, and sanguineous. The thinness of the blood was a striking feature. Only in the right heart were there a few small clots. The left ventricle contained a small amount of thin dark blood. The epithelium of the epiglottis was reddened. There was a greyish-red mucus in the trachea. The injection of the mucous membrane was more marked lower down, and most marked at the bifurcation of the trachea. The tongue was pale. There were a few hæmorrhagic spots, about the size of a grain of mustard seed, on the left lower lobe of the lung under the pleura. The surface was dark bluish red, the cross-section was cherry-red. Upon pressure there exuded from the bronchi a copious whitish mucus. The mucous membrane of the bronchi was markedly injected. The veins of the dura contained very thin dark fluid blood. The vessels of the pia were sharply outlined, having been filled with blood of a similar character. They formed swellings, as if they were artificially injected with wax. The brain substance also showed numerous hæmorrhages, and was wet and succulent. Chemical examination of the different portions of the body revealed the presence of bromoform in various quantities. From the stomach, intestines, and their contents there were obtained 1.165 gms. of bromoform. This also was present in the blood and various organs, although not in measurable quantities. When it is considered that of all these organs only small portions were taken and examined, it must be concluded that bromoform had doubtless accumulated in the body in large quantities. The most important points in the case were the thinness of the blood, the dark red colour, and the unusually marked injection of the vessels of the brain and its meninges. Of less importance was the irritation of the mucous membrane of the small intestine. This being the only reported case of death from bromoform poisoning in which autopsy was made, we have given the post-mortem changes at some length.

Medical Record, October 22nd, 1898.

A Case of Blue Nasal Secretion.—At the annual Congress of the French Society of Otology and Laryngology, M. Molinié, of Marseilles, related the case of a young woman twenty-five years of age, in whom, after a severe attack of grippé,

accompanied by intestinal, gastric, and nervous symptoms, there occurred a discharge of blue secretion from the nose. In the beginning the discharge was generally viscous and colourless. Several times during the day, however, the mucus was streaked by lines of blue as deep as methylene blue. Examination of the nasal fosse demonstrated that the source of the secretion was the middle meatus of the right side. Bacteriological investigation revealed the presence of a short, squat bacillus with rounded extremities, coloured by methylene violet and gentian violet, and retaining its colour under the Gram reagent. Although cultures did not yield the characteristic blue colour, it is very probable that this case of blue chromorhinorrhœa was due to the development of a pyocyanic colony in the frontal sinus of the right side.

Revue Hebdomadaire de Laryngologie.

DR. SINGER showed at the Austrian Otological Society a case of "nystagmus following the radical operation." Certain mechanical stimuli produced the movements, *e.g.* forcible syringing, especially with cool water. Compression of the air in the meatus caused slow and short horizontal movements. When the region of the horizontal semicircular canal was touched with a probe the patient felt very dizzy, and made curious rotatory movements, and at the same time there was brisk rotatory nystagmus of both eyes. The pupils dilated during the nystagmus, but contracted again directly. Prof. Gruber remarked that nystagmus was not at all rare under the circumstances mentioned.—*Journal of Laryngology*, Nov., 1898.

STATISTICS show an exceptionally high mortality from chloroform anæsthesia in the operation for the removal of lymphoid hypertrophies of the pharynx. The observations of the Vienna pathologists show that sufferers from "adenoids" frequently belong to an abnormal constitutional type that has been found peculiarly susceptible to chloroform narcosis. In view of the statistical and pathological data presented, the general use of chloroform in the operation for hypertrophied tonsils or naso-pharyngeal adenoids is inadmissible.—HINKEL, *N. Y. Med. Journ.*, Oct. 29th, 1898.

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* Specially reported for *The Clinical Journal*. Revised by the Author.

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ON THE RELATIVE VALUE OF THE MALE AND FEMALE REPRODUCTIVE ORGANS IN THEIR RELATION TO OPERATIONS.*

BY

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F.R.C.S.Eng.,

Surgeon to Guy's Hospital, Consulting Surgeon to the Evelina Hospital for Children.

MR PRESIDENT AND GENTLEMEN,—When your retiring President, Dr. Bindley, insisted with some importunity that I should accept the honour of delivering the opening address of your Medical Session, there was left to me but three short weeks, at a busy season, in which to arrange my thoughts; and when he gave me the distinguished names of those who in the five preceding years had opened your session I felt still more the difficulty of doing adequate justice to the occasion.

Even the title which I suggested for the address did not convey to him any very definite ideas; but as an opening address from my point of view should be discursive rather than exact, and suggestive rather than dogmatic, the difficulty he experienced in understanding the drift of the title, and the points upon which it was my intention to touch, determined me, in accepting his courteous invitation, to retain the designation announced.

In speaking of the relative value of the male and female reproductive organs in their relation to operations, I would explain at once that I refer only to the ovaries and testes, and not to those subsidiary organs associated with the generative function, and essential for procreation, gestation, and parturition. It is to the fountain sources of generation, the sperm and germ secreting organs, to which my remarks will be confined. Either of these would furnish material sufficient for many

* An address delivered before the Willesden and District Medical Society, October 28th, 1898, at the opening of the Session.

addresses were I to refer in any detail to the various diseases to which the organs are liable, and to the surgical procedures that may be adopted for their relief. Such, however, is not my intention, but rather to refer to the marvellous influence exerted by these organs over the development of the individual, and on the maintenance or atrophy of certain sympathetically associated organs—an association or control which has led of late years to the removal of the reproductive organs for the cure of diseases situated in distant parts. It is in this newly opened “sphere of influence,” if I may adopt the modern political phrase, that we must tread with caution, and discuss the advantages to be obtained side by side with the risks to be incurred. It may be with certainty predicted that any advance founded on a sound scientific basis will stand the test of time, though its adoption may be gradual; but pioneers are liable to be led into indiscretion through their enthusiasm, and the small forces at their disposal are often scattered or exterminated by the prejudices of the aborigines. Many advances are made which eventually end in disaster, whilst others lead to retreat, only that, with the reinforcement of greater experience, more definite advantages may be obtained. I think you will understand from these preliminary remarks that the drift of my address will be towards what may be termed the “foreign policy” of the reproductive organs.

The wide influence which is exerted by the reproductive organs on the stature and physical and mental developments of the male child has been known for centuries, and the withdrawal of this influence has been made use of for various purposes, more especially among Eastern nations, where the operation of castration is performed in order to obtain suitable attendants to guard the harems.

When these organs are removed in the child the stature is dwarfed, the cellular tissue accumulates fat, the breasts become full, the voice at puberty fails to break and retains the feminine pitch, and the beard remains undeveloped. I have chanced in the course of a long clinical experience to meet with two or three of such individuals, in whom by accident, disease, or operation, both organs having been destroyed in infancy, the results were such as I have described. The operation is universally employed on domestic animals required for food—

oxen, sheep, and pigs, in order that they may be less trouble to pen and more easy to fatten for the market. In horses it is commonly practised to lessen their courage, and render them more tractable and more easily managed. The feminine type of the gelding is familiar to all.

The effects of castration on the adult are quite different; for the physical contour has been acquired, and will not recede. The beard will not fall off, nor can the frame or voice revert to a feminine type. Although in some cases there may be a tendency to melancholia developed, the mental development remains high, and the courage appears but little, if at all, affected. Eunuchs in former days have been leaders of armies, and have held high official positions in their states. I can speak with some personal knowledge of horses that have been left some years before being castrated; they gain a grander build, and, were it not for the expense of keeping them for this delayed operation, the advantages to an owner not afraid of “mettle” would be very great.

To what, then, are we to attribute this marvellous influence of the male reproductive organs upon the developing child? Is it due to some nervous influence of a reflex kind dependent on the primary exciting presence of such organs, or are we to regard these organs, even in their earliest development, as capable of supplying some internal secretion having the power of influencing the metabolism of almost every other organ? Certain it is that, to those who study them closely, male and female infants present differences in manner and character as well as form easily to be distinguished; and though these differences become greatly developed and exaggerated at the age of puberty, they are to be traced from the earliest infancy. Formerly, when the nervous system was little understood, and was regarded with a kind of superstitious reverence, everything difficult of explanation was referred to it. Even now, in spite of the great advances made in the differentiation of nervous diseases and in the localisation of cerebral centres, a certain mystification hangs about much that is written on the subject. Some years ago, soon after Charcot described the joint disease of the ataxic, it became the fashion to attribute all joint affections to the influence of the nervous system. Rheumatic fever and gonorrhœal rheumatism were thus explained. The discovery of the

gonococcus of Neisser delivered one of these diseases from the sphinx of the nervous system, and I am convinced that it will not be long before the microbe of rheumatic fever will be as demonstrable as that of tubercle or of anthrax.

The humoral view or internal secretion theory of the effect of these organs also presents difficulties, but it has gained a great impulse from the discovery of the uses of the thyroid gland, and the marvellous influence exerted in cases of myxœdema by the administration of this gland or its extract taken from the lower animals. Some will remember with what contempt the communication of the aged physiologist, Brown-Séquard, was received, when he published the account of the effect on himself of the testicular extract he had prepared. It was regarded as the drivelling outcome of a degenerate cerebrum; but not only analogy but further experience seems to show that the testes and the kidneys (which formerly were regarded merely as excretory organs) exert an influence apart from their excretory functions, which by many is believed to be due to their so-called internal secretion. There is yet an intermediate view which might be held, that these organs do not act on the development of the various organs—either immediately through their secretion, or by directly transmitted nervous influence, but by the effect of the internal secretion upon the central nervous system. The marvellous toxic effect of the drum-stick bacillus of tetanus upon the nervous system might be quoted in illustration of the effect on the nervous system of an agency introduced from without, whilst the normal internal secretion of an organ might be supposed to exert an altogether milder and beneficial stimulating influence on the general economy through its action on the central nervous system. If we look into an ancient pharmacopœia we see that physicians in olden times were in the habit of administering the organs of the lower animals for the cure of diseases; brains, testes, pancreas, kidneys, and livers were recommended for various ailments. Perhaps these blind empirics were not always wrong. I remember some fifteen or sixteen years ago, when pancreatic juice was being introduced, a physician, given to strong expressions, stigmatised its use as the giving of "advertising gallipots of pancreatic filth;" but the deptonising of foods outside the body is now

commonly practised for invalids, and the reintroduction of the extracts of various organs constitutes the fashionable pharmacy of the day.

The influence of the female reproductive organs on the growing infant we have little opportunity of studying in an experimental form, since the unsexed female has not proved herself of service to the state; but it is generally believed that a masculine development, narrowed pelvis, and tendency to the production of facial hair are associated with a weakened influence of these organs. The crowing hen of the poultry-yard is known not to lay eggs. Among horses, mares are occasionally noticed, built on the stallion type, with high crests, thick necks, bushy manes, &c.; such mares often have tushes or canine teeth in the lower jaw, which are usually only found in male horses, and these mares will seldom bear colts. A few years ago a friend who had a fine brougham mare, from which he wished to breed, sent her to be covered by various stallions without success. He told me of his difficulty, and asked me to look at the mare. I opened her mouth, and showed him that she had two fully developed tushes or canine teeth, which indicated her masculine tendency, and weakened capacity for reproduction. On the other hand, early abnormal stimulation of the ovaries will bring about precocious puberty. In the Clinical Society's 'Transactions' of 1884 I published a case of precocious puberty in a child of seven years associated with a sarcoma of the right ovary. This child menstruated at intervals, had firm breasts as large as oranges, a well-developed mons Veneris, and pubic hair an inch in length, so that she presented the appearance of a dwarfed woman. Removal of the tumour led to recession of the breasts and to a cessation of menstruation, but the pubic hair remained.

Having glanced at the influence of the reproductive organs on the growing child, we must consider for a moment their relation to adults, whose masculinity or effeminacy is fully established. That they are not essential to the life of either man or woman has been proved so fully, that it is scarcely necessary to allude again to the effects of their removal. It would rather seem that, having by their influence brought an individual to a certain pitch of development, their interest to him or to her becomes more detached, and less essential to his or her well-being. In other words, they now

belong not to the individual, but to the next generation. They more often bring the individual into notoriety than into renown. Courting often ends in the law courts, and generating in degeneracy. The individual is in the position of a trustee for the next generation, and very badly does he commonly carry out his trust. Were it not for the pleasure of transmitting the dividends, by what a large percentage would the trust be repudiated! But a certain pride is taken in the power of the individual over repetition, and the simple mathematics of multiplication and addition have a fascination for many. This pride in the power of procreation plumes the peacock's tail. Cast a reflection on a man's masculinity, and you make the most vindictive of enemies; and remember that Sarah of the Bible scoffed not only at her husband, but at her God.

If we now compare the relative procreative periods of the human male and female, that of the latter may be roughly stated as from twenty-five to thirty years—say from the age of eighteen to forty-five, though, of course, cases occasionally occur beyond the extremes of either limit mentioned, and in many the period is much shorter. In men the limit seems often only terminated with the end of life. I know a family where there was an interval of exactly fifty years between the age of the eldest son and that of the youngest half-brother. Only a few days since an old gentleman on the verge of eighty was in my consulting room boasting of his prowess, and complaining of the comparative coolness of his much younger wife.

From these facts it follows that the value of the reproductive organs in the female declines rapidly at the age of forty, and from a procreative standpoint become useless a few years later, whereas the corresponding organs in the male retain their functions to an indefinite period in the life of the individual.

How should these facts affect our practice in regard to operations on these organs? The idea of maternity, apart from any sensual thought, is very strongly impressed on the feminine mind—much more strongly than paternity in the mind of man; and it is our duty, within the limits of the patient's safety, to respect this feeling. "Sometimes to her sorrow," says a recent author, "a woman's work lies beyond her home. The duty of Mary was not to Joseph." Here, then, the

question of age might largely affect our practice. Supposing one ovary extensively diseased in a girl or young woman, say from fifteen to twenty-five years of age, and the other sound, no one would, I presume, in the present day think of removing both ovaries unless the disease were of a malignant character. Even if the second ovary showed some early evidence of simple degeneration, but yet in the main was sound and capable of discharging its functions (it may be only for a time), I should hesitate at this age, seeing the comparative safety with which such operations can now be performed, to cut off the patient absolutely from the next generation. Such practice might be greatly modified as the patient approached the age of forty or thereabouts, as the ovaries in their reproductive value have greatly declined, and the security of the health of the individual begins to outweigh her weakened chances of maternity. Some eighteen months ago I put forward this plea in some letters to the 'Lancet' "On the question of the second ovary in ovariectomy," which brought forward some rather conflicting opinions from other operators. The conservative surgery of the ovaries, which seemed to fall rather under a cloud with the decline and death of Sir Spencer Wells, is, I believe, again gaining ground. As showing the value of one ovary, I may mention that surgeon's statistics:—In 223 women who survived unilateral ovariectomy, and were under forty years of age, 120 gave birth to 230 children, or nearly two children to each woman.

Quite recently (January 29th, 1898) Professor Howard Kelly, of the Johns Hopkins Hospital, Baltimore, published in the 'British Medical Journal' a paper on the "Conservation of the Ovary in Hysterectomy," and he related therein twenty cases of operation on the uterus in which one or both ovaries were left. This is just the reverse operation to Battey's operation, in which the ovaries are sacrificed to reduce myofibromata of the uterus. Dr. Kelly's conservatism of the ovaries is made to rest not on any idea of procreation, which, of course, with the uterus removed is impossible, but upon the value of the ovaries on account of their supposed internal secretion in preventing the symptoms usually referred to a hastened menopause.

Now let us consider the operations that have been undertaken of recent years upon the repro-

ductive organs, with a view of influencing structures at a distance.

Batley's operation, or, as it has been paradoxically termed, "normal ovariectomy," or oöphorectomy, was originally proposed for the cure of nervous affections, which the ovaries were presumed to induce, or by some reflex influence to maintain. The normal ovaries were removed in order to cure these neuroses. The results have been altogether disappointing, and the operation has, I hope, for these cases been generally condemned and abandoned. Professor Hegar extended the operation for the relief of uterine fibro-myomata with the object of hastening the menopause and causing atrophy of the tumours. For these conditions it has been largely practised, and with beneficial results, though atrophy does not always follow; and since the clamp in hysterectomy has been generally abandoned, the removal of the diseased uterus is being practised with greater safety, so that removal of the whole disease may not improbably be more generally adopted in the future, and the ovaries, one or both, may possibly be left, as Professor Kelly suggests in the paper already alluded to. The latest development of Batley's operation is, however, only two years old. In May, 1896, before the Edinburgh Medico-Chirurgical Society, Dr. Beatson,* of Glasgow, proposed the removal of the ovaries with a view of checking the advance, or of curing cases of cancer too far advanced for further local operation—"inoperable cases," as he termed them. He argued that the epithelial cell-proliferation of lactation up to a certain point closely resembled cancer formation; but whereas in lactation the cells vacuolate and break down to form milk, in cancer they remain, infiltrate the surrounding structures, and spread to the lymphatics. He found in Australia there was a practice of spaying cows after calving to keep up the supply of milk, whereas in this country the influence of the ovary is got rid of by re-impregnation. He then conceived the possibility that if the ovarian influence were removed the cells of cancer might undergo vacuolation and fatty degeneration, as the secreting cells do in milk formation. He also administered thyroid extract to assist in the process.

In one case the tumour of the breast underwent

atrophy after the removal of the ovaries, and in another improvement took place. Since then Mr. Stanley Boyd* has published five cases, in two of which atrophy of the growth followed for a time. In neither of these women had menstruation ceased. In others no effect was produced where menstruation had previously ceased. In two cases where menstruation had ceased naturally during the progress of cancer, improvement had been noted, so that the weakening of the ovarian influence and the resulting menopause appear to bring about the improvement. Mr. Cheyne† in April of this year published two cases in which Beatson's suggestion was carried out for advanced cancer of the breast and glands. In the first, a woman of thirty-four, improvement took place for six months, then the growth again rapidly increased. In the second case, a woman aged thirty-three, with cancer of both breasts, no improvement followed oöphorectomy.

The latest reports are to the effect that in no case (not even in Beatson's first case) has the improvement ended in a complete cure, though in many, temporary wasting of the growth has been noticed to follow oöphorectomy. Extirpation of the ovaries has also been tried for the alleviation or cure of cancer of the uterus, but no benefit has accrued from such operations.

To return now to the male reproductive organs, I would remind you that the male urethra is complicated by a double function. It is common to both the urinary and generative functions, and the one is apt to interfere with the other. Stricture would be very uncommon were it not that the generative function exposes the urethra to contamination, which the urinary would never do. Then there is the prostate gland, which serves as a sphincter to the bladder, and at the same time is a most essential part of the generative function. Over this gland the testes exert a powerful developmental and permanent influence. If the testes be removed in infancy the gland fails to develop, and at any period of life removal of the testes leads to atrophy of the prostate gland. Professor J.W. White, of Philadelphia, in 1893 first proposed to make use of this physiological fact in cases of prostate hypertrophy, and Ramm of Christiania was the first to adopt the suggestion, his first case being

* Published in the 'British Medical Journal,' July 11th, 1896.

* 'British Medical Journal,' Oct. 9th, 1897.

† 'Lancet,' April 30th, 1898, p. 1191.

performed on April 3rd, 1893. The reproductive organs of the male have an active procreative life of, it may be, fifty or sixty odd years, being nearly double that of the procreative life of the corresponding organs in the female. Did the function cease at the same period as in the female we should scarcely know what hypertrophied prostate meant, for it is comparatively rare under the age of fifty. The activity of the testes is in some way responsible for the hypertrophy of the prostate, for removal of these organs almost invariably brings about shrinking of the enlargement. Most remarkable results have been obtained by this operation in many cases, but it must be remembered that the patients are already near their natural term of life, and their kidneys may be in an advanced stage of septic dilatation before consent is obtained for operation, so that considerable risks must be incurred. The idea of mutilation, and the contempt with which most men regard the eunuch, stand more in the way than anything else to the general adoption of this operation for very old men.

It must, however, be conceded that the urinary function at this period of life is much more essential to the individual than the generative, and the constant passage of catheters in prolonging life gradually reduces the individual to an almost intolerable form of existence.

As regards the results of the operation, White published in 1895* 111 cases. Rapid atrophy occurred in 87·2 per cent.; amelioration of symptoms in 83 per cent.; return to almost normal urinary life in 46·4 per cent., deaths 18 per cent. More recently McEwen† collected fifty-two cases; 82 per cent. were relieved, 7 per cent. showed no improvement, and 11 per cent. died.

If the cases of uræmia, following quickly upon the operation when performed too late, are taken into consideration, I think the apparent mortality would be higher. In the last two cases I have operated on I removed one testis before the other, in order to avoid the possible shock of suddenly removing the influence of both testes together. One of these cases completely recovered the control of his bladder, and it was interesting to note the shrinking of the side of the prostate on the side corresponding to the first testis removed,

before I proceeded to remove the second. The second case died of uræmia before I removed the second organ.

An alternate suggestion has been made by Mr. Reginald Harrison to tie or excise part of the vas deferens in order to bring about atrophy of the prostate. It must be admitted that the results obtained in this way are far less remarkable, and in some cases it seems to bring about no atrophy of the testis. An old man of seventy-five, in whom I removed an inch and a half of each vas, came up a year after with no atrophy of the testes, and told us, to the surprise of all who heard him, that he was capable of connection once a week, and oftener if he wished. This case, in which I dissected each vas clear of other structures, seems to suggest that ligature of the neighbouring artery of the testis with the vas is the cause of the atrophy, and not the division of the vas itself.

To sum up my argument :

1. The male and female reproductive organs, originally allied in development, exert a remarkable influence on the development of the individual apart from their procreative function.
2. Maturity having been obtained, the procreative function outweighs in importance the nutritive influence exerted on the various tissues of the body through these organs.
3. From a national as well as domestic standpoint, the procreative function of woman is her richest dowry; so that in a young woman, whenever possible, it should be preserved to her by conservation of an active ovary.
4. With the approach of middle life the ovaries decrease in value, and may be more freely sacrificed to prolong the life of the individual.
5. Both ovaries and testes exert throughout life a certain influence on associated organs, and perhaps to a less extent on all the organs of the body.
6. This influence, formerly attributed to reflex nerve action, by many is now thought to be due to an internal secretion from these organs.
7. The removal of normal ovaries (Battey's operation) has failed as a cure for neuroses, and cannot be too strongly condemned for such cases, especially in young subjects.
8. The same operation for fibro-myomata of the uterus has often proved of great service in reducing the tumours by hastening the menopause.

* 'Annals of Surgery,' July, 1895.

† 'British Medical Journal,' vol. ii, 1896.

9. Kelly, impressed by the importance of the internal secretion theory, has lately recommended conservation of one or both ovaries when the uterus is removed, which is a reaction into somersault on Battey's operation.

10. Battey's operation has lately been suggested by Beatson for the cure of inoperable cases of cancer. Remarkable shrinking of the tumours of breast and glands has been noticed to follow the operation in women who had not reached the menopause, but complete cure seems never to have been obtained.

11. The prostate has both a generative and a urinary function. Late in life the urinary is increasingly more essential than the generative.

12. The testes throughout life exert a remarkable influence on the development of the prostate, so that their removal at any period leads to atrophy of the organ.

13. To make use of this influence for the cure of enlarged prostate was suggested by J. W. White in 1893, and has been performed by many surgeons since. In a large proportion of cases a great amelioration of symptoms follows.

STUDYING carefully a hundred cases of brain tumour in which an ophthalmoscopic examination had been made for the presence or absence of choked disc (optic neuritis), Dr. Krauss announced the following conclusions :

1. Optic neuritis is present in about 90 per cent. of all cases of brain tumour.

2. It is more often present in cerebral than in cerebellar cases.

3. The location of the tumour exerts little influence over the appearance of the papillitis.

4. The size and nature of the tumour exert but little influence over the production of the papillitis.

5. Tumours of slow growth are less liable to be accompanied with optic neuritis than those of rapid growth.

6. It is probable that unilateral choked disc is indicative of disease in the hemisphere corresponding to the eye involved.

7. It is doubtful whether increased intra-cranial pressure is solely and alone responsible for the production of an optic neuritis in cases of brain tumour.—*N. Y. Med. Journ.*, Oct. 22nd, 1898.

CYSTITIS: ITS CAUSATION, DIAGNOSIS, AND TREATMENT.

BY

CAMPBELL WILLIAMS, F.R.C.S.

(Concluded from Vol. xii, p. 454.)

CHRONIC CYSTITIS is the more common phase of the complaint. It is almost certain to make its appearance sooner or later in cases of enlarged prostate, urethral stricture, or when a stone or foreign body is present in the bladder. Other causes that may be cited are the entrance of faecal matter through a vesico-intestinal fistula, abscess draining into the viscus, ulceration of the bladder wall either of a tuberculous or malignant nature, as a sequel of acute inflammation from any cause, from the loss of muscular power from atony, from paralysis in connection with the general nervous system, and from gout. The symptoms of chronic cystitis are practically those of the acute type, only modified. They may be divided into local or vesical, and constitutional, both being governed as to intensity by the effects of the inflammation and the condition of the contained urine. In many cases the constitutional disturbance may be trifling, urinary fever being absent, the pain slight, and the frequency of micturition not sufficient to wear out the patient from want of rest, whilst the urine is high-coloured, with an excess of mucus, and of unpleasant odour. On the other hand, the pain may be severe—in fact, intense, the calls to urinate being so frequent and distressing that they soon exhaust the patient, whilst the septic absorption from the putrid urine by the production of a high temperature quickly prostrates the sufferer. The febrile condition in chronic cystitis varies necessarily with the degree of putrefaction of the urine and the amount of toxic absorption that takes place into the system. But when severe rigors occur, separated by short intervals, one commences to suspect implication of the renal substance and the formation of either interstitial or subcapsular foci of suppuration, or of a perinephritic abscess; more especially so if such occurs when the bladder has reacted considerably to treatment, and the condition of the patient becomes quite out of proportion to what one would expect from an examination of his

urine. The cause of this occurrence may be due to the presence of a saccule of the bladder which escapes our cleansing efforts, and will not empty itself of the small amount of foul urine mixed with exfoliated epithelium that it contains. This infection of the kidney may take place either by direct extension along the mucous membrane of the ureter to the renal pelvis, or by absorption and transit by the lymphatics which travel along the ureters from the bladder to the kidney. Another danger is that ulceration of its walls may ensue with extravasation of pyogenic material into the general peritoneal cavity, causing diffuse septic peritonitis and death. The amount of anorexia present depends upon the febrile state, which in its turn is in ratio to the rate or amount of septic absorption that is taking place from the bladder. The state of the *tongue* is a good guide to the degree of sepsis present and to the condition of the appetite. This organ may simply be coated with a little white fur in the slighter degrees of cystitis; but when septicæmia is in evidence it may be dry, covered with brown fur, the tip ham-coloured and glazed, and the dorsum fissured. This, together with the unpleasant breath, and the parched lips covered with sordes, would tell us, apart from the emaciated and exhausted condition of the patient, that unless the septic absorption can be checked we must ere long expect collapse and coma that herald the advent of a speedy dissolution. The recuperative powers of the subjects of chronic cystitis, from the fact that they are so frequently advanced in years, are not great. The patients often not only "run down" very quickly under slight septic conditions, but also fail to rally.

The amount of fluid that an inflamed bladder can contain is regulated either by the degree of irritability present or by the capacity of the viscus. When the urine commences to collect, and by distension of the tender walls sets up pain, the patient is *urgently* impelled to micturate. The fluid is ejected more or less forcibly in a *spasmodic* manner, and great relief is experienced with its evacuation. A persistence or intensification of pain *after* the bladder has been emptied or towards the end of the act is strongly suggestive of the presence of a calculus, ulcer, or new growth within the viscus. Frequency of micturition occurring from a bladder which either does *not*

contain residual urine, or whose actual capacity *has not* been sensibly diminished by hypertrophy, and therefore *not* due to a true decrease in the containing capability of the organ, will be found to be in relation to either the alkaline state of the urine, the pain, the inflammation, or the readiness with which reflex action is set up. On the other hand, frequency of micturition may be due to an actual reduction in the vesical capacity, and that true distension of the bladder is caused by quite small quantities of fluid.

The state of the urine in a typical case of chronic cystitis is one of alkalinity and ammoniacal decomposition. If a portion be allowed to stand in a measure-glass it will be seen to separate into three layers: the uppermost is slightly turbid, that is, it is not quite clear; the middle stratum is thickish and cloudy, whilst the lowest is an opaque *quasi*-solid mass consisting of ropy, slimy, tenacious mucus mixed with pus and epithelium cells, together with abundant crystals of triple phosphates. If this fluid be poured from one receiver to another, the bottom layer will be seen to cling to the glass in a very characteristic manner. The alkalinity is due to the formation of carbonate of ammonium from the decomposition of the urea, from the fermentative action upon it of the micro-organisms present. If a drop of the urine be placed on a glass slide, allowed to dry, and then stained with 2 per cent. solution of methyl blue, it will be found to be teeming with organisms, and a rod-shaped body—the *Bacterium termo*—will be plainly visible, together with micrococci. It is to a yeast-like action of the *Bacteria termo* that the urinary decomposition has been attributed, and in favour of this theory it may be mentioned that the sweetening of the urine is coincidental with the disappearance of this organism of putrefaction.

It is needless to say when chronic cystitis is present in a bladder that contains residual urine, owing to a prostatic pouch caused by an hypertrophied prostate, that unless one can manage either by continuous drainage or by repeated emptying of the static urine by frequent catheterisation to remove the congenial medium for the development of the organism, that one will not readily if ever ameliorate the condition. When once under these conditions a patient has entered on "the catheter life," it must be continued until "the end of the chapter," or a repetition of cystitis

will quickly ensue, and the urine will relapse into its former foetid and irritating state.

The treatment of chronic cystitis is primarily directed to the subdual of the inflammation, and the prevention of septicæmia by producing an alteration in the state of the urine. The correction of the ammoniacal condition, which is a direct irritant to the bladder's mucous membrane, necessarily carries with it a germal destruction which leads to this chemical production. The most direct way to attain this end is by local treatment of the infected area by antiseptic or germicidal lotions, and by drainage of the bladder to lessen the amount of decomposing fluid. In severe cases, and more especially those complicated with urethral stricture, a most excellent plan is to combine frequent irrigation with continuous drainage. This course of treatment necessitates rest in bed, but as a rule the patient is in such a state that rest in bed is not only necessary, but voluntary. Continuous drainage consists in tying in a soft or flexible catheter, so that its tip is within the bladder. To the free end of the instrument a length of india-rubber tubing is attached. This must be long enough to reach a chamber-pot beneath the bed. A leaden weight is fixed to the open end of the tube, so as to anchor it within the receptacle, and also to keep it sunk beneath the carbolic lotion which it should contain. A cage supports the bedclothes and keeps them clear of the instrument. The presence of the catheter resting on the trigone may cause some irritation at first, but this soon wears off, especially if the tip of the instrument be withdrawn from the bladder for a few minutes at a time. The *modus operandi* is multiple, for it not only dilates the stricture, but by keeping the bladder empty gives the patient rest from the act of micturition with its attendant pain, allowing him to sleep, and also by drainage of the decomposing and irritating fluid reduces not only the inflammation, but the septic symptoms. Care must be paid to the catheter lest it become cracked or encrusted with phosphates. It should be taken out and cleansed twice daily, and if it becomes rough or cracked it should be replaced by a new one. The bladder should be washed out at least thrice daily, and a little iodoform emulsion thrown in at night. Though most of this drains away, a little adheres to the bladder. It is noteworthy how much the patient's state may improve

after a few hours' employment of this method of treatment, the temperature falling, the pain ceasing, and sleep ensuing. As in all other surgical states referable to septicæmia which are relieved by drainage, so in this. With the withdrawal of the toxic element the patient's general condition improves, the facial and dermal aspects alter, the tongue cleans, and a gradual return towards health ensues. It is certainly a most useful procedure when there is either residual stasis, loss of expulsive power, or sacculation of the bladder, for it practically ensures to a greater or lesser extent a continuous curative action on the bladder during those hours that the doctor is absent from his patient. But I will give a word of caution *against* employing this method should the *kidneys be known to be markedly diseased and renal insufficiency well established*, for I have seen very serious results, varying from temporary to permanent suppression of urine, follow its application.

As regards the choice of a solution for vesical irrigation, one has a great number to pick from, such as saturated solution of boracic acid, quinine dissolved in sulphuric acid, izal, nitric acid, solutions of nitrate of silver and of perchloride of mercury, iodine, and permanganate of potash or zinc.

The quinine and sulphuric acid lotion has not only a germicidal action, but the sulphuric acid exerts a slight soluble action on the phosphatic encrustations on the bladder walls should they be present. The same applies to weak solutions of nitric acid (mj to 3j). It is in chronic cystitis that we get such good results from the use of mercurial solutions. Washing out a foul bladder with a lotion of the strength of $\frac{1}{5000}$ will rapidly sweeten it. In the slighter form of chronic cystitis, when the urine has never deviated much beyond containing an excess of mucus, a few pus corpuscles, and having a faint odour, or when it has been brought to that state by treatment, the astringent action of the solutions of silver nitrate are most effective. When mucus is ropy and tenacious it will be found difficult to clear away, for it not only adheres to the bladder, but occasionally causes blockage of the eye of the catheter, or even the urethra. Irrigation with a weak alkaline lotion of bicarbonate and bichlorate of soda (of each 10 grs. to 3j) will materially assist in getting rid of it; and unless the bladder is thoroughly cleansed of this medium

for the development of the germs of putrefaction it will be impossible to implant a healthy state within the viscus.

The procedure for relief of cystitis due to the presence of a foreign body such as a stone, or some article that has gained access to the cavity of the bladder, must be of an evacuative nature; and whether we employ extractive, cutting, or crushing methods to that end must be decided by the nature of the substance, sex of the patient, and exigencies of the case. Should chronic cystitis be due to a continuous or intermittent discharge of fæces from the intestine into the bladder, we are confronted with the necessity of not only making a diagnosis as to the possible cause and nature of the fistula, but also of suggesting an operation that may cut off the supply of fæcal matter.

Vesico-intestinal fistula may result from dysenteric ulceration of the bowel, forming a localised abscess, which eventually discharges itself into the bladder, and leaves a patent and permanent track by which flatus and fæces pass, or the fistula may be of a tuberculous or malignant origin. The question arises, What is the best operation to be undertaken in these cases? The courses open are either to perform an inguinal or lumbar colotomy, so as to cut off the passage of the fæces by getting the artificial anus *above* the junction of the bowel and the bladder; or, having opened the abdomen, to seek for the lesion between the bladder and bowel, and if possible to separate them and to close the opening in the bladder, and to effect a similar object in the intestine either by resection of a segment of the gut or by simply suturing up the rent without excising the indurated edges of the opening.

Needless to say, the foregoing procedure is a difficult and severe one, and only likely to be successful in the absence of malignant disease. One has not only to separate the bowel which is matted to the surrounding structures by dense inflammatory tissue, but if one manages to do so the bladder must be rendered water-tight and the bowel impervious to the leakage of fæcal matter. On the other hand, a colotomy may or may not be a quick and easy operation, devoid of danger to the patient, and accomplishing the object for which it was undertaken.

Should chronic cystitis be due to the presence of a tumour of the bladder, as proved by clinical

signs, microscopical evidence of the fragments of the growth in the urine, or by cystoscopic examination, one would again have recourse to operation for its cure. The most useful operation of suprapubic cystotomy is the procedure that deservedly finds most favour. It is equally efficacious in the treatment of tuberculous ulceration of the bladder, the removal under certain circumstances of free foreign bodies as well as encysted ones, excision of certain forms of enlarged middle prostatic lobe, especially when this is pedunculated and acts like a ball stopcock, as well as for the extraction of tumours. The excision which is sometimes practicable of an undoubted malignant tumour when it has not infiltrated the bladder wall gives a respite, if not a reprieve, and although one knows that it is impossible to say how long the vesical amelioration may last, nevertheless it is justifiable to urge the patient to submit to operation if only for the relief that drainage and sweetening of the bladder afford. But there is one point to bear in mind, and that is that the growth shall not have infiltrated the anterior wall of the organ through which one must incise. The chronic cystitis that is met with in conjunction with tuberculous ulceration of the bladder is not only easily dealt with after suprapubic cystotomy, but the continuous drainage that ensues gives rest to the organ, and this, together with the topical treatment, may lead to complete healing of the ulcers. Such I have seen happen in tuberculous bladders that have resisted all treatment *per urethram*.

Let us now consider the special treatment, if such it can be called, of chronic cystitis due to obstruction. Should a urethral stricture be the bar to free micturition it should be rendered patent. I have already mentioned continuous drainage, which also embraces continuous dilatation of the stricture. We have also the intermittent, rapid, and forcible dilatation to fall back upon. I only mention the last method—namely, forcible dilatation—to condemn its employment. It is a "hazardous proceeding" for the patient.

Operative interference may necessitate the resort to external urethrotomy, and according to the requirements of the case one can pick a Syme, Wheelhouse, or a Cock, the last named being seldom requisite.

An intimate surgical connection with one of the apostles of *internal* urethrotomy convinced me

that when there is a question as to renal insufficiency, or when a catarrhal or septic condition of the bladder demands a free access to the viscus, *external* urethrotomy is much less liable to be followed by reflex renal symptoms, and that it is preferable to *internal*. Some surgeons advocate as a routine practice that an instrument should be tied in after the internal division of a stricture, whilst, on the other hand, it has been urged that no instrument should be passed until the tenth day after the operation. Since urethrotomy in these cases is undertaken to a great extent as a means for gaining access for vesical disinfection, one is confronted with the question, which will irritate the lesser—the continuous presence of the instrument on the incised structure, or the repeated passage of a catheter over it? External urethrotomy solves this difficulty of itself, for not only does it ensure vesical drainage and admission, but it obviates the risk of septic material being driven by a *vis a tergo* into the cut tissues from the *internal* urethrotomy incision, which apart from causing a febrile state may lead to the formation of a periurethral abscess.

Chronic cystitis in conjunction with hypertrophic enlargement of the prostate is frequently met with. Here we not only have to deal with an obstruction to the outlet of urine or the ingress of an instrument, but also a greater or lesser amount of residual urine which cannot be expelled from the bladder by nature's efforts. Unless this residual collection be controlled by either intermittent or continuous drainage, so that it does not remain in the bladder sufficiently long to undergo putrefaction, one cannot hope for great success in ameliorating the cystitis. The means at our command for attaining this end are mechanical, that is regular and efficient catheterisation, and operative interference. The latter method comprises various procedures, but the end in view of all of them is a removal of the obstruction, either directly by excision or indirectly by producing a shrinkage in the offending organ. Excision of the middle lobe of the prostate is in a small proportion of cases eminently satisfactory, but one cannot be positive as to the practicability of the operation until the conditions that exist are visible to the eye after a supra-pubic cystotomy.

It may so happen that the removal of the obstructive condition only requires the snipping of

a pedicle, or that by cutting the mucous membrane over the enlargement and reflecting it from its surface the lobe can be shelled or scooped out. Again, by passing an *écraseur* along the urethra it may be encompassed and thus removed. This method has been invoked when the enlargement has extended in a ridge-like formation into the prostatic uretera. But, on the other hand, the diffusion may be so great, from lateral extension, that one would hesitate to proceed with removal if only for fear of the copious hæmorrhage that would ensue.

Shrinkage of the prostate has been brought about by doing a perinæal section and subsequent prolonged drainage with a tube. This by continuous and lengthy pressure causes a subsidence in the size of the gland.

But the more recent method is one that has been advocated by Mansell Moullin, namely, castration. It is well known that the so-called fibroids of the uterus undergo retrogressive changes after ovariectomy, and that the myo-fibromatous tissue atrophies subsequently to oöphorectomy. Arguing that the prostate in man is the analogue of the uterus in the woman, and the relationship of these organs to the testes and ovaries is similar, together with the histological resemblance between the uterine and prostatic tumours, has led to the operation of castration for the relief of the vesical troubles attendant on an enlarged prostate. For it is admitted that shrinkage of an hypertrophied prostate does follow excision of the testicles. But in urging such an operation it must be borne in mind that the mental condition is sometimes profoundly altered after emasculation. To obviate or minimise the possibility of this occurrence, as well as to substitute a less severe operation, recourse has been had to excision of a portion of each vas deferens. This operation has been practised to produce atrophy of the testicle for the amelioration of sexual mania in which masturbation figured prominently. Presumably the *modus operandi* on the prostate is secondary to testicular atrophy, and its effects, although similar in the long run to castration, are gradual rather than abrupt.

Treatment by mouth.—The administration of such drugs as are beneficial in acute cystitis will be found suitable in the *chronic* form of the complaint, and I will therefore confine myself to a

recapitulation of the same. The medicinal agents in vogue are those which are considered to have a sedative influence on the bladder, such as hyoscyamus, uvæ—ursi, buchu, hordeum, Triticum repens, and pareira, or those which exercise a germicidal action on the decomposing urine like lactic acid, benzoic acid or its compounds, salol, salicin, eucalyptus, quinine, and urotropin. For the benzoic group it may be claimed that like copaiba, sandal-wood, and cubebs, they exert a sedative as well as a germicidal action. Lastly we come to the alkalies and acids, and those preparations such as piperazine, colchicum, and urocedin, which produce an alteration in the quality or quantity of the excreted chemical constituents of the urine.

The question often arises in chronic cystitis when asthenia is supervening as to the advisability of ordering a stimulant in the form of alcohol. For although beneficial results, dual in character when sepsis is present, may be obtained from diffusible stimulants such as ammonia and bark or effervescing quinine mixture, nevertheless one often has to decide as to whether alcohol is not imperatively necessary to combat the progressive weakness. In such cases, notwithstanding that spirit acts as a strong irritant to the vesical mucous membrane, the *harm* produced by their imbibition is quite outbalanced by the *good* effects produced on the general asthenic condition. But the use must be restricted to the lowest possible quantity compatible with efficient action, and for a time at least a stimulatory *régime* must take precedence of all other considerations.

Hypodermic injections of strychnia and the inhalation of oxygen may be necessary adjuvants, more particularly when renal insufficiency is present. The oxygen is most useful when uræmic symptoms develop, and acts as a cardiac tonic as well as having a general circulatory effect.

In conclusion I will refer briefly to cystoscopic examination of the bladder. This method may be employed either for making a diagnosis in an obscure case, or to confirm one already determined by clinical and microscopical evidence. The approximate localisation of vesical tumours, and to a certain extent their nature, can be settled by the aid of the instrument known as the "cystoscope." One may also be fortunate enough to gain definite visual evidence of the presence of an encysted

calculus, tuberculous ulceration, or even of a purulent discharge from the ureters into the bladder. The form of instrument with which I am best acquainted is that made by Leiter of Vienna. This is so designed that the bladder can be kept distended by a continuous flow of *clear* fluid so as to facilitate examination, which is often rendered difficult owing to a small amount of blood producing opacity and obscuring the view.

Lumbar Pain and Sciatica.—In the 'Deutsche Medicinische Wochenschrift' of June 9th, 1898, Minor gives the result of his observations regarding certain movements of patients suffering with lumbar pain, and contrasts them with those of patients afflicted with sciatica. According to the author, individuals suffering with contusion of the muscles of the lumbar region, caries vertebrarum, or lumbago from any cause, perform practically the same series of movements associated with pseudo-muscular hypertrophy. In getting up from the floor a patient with bilateral lumbar pain first turns over in the all-fours position, and raises the trunk with his arms. The hands are then moved along the floor until the knees are reached; then, with one hand upon a knee, he lifts himself, grasps the other knee, and gradually pushes himself to the erect posture by climbing up his legs. Bramwell and Oppenheim have both drawn attention to the fact that the above movements were not pathognomonic of pseudo-hypertrophia musculorum, but that they were also observed in inflammatory processes involving the vertebræ, or any condition of the lumbar muscles accompanied by pain on attempting to assume the erect position. The author adds that the above is true only when lumbar pain is present on both sides. A patient with sciatica proceeds in an altogether different manner. In the majority of cases he first stretches out his sound leg, while the affected limb remains slightly bent at the knee. In this position the patient steadies himself by placing both hands on the floor behind him. He then swings his pelvis backward between his arms acting as supports until the feet approximate his centre of gravity. Then by a series of movements the trunk is slowly elevated; one hand remains for a short time on the floor, the other swings in the air to establish the equipoise, the knees straighten, and the patient finally stands on his feet.—*Medicine*, October, 1898.

CONSTIPATION AND ITS MODERN TREATMENT.

BY
GEORGE HERSCHELL, M.D.Lond.

(Continued from p. 46.)

Treatment.

Having now by the investigations that we have made arrived at a clear idea of the nature of the case, and of the cause of the constipation, we are in a position to initiate a treatment with some hope of success.

The chief indications for such a treatment are the following :

(a) *The Removal of all Retained Faecal Material from the Intestine.*

The very first thing to do when commencing the treatment of a case of constipation is to make absolutely certain that there is no faecal matter abnormally retained. In some cases you will have ascertained its presence by the different methods of examination described ; in others there will be symptoms present which warrant you in suspecting it, but in others both of these will be absent. But in the first two instances you will certainly feel no hesitation in washing out the bowels, but in the last the question arises whether you may assume that the bowels contain nothing abnormal, or whether you should make sure by one or two large enemata. In my opinion you should certainly assume that there may possibly be faecal retention, and act accordingly for the following reasons :

It is extremely unlikely that habitual constipation can have existed for any considerable length of time without some faecal masses being abnormally retained ; many faecal masses cannot be discovered on a physical examination of the abdomen ; chronic faecal retention does not invariably give rise to symptoms of any kind ; you certainly cannot cure the constipation as long as there are any faecal masses permanently retained in the intestine, and the treatment for their removal can do no possible harm to the patient. You will therefore commence your treatment by making certain that the large intestine is empty, and thus have a fair start in the management of the case, with the

definite knowledge that you have eliminated a possible factor.

You may find one or more of the following four conditions present, and your measures will vary accordingly :

1. Faecal masses may be felt by an examination *per anum* blocking the rectum.
2. Masses may be made out by palpation in the sigmoid flexure.
3. Masses may be felt in the remainder of the colon or in the caecum.
4. There may be no physical evidence at all of anything abnormally retained.

Any one or more of the first three conditions may be present in conjunction. I shall take them in order, and point out the treatment to be pursued in each case.

1. *When faecal masses can be felt in the rectum.*—The rectum must be emptied before you can do anything more in the treatment, as obviously until this has been done no fluid can be introduced into the upper parts of the colon. In many cases a simple enema of hot soap and water will be all that is required, but in others it proves of no avail, and it is necessary to use some agent which has a solvent action. Two substances are of especial value for this purpose — olive oil and ox-gall. Olive oil consists mainly of oleic acid, which is a powerful solvent of faeces, and ox-gall acts in the same manner, but more energetically. A combination of the two forms one of the most efficient solvents of faecal matter that it is possible to conceive. I may add that for a knowledge of this valuable property I am indebted to Mr. William Allingham, who communicated it to me over twenty years ago. It is quite possible that the rectum may be so tightly packed that there may be very little room for any injection to be introduced. Under these circumstances three ounces of the assafoetida enema of the B. P. mixed with an ounce of fresh ox-gall or with a drachm of the purified dried preparation may be used every two hours until an effect is produced. Another method is to introduce a mixture of warm olive oil and ox-gall every night at bedtime, and allow it to remain up all night. In the morning a large enema of hot soap and water will usually empty the rectum. If, however, the symptoms are urgent, and it is necessary to empty the rectum as soon as possible, continuous irrigation with the double tube is the

best course of action, and I have designed a special piece of apparatus for this purpose. A large vulcanite tube long enough to pass beyond the internal sphincter, terminates at the end which is to be introduced into the patient in a large opening with thick rounded edges, and to the other is attached a piece of rubber tube leading into a pail placed upon the floor, or a Kelly's douching sheet may be used. A small orifice is made in the rubber tube about an inch from its junction with the vulcanite rectal one, and through this is passed into the bowel a soft rubber stomach-tube, with preferably a terminal eye. This in its turn is attached to a douche-can hanging at a convenient distance above the patient. The effect is that the water enters the bowel in a small forcible

as just described, but masses can be felt in the sigmoid flexure.—Enemata of soap and water, olive oil and ox-gall are to be tried as above, but the technic of their administration is a little different from that used when it is only desired to empty the rectum. Then an ordinary Higginson's syringe could be used, now we must make use of a douche-can raised about a couple of feet above the patient. If these prove ineffectual to empty the bowel, continuous irrigation as in the case of the impacted rectum must be used. A Turck's double colon tube is introduced into the sigmoid flexure, an operation not at all difficult of execution; one tube is attached to a douche-can two feet above the patient, and the other one to a rubber pipe leading to the receptacle under the bed. The patient lies

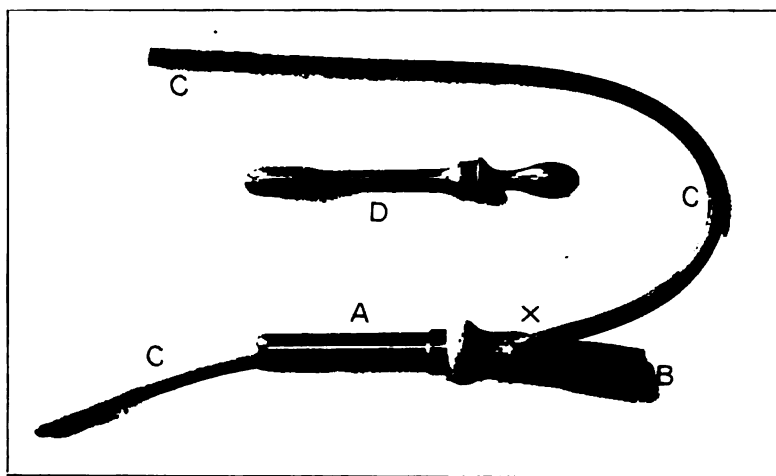


Fig. 1.—A. Vulcanite tube. B. Rubber tube. C. Soft rubber tube. D. Obturator.

stream, and the *débris* leaves it again almost at once by the wider vulcanite one, carrying with it bit by bit the fæcal masses which undergo continuous disintegration. It is convenient to introduce the large vulcanite tube before the rubber tube is attached, as this permits the use of an obturator, and materially facilitates the operation. Such an apparatus may be easily extemporised from any piece of tube of the required diameter, but one must remember on no account to use a glass one, as it might possibly be crushed by spasmodic action of the sphincter and seriously injure the patient. The apparatus is shown in Figs. 1 and 2.

2. *When the rectum is empty or has been emptied*

on his right side. This irrigation of the bowel may be kept up for an hour without the slightest damage to the patient. There is not the slightest doubt but that we owe a deep debt of gratitude to Professor Turck for placing within our reach this method of irrigating continuously the large bowel. By its aid the treatment of the various inflammatory affections of the colon is much simplified, and we are enabled to remove fæcal accumulations which were hitherto beyond our aid.

3. *Masses may be felt in the transverse or ascending colon or cæcum.*

4. *The large bowel is apparently empty.*

Our procedure in both these cases will be the same. Our object is to completely clear out any

possible retained fecal matters from the whole of the large bowel. This is accomplished by the administration on several days in succession of a douche of hot water containing a little boric acid or other harmless disinfectant. Latterly I have been using a normal salt solution. Enemata of the ordinary size (a pint or two) are useless for this purpose, as it is tolerably obvious that to reach, for example, a fecal mass in the cæcum, the whole of the large intestine must be filled with liquid. Into a normal adult healthy large bowel half a gallon of water can be injected without materially

you produce in the bowel infallibly give rise to expulsive efforts long before a sufficient quantity of water has been introduced, but you are absolutely unable to regulate the exact pressure which you are using.

2. The pressure under which the water passes into the intestine must be a low one. Anything above two pounds to the square inch in an adult will be dangerous, much less to a child. As the amount of pressure actually exerted in the bowel depends not only upon the height of the column of water (*i. e.* the distance of the can above the

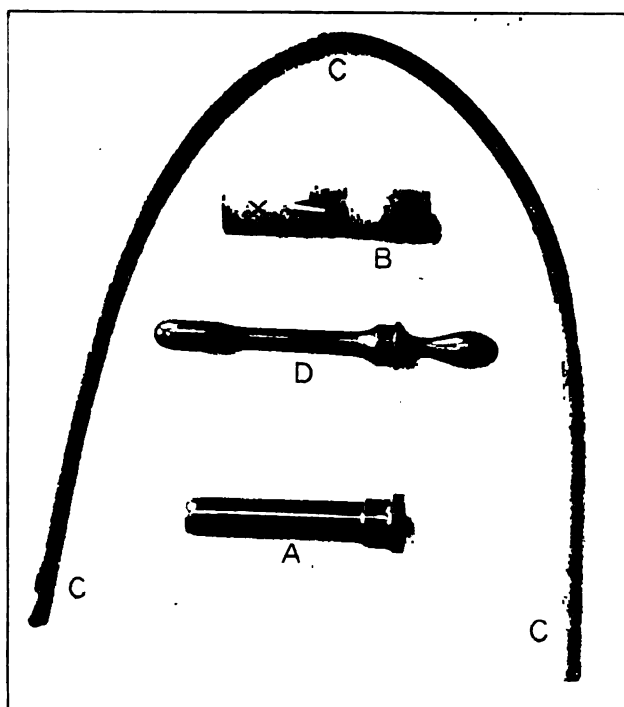


Fig. 2.—A. Vulcanite tube. B. Rubber tube. C. Soft rubber tube. D. Obturator.

stretching it, and much more is required to fill the dilated colons which we so frequently meet with in cases of long-standing constipation.

The problem, then, is to introduce a sufficient quantity of liquid without exciting the propulsive efforts of the patient. This can be accomplished by attention to the following points of detail:

1. A continuous pressure must be used, not an intermittent one. You can do this by means of a douche-can suspended at an appropriate distance above the patient. With a Higginson syringe not only will the rhythmic alterations of pressure that

patient), but also upon the resistance which is met with in the bowel, it is of great practical assistance to include a mercurial manometer in the circuit—that is if you wish to do your work scientifically and intelligently, and to know exactly what is taking place inside your patient. By this means you know the exact moment when the fluid, ceasing to simply flow into the intestine, commences to distend it.

3. The temperature of the water must be about 100° F. If higher or lower the patient will suffer unnecessary pain.

4. The force of gravity must be called in to assist the passage of the liquid towards the cæcum. The patient must be placed in the knee-elbow position or on his back, with the hips raised. Personally, for the latter purpose I use a special apparatus, but it can also very well be managed by raising the foot of the bed on blocks.

5. It is a distinct advantage to place the flat of the disengaged hand upon the abdomen of the patient, as by this means the rapidity with which the colon is being filled can be estimated.

6. When you have once commenced a series of flushings of the colon you must on no account stop them for longer than twenty-four hours at a time, until the appearance of the washings informs you that all retained fæcal material has been removed. Otherwise you may make the patient very ill, as the long-dried fæcal matter being moistened and dissolved liberates in the intestines of the patient the locked-up toxins, and such a manifestation as an attack of acute urticaria may supervene. For this reason it is advisable to add to the washing water a little boric acid, orphol, or other harmless disinfectant.

I shall now describe in detail the method I adopt of washing out the colon, because, easy as it really is, I have found that without specific directions those who attempt it for the first time do not as a matter of fact succeed in introducing a sufficient quantity of water to be of any real service. Moreover, they give the patient unnecessary pain and annoyance.

The most essential part of the apparatus is a proper douche-can. This should hold one gallon and two pints, and must be provided with a glass gauge graduated in ounces, by which the operator can estimate the rapidity of the flow, and the amount of liquid which has passed into the patient.

The rubber tube coming from the can should be five feet in length, and provided with a tap at its distal extremity, which can be manipulated with one hand. Its extremity is armed with a conical soft rubber anal tube of about six inches in length, with a terminal eye.

The apparatus I use for securing the proper inclination of the patient's body is very simple, and consists of a base-board about four feet in length, to an extremity of which is hinged a similar padded board. By a well-known device the upper board can be retained at any angle of inclination

with the lower one. The patient is placed upon the apparatus in such a position that the free edge of the upper board will be level with his buttocks. In certain cases, instead of the dorsal position it is preferable to place the patient in the knee-elbow position.

The details of the administration of the douche are as follows:—The patient, dressed in warm dressing-gown, drawers, and stockings, is placed upon his back upon the padded board, which lies on the bed in such a position that the edge of the apparatus comes to the fold of the buttock. By the handles at the side the upper board is raised until the inclination of the upper board with the lower one is about 20°. All exposure is avoided by placing a towel across the patient's thighs. Nine pints of water at 100° F. are now placed in the douche-can together with as many grains of common salt as will produce a normal saline solution of .75 grain per cent. We now see that the taps of the apparatus are shut, and raise the can to the determined position above the patient. Every foot we raise it will give us half a pound pressure to the square inch in the bowels of the patient. We now open the upper tap, and then the lower one, and allow a pint of water to flow through the apparatus. This shows us that it is in running order. As soon as the level of the liquid in the gauge has sunk to the gallon mark we close the lower tap near the distal end of the tube, and we are now ready to commence the operation. The operator now takes his stand at the right-hand side of the patient as he lies in bed, and lubricates the anal rubber tube held in his right hand with a piece of soap which the nurse is holding ready for him. Putting the hand holding the tube under the patient's right thigh as he lies in bed, he introduces it into the anus without exposing him in any way. The left hand is then placed upon the patient's abdomen, where it remains during the whole operation to check the effect the injection is producing upon the abdominal viscera. With the thumb of the right hand the tap is turned on, and the water allowed to pass into the rectum.

As soon as the patient complains of pain or discomfort we at once turn off the tap, and wait for a short time to allow the spasm to pass off. Remember that if the water is introduced too rapidly it will cause pain, likewise if it is too hot or too cold. After as much liquid has been introduced

as the patient can bear the tap is turned off, the tube withdrawn, the support is lowered into the horizontal position, and the patient told to lie on his back as long as he can possibly retain the injection.

If the mercurial manometer is used, it is attached to a chair or the pillow of the bedstead at the level of the patient's body. At first the rise in the column of mercury will be slight, as the fluid is passing into the bowel without meeting with any resistance. As, however, the bowel fills up, the column of mercury will rise until it registers the maximum pressure which the height of the can above the patient will produce. On more than one occasion when using it I have been informed, by a sudden fall in the column, of the fact that the fluid in the bowel had found its way past a faecal mass which was blocking the passage.

It is important to bear in mind, if any considerable amount of pain is caused with less than a pressure of one pound to the square inch, that there is probably an inflamed condition of some part of the large bowel. In such cases either the operation must be discontinued, or the operator must content himself with introducing a comparatively small amount at a much lower pressure.

Whatever is brought away by the douche should be carefully examined by the physician, as it is of the utmost importance to ascertain whether the diagnosis of faecal retention is correct.

Faecal matter may be known to be old by—

1. Its colour; this is dark green, brown, or black, except in cases where the patient has been for a long time on a purely milk diet. In this case it is whitish or putty-colour, but very sticky and plastic. It is important not to be misled by the colour which is given to the stools by iron, bismuth, and certain articles of diet, and by the presence of altered blood in the stools.

2. Its consistency. This is hard unless it has been softened by oil or other injections.

3. Form. It is usually in the form of scybala—small lumps rounded either by attrition, or moulded by the pockets in which it has been so long retained.

These douches are best given in the evening, in order that the patient may be able to lie still for some hours afterwards. This is important, as the introduction and removal of such a large quantity of liquid into the abdominal cavity is apt to dis-

turb the circulation. The treatment thus given will not interfere with the daily avocations of the patient. In severe cases, and where there is the slightest suspicion of inflammation, he should lie up for the week or so that the treatment lasts.

The douches should be continued every day until the washings show that all faecal material has been removed. If discontinued before this is the case, auto-intoxication is not unlikely to ensue. I have met with several such cases. One in particular which I saw last year in consultation impressed itself very strongly upon my mind. In this instance the patient refused to take any more douches after the third one, although many dark and hard scybala were coming away. The second day after the last injection she had an attack of most acute urticaria accompanied with general malaise. This promptly disappeared on resuming the douches, which removed a further large quantity of inspissated faecal material. The dry scybala are probably almost innocuous whilst undisturbed, but when softened by douche set free the toxins which they contain.

Although a series of large douches as described above is of the utmost use for the specific purpose of emptying the bowel in order to commence the treatment of the constipation, the patient must not on any account be allowed to repeat them, as they are often prone to do whenever they imagine that the bowels have become abnormally full. If the patient is properly and scientifically treated they will probably not be required again during the lifetime of the patient. Whenever they are given it must be under the immediate observation of the physician. Whilst five or six are harmless, yet when given at all frequently they are very relaxing to the muscular substance of the intestine, and if persevered in will effectually take away any chance which the patient has of eventually being cured of his constipation.

These points are well illustrated by the two following cases which were under my own immediate observation.

Mrs. H—, aged 50, consulted me in October, 1897. She had been troubled with constipation since the age of ten years, and for the last year or two her bowels, if left to themselves, would only be opened every ten days. She had taken all kinds of purgative medicines with only temporary relief, until five months ago when she began, on the

advice of some friends; to give herself every week a large enema of warm water of sufficient bulk to wash out the whole of the colon. At first this acted in a very satisfactory manner, and ceasing entirely to have natural evacuations she periodically washed out her bowels as often as they became loaded enough to cause discomfort. Her health at first was very much improved, her appetite was good, and she felt an unusual amount of *bien-aise*. But about two months ago she began to suffer with indigestion and flatulence, and she observed that apparently the whole quantity of liquid introduced by the douche did not come away, sometimes several, as much as half a gallon being retained. She had also now to use much larger douches than at first in order to relieve the bowels.

On examination she was seen to be very sallow, with a dry and unhealthy skin, and had a considerable number of pigment spots on the forearms. The abdominal walls were thin and flaccid, and there was evidently, to a moderate extent, enteroptosis. I commenced treatment by instructing her to leave off the intestinal injections, and to take three grains of the solid extract of cascara three times a day. My object in doing this was to ascertain if possible whether the bowel was still able to react to a moderate degree of stimulation. As the bowels had not acted after thirteen days I washed them out, stopped the cascara, and commenced the daily application of the electrical current from the coil described presently.

Every third day a moderate douche was given to partially empty the bowels. The first indication of improvement was noticed after the thirteenth application, when the patient had a natural inclination to stool, followed by a small evacuation.

A pill of two grains of the solid extract of cascara sagrada was now prescribed every night, and this in conjunction with the daily application of the electricity produced sometimes one and often two natural actions of the bowels. No douches were required after the twentieth application of the current. After the fortieth time the treatment was given only on alternate days, and after fifty applications the cascara was discontinued. Exercises for the abdominal muscles were now substituted for the electrical applications, and the patient was dismissed with the direction to continue these indefinitely. Her bowels have continued regular

since the termination of the course of treatment without the aid of any purgative whatever.

Miss J. F—, æt. 41, a domestic servant, was sent to me by my friend Dr. E— in January of this year as a case of incurable constipation. Her history was that she had been in very poor health since a great mental shock which she had sustained about twelve years ago. She gradually lost strength, and to some extent control over her legs. She suffered from indigestion, loss of appetite, and a feeling of faintness after very slight exertion. With this was associated very obstinate constipation, and at the time she came under the treatment of my friend the very strongest purgatives appeared to have lost their effect. Dr. E—, very rightly grasping the fact that the neurasthenic phenomena were probably due to the absorption of toxins from the gastro-intestinal tract, commenced in May, 1897, to wash out the large intestine with large douches given in the manner advocated by me. The correctness of his deductions was verified by the result, as on the removal of enormous masses of faecal matter her symptoms practically disappeared. But the constipation remained, and it was found that the bowels could only be opened by the use of very large enemata, of which she had forty or fifty between May, 1897, and January, 1898.

She was admitted into hospital under me on January 26th, 1898, and put to bed. On a physical examination there were no abnormal signs, with the exception of a considerable degree of abdominal relaxation. She was kept in bed on ordinary diet, and no medicine was given except a little infusion of gentian. On January 31st, there having been no action of the bowels since admission, she was ordered the daily application of the ordinary faradic current to the abdominal muscles, and to the colon in lengths, at the same time being given salol by the mouth with the idea of limiting intestinal putrefaction. On February 3rd, there having been no evacuation, a douche was given, but without result; and this being repeated, there followed a copious action of the bowels on February 6th, followed by three slight ones on the two following days. On February 17th treatment was begun with a large primary coil, especially wound with very thick wire for use in electric baths. When the contact breaker of this apparatus is arranged to give slow interruptions very powerful contractions are produced

in the abdominal muscles. A large flat electrode was placed over the lumbar region and kept there, the patient lying on it at the same time that the other pole was applied in succession to the different parts of the abdominal wall. The patient was also made to go through certain exercises daily, calculated to develop the abdominal muscles. On February 20th, 21st, 22nd, 23rd, there were two actions of the bowels on each day, and subsequently one daily until the end of the month. On March 1st a pill consisting of two grains of the solid extract of cascara was ordered to be given every night, and the electricity was diminished. On the 7th the electricity was discontinued, and the patient given a pill three times a day containing a grain of hydrastin, a third of a grain each of euonymin, alum, powdered ipecacuanha, with a tenth of a grain of extract of belladonna. From this time until her discharge on March 14th the bowels were regularly moved every day.

The lessons which we may learn from these two cases are, I think,—

1. That it is not safe to give many enemata of large amount unless we also take steps to improve the tone of the abdominal muscles.
2. That even in apparently hopeless cases, where purgative drugs have quite ceased to be efficacious, and the ordinary secondary faradic current produces no good result, we may still restore tone to the abdominal muscles by the current from a primary coil of thick wire slowly interrupted.
3. That when we have done so we may hope to perpetuate the good result obtained by the daily use of exercises for the abdominal muscles, with or without a small daily dose of cascara or aloes.

(To be continued.)

SOME NOTES ON ABORTION AND ITS MANAGEMENT.

UNDER this heading we find in the 'Intercolonial Journal of Australia' of March 30th, 1898, a paper by Adam. Here Adam pleads for an intelligent expectant treatment—a delay until Nature has made her effort to complete the detachment of the ovum from the uterus, provided always there are no untoward signs. He does not subscribe to the

view that curettage of the uterine cavity is advisable in all, or at all events most, cases of abortion. But the opinion is gaining ground that very many cases of abortion are injuriously affected by the indiscriminate use of the curette. The argument that abortion is a pathological process, and therefore there must be a diseased endometrium, which should be removed, does not seem to be sound.

The prime agent in the management of abortion is rest. When signs of impending abortion appear, the patient should be sent to bed, and should remain there absolutely until she has convalesced. As long as there are no symptoms of anything going wrong, such as hæmorrhage, or foul-smelling discharge, rise of temperature or pulse, it is only necessary to keep the patient at rest, and allow the ovum to separate in its own good time. Should there be hæmorrhage in an early abortion, and the case seen before separation of the ovum has taken place, it is an excellent plan after cleansing the vagina thoroughly to insert into the cervical canal a strip of iodoform gauze, packing it fairly tightly, and then to pack the vaginal roof with similar strips, leaving them from twelve to twenty-four hours in position. On their removal the vaginal roof will be found nice and clean, with the cervix well dilated, and in many cases the ovum will be found quite separated and free. If it is not, its removal is greatly facilitated by having the canal dilated. In the past, before we ventured to thrust gauze or anything else into the uterus, the writer frequently made it a practice to tamponade the vagina, and although the sterilisation of the vagina was necessarily imperfect, the results were excellent.

The routine administration of drugs, such as ergot, quinine, or Indian hemp, cannot be too strongly deprecated. Ergot, if it has any effect at all, must render the uterus in such a condition of tonic contraction that any subsequent procedures will be hindered; and as for quinine or cannabis indica having any effective ecboic action, Adam has doubts. But it is quite another thing to give such drugs after the contents of the uterus have been expelled. Then their effect on the vessels and uterine involution is marked. Perhaps one of the best combinations for the purpose is chlorate of potassium with ergot; another useful prescription is the stock one at the Women's Hospital, known as the Q. E. D. mixture—quinine, ergot, and

digitalis. Such treatment quite suffices for an ordinary abortion; but in those cases where hæmorrhage has been going on for some time, or where decomposition of the uterine contents is threatening, the uterus must be evacuated without delay.

Here it is that curettage has its legitimate field of operation. The patient should be anæsthetised and placed on a table opposite a good light; and the cervix well exposed to view. If the cervix is not already well dilated, it should be carefully opened up, and the uterine canal gently and carefully curetted. The physician should be sure to examine all the scrapings, to see that the whole of the ovum has been removed, for the writer has known quite large pieces to be left behind sufficient to nullify the purpose of the operation. The uterus is then irrigated with sterilised water, or a weak solution of lysol, and then packed with iodoform gauze.

The operation is simple enough, but it has its dangers, and should not be lightly undertaken. Chief amongst the risks is the conveying of sepsis, although it would hardly be thought so in these days of asepsis. Another risk is the tearing of the uterus during dilatation, and of perforating it with the curette. The conditions of the case favour these accidents, for the uterus is soft and friable, and this is especially so when decomposition of the uterine contents has taken place. So easily may the uterus be perforated under such conditions that the writer has seen it done, and the operator was not aware of it until he hooked down with his curette a piece of intestine.

One more caution before resorting to the curette. After the patient is anæsthetised, make a thorough exploration of the pelvis bimanually in order to see that there are no other diseased conditions, such as a pyosalpinx on one side, or a cystic tumour of some sort. For the writer has known patients to die from acute peritonitis after curettage from, apparently, a want of care in this respect.—*Therapeutic Gazette*, October, 1898.

DR. WHITRIDGE WILLIAMS, writing in the 'American Journal of Obstetrics and Diseases of Women and Children' on "The Bacteria of the Vagina and their Practical Significance," arrives at the following conclusions:

1. We agree with Krönig that the vaginal secre-

tion of pregnant women does not contain the usual pyogenic cocci, having found the *Staphylococcus epidermis albus* only twice in ninety-two cases, but never the *Streptococcus pyogenes* or the *Staphylococcus aureus* or *albus*.

2. The discrepancy in the results of the various investigators is due to the technique by which the secretion is obtained.

3. As the vagina does not contain pyogenic cocci, auto-infection with them is impossible; and when they are found in the puerperal uterus they have been introduced from without.

4. The gonococcus is occasionally found in the vaginal secretion, and during the puerperium may extend from the cervix into the uterus and tubes.

5. It is possible, but not yet demonstrated, in very rare instances that the vagina may contain bacteria, which may give rise to sapræmia and putrefactive endometritis by auto-infection.

6. Death from puerperal infection is always due to infection from without, and is usually due to neglect of aseptic precautions on the part of the physician and nurse.

7. Puerperal infection is to be avoided by limiting vaginal examinations as much as possible and cultivating external palpation. When vaginal examinations are to be made the external genitalia should be carefully cleansed and disinfected, and the hands rendered as aseptic as if for a laparotomy. Vaginal douches are not necessary, and are probably harmful.

Auto-intoxication and Albuminuria ('Berliner klinische Wochenschrift,' 1898, No. 14).—Praetorius thinks it impossible to deny that there are cases of functional albuminuria, cases in which there is no organic disease of the kidneys. This is specially the case in so-called cyclical albuminuria, where the albuminuria is present in the morning, and disappears as the day goes on. He quotes a case associated with enlarged liver which proved to be due to hydatids, and disappeared after this had been cured by incision and drainage. But he contends that the main cause of functional albuminuria is auto-intoxication, either gastric, or intestinal, or hepatic, or, as in the albuminuria of myxœdema, from the thyroid gland; or from some deranged tissue metamorphosis, such as occurs in gout, diabetes, and obesity.

Birmingham Medical Review, Nov., 1898.

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THE USE AND ABUSE OF MYDRIATICS.

BY

ERNEST CLARKE, M.D., B.S.Lond., F.R.C.S.,

Surgeon to the Central London Ophthalmic Hospital,
and Ophthalmic Surgeon to the Miller Hospital.

Mydriatics (from $\delta \mu\delta\rho\alpha\varsigma$) in common use in medicine are, with the exception of cocaine, alkaloids derived from the *Solanaceæ*. They are powerful poisons, and, as their name implies, dilate the pupil and diminish the power of, or paralyse entirely, the ciliary muscle. They are—

Atropine sulphate, from *Atropa belladonna*.

Homatropine hydrobromate, a derivative of atropine.

Duboisine sulphate, from *Duboisia myoporoides*.

Hyoscyamine sulphate, from *Hyoscyamus niger*.

Scopolamine sulphate, from *Scopolina atropoides*.

Daturine sulphate, from *Datura stramonium*.

Atropine, hyoscyamine, and hyoscyne are the three pure alkaloids present in this group.

Cocaine hydrobromate or hydrochlorate, from *Erythroxylon coca*.

And amongst the rarer drugs (seldom employed) are—

Ephedrin, from *Ephedra vulgaris*;

Mydrin, a mixture of 1 part of homatropine with 100 parts of ephedrin; and

Gelseminine, from *Gelsemium sempervirens*.

The Method of Action of Mydriatics.

The sphincter iridis is supplied by filaments running in the third or oculo-motor nerve from the oculo-motor centre situated beneath the floor of the aqueduct of Sylvius. The dilator iridis is supplied by filaments of the sympathetic from the sympathetic centre in the medulla. Now atropine must act either by stimulating the dilator through the sympathetic, or paralyzing the sphincter by its effect on the nerve endings of the oculo-motor, or by both. Many accept this latter theory, viz. that

atropine acts by stimulating the sympathetic and paralysing the third nerve, but the following experiments go to prove that dilatation of the pupil by atropine *is solely effected* by the direct action of the drug upon the oculo-motor nerve endings or ganglia in the sphincter iridis. In all opposing muscles central innervation keeps up a balance; in oculo-motor paralysis this balance is destroyed, the dilator is allowed to "have its own way," and the pupil dilates. *Now atropine will cause a further*

This shows that the muscle, although cut off from its centre, has still a certain amount of tone, and therefore does not become fully relaxed; but atropine completes the paralysis, and the pupil dilates further.

Again, Braun has shown that if we remove a piece of the sympathetic, allow the nerve to degenerate, and then drop atropine into the eye the pupil becomes wider, but not so wide as normally. Now, division of the sympathetic allows the sphinc-

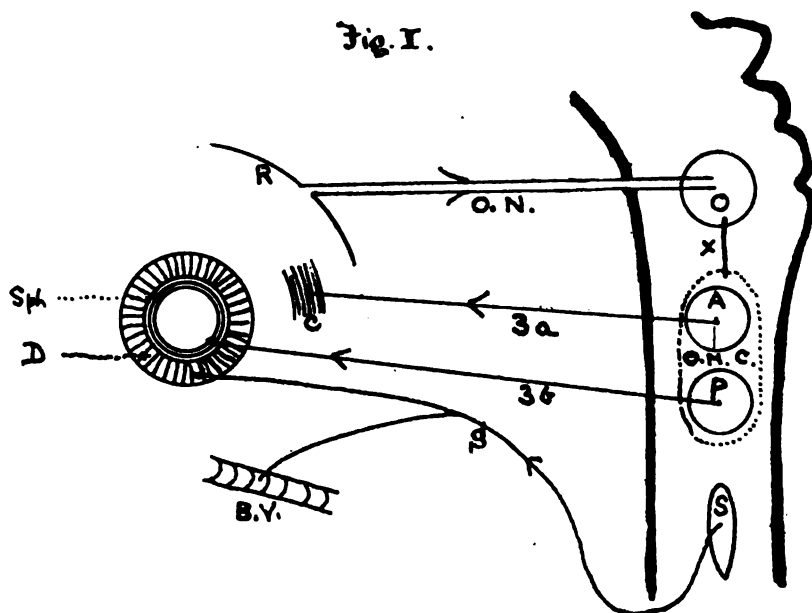


Fig. 1 (slightly altered from Erb).

- Sph.—The sphincter iridis supplied by the 3rd nerve (3 b) from the pupillary portion (P.) of the oculo-motor centre (O. M. C.).
 D.—The dilator iridis supplied by the sympathetic branch from the dilator centre (S.) in the spinal cord, branches from which also supply the blood-vessels (B. V.).
 C.—Ciliary muscle (portion of), supplied by branches of the 3rd nerve (3 a) passing from the accommodation portion (A.) of the oculo-motor centre.
 R.—Retina receiving an impression of light which passes by the optic nerve (O. N.) to (O.) the optic centre in the corpora quadrigemina. By means of the connection (X) between the optic centre and the oculo-motor centre this impression of light is conveyed to the pupillary centre (P.), and thence by the 3rd nerve (3 b) to the sphincter iridis, causing contraction of the pupil.

(In order to simplify this diagrammatic scheme the ophthalmic ganglion has been omitted.)

dilatation, and this was supposed to prove that the drug stimulated the sympathetic; but Bernstein has shown that if the oculo-motor nerve inside the cranial cavity of an animal is stimulated after the introduction of atropine into the eye, there is no contraction of the pupil, but that the contraction can be brought about by directly stimulating the sphincter iridis with the current.*

* Binz, 'Lectures on Pharmacology,' New Sydenham Society, vol. cliv, p. 208.

ter to preponderate, consequently the pupil contracts; but when atropine is introduced the sphincter is paralysed and the pupil dilates, but not so fully as normally because the dilator is cut off from its centre of innervation. The mydriasis of cocaine appears to be produced by stimulation of the sympathetic, as a cocaine pupil will contract to light, showing that the oculo-motor is not paralysed.

Atropine stimulates the vaso-motor system, and

causes the minute vessels to contract. Some writers have affirmed that this is due to a direct action on the muscular fibres in the coats of the vessels.

Atropine paralyses the *ciliary* muscle through its action on the third nerve, consequently all power of accommodation is lost, leaving the eye adjusted for its far point.

Homatropine has the same general effect as *Atropine*, but differs in that its full effect on the pupil and ciliary muscle manifests itself more promptly and disappears much more rapidly than *Atropine*, but it is not so complete a paralysing of the ciliary muscle as *Atropine*, and in young people whose accommodation is very active it is not to be relied on. On the other hand, in people over twenty-five years of age, whose accommodation is considerably weakened, it paralyses the muscles quite enough for all practical purposes when used in sufficiently strong doses and combined with Cocaine.

Cocaine favours the absorption of the drug by rendering the outer epithelial layer of the cornea and the conjunctiva more pervious.

Cocaine also lessens the irritability of the conjunctiva and the hyper-secretion of tears (Casey Wood).

I have made a series of experiments upon the eyes of persons over the age of twenty in order to determine the relative time taken by *Atropine* and *Homatropine* with Cocaine in producing cycloplegia or mydriasis, and also the time taken for the effect of the drug to pass off.

Sulphate of *Atropine* (1 per cent. solution) and tabloids of hydrochlorate of *Homatropine* and Cocaine ($\frac{1}{80}$ gr. each) (prepared by Messrs. Burroughs and Wellcome) were the forms of the drugs used.

The results, which differed in very slight degree, were averaged, and are shown by Figs. 2 and 3.

From a perusal of these diagrams we gather the following facts:

Atropine.

1. *Loss of accommodation:*

The action began 10 minutes after exhibition, but at the end of 25 minutes only $\frac{1}{2}$ dioptr of accommoda-

Homatropine & Cocaine.

At the end of 10 minutes a whole dioptr of accommodation power was lost, and the full action of

Atropine.

tion power was lost, and the full action of the drug was not shown for 90 minutes.

The effect of the drug began to pass off at the end of 36 hours, but the accommodation power was not fully restored until 10 days had passed.

Homatropine & Cocaine.

the drug was manifested in 60 minutes.

The effect began to pass off in 7 hours, and the whole effect had disappeared in 26 hours.

2. *Dilatation of pupil:*

No appreciable dilatation for 15 minutes, but complete in 30 minutes.

Pupil restored to normal size in 12 days.

At the end of 15 minutes the pupil had increased 2 mm. in size, and was fully dilated in 25 minutes.

In 36 hours the effect had completely passed off.

The differences between my results and those obtained by Messrs. Lang and Barrett ('R. L. O. H Reports,' vol. xi, p. 219) are perhaps accounted for by their using the *Homatropine* in castor oil, which I have found a very unreliable mixture.

Another advantage of *Homatropine* over *Atropine* is that it rarely, if ever, produces toxic symptoms.

The toxic effects of *Atropine* are local and general.

Local.—*Atropinism.* Papillary conjunctivitis, puffy lids, erythema and eczema of the lids and face may appear in those who cannot tolerate the drug, and even in others after long use.

General.—Dryness of fauces, quick pulse, flushing of face, palpitation, headache, nausea, prostration, garrulous delirium, and death.

In children we may get restlessness, fever, delirium, and hallucinations. These toxic symptoms are more likely to follow the employment of *Atropine* in solution, because in this form it more readily passes down the tear passages into the throat.

Duboisine acts in the same manner as *Atropine*, but is stronger. It does not irritate the conjunctiva, and is very useful in cases where *Atropine* is not tolerated, but its general toxic effects are

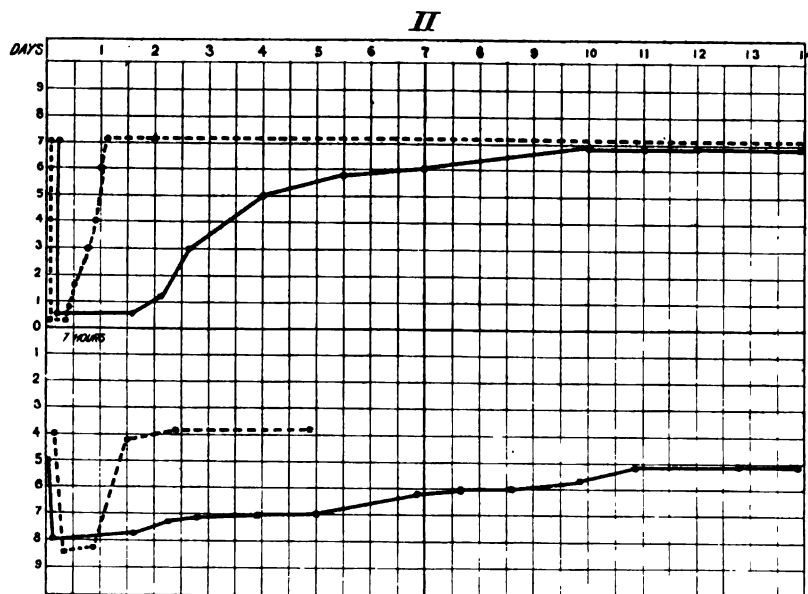
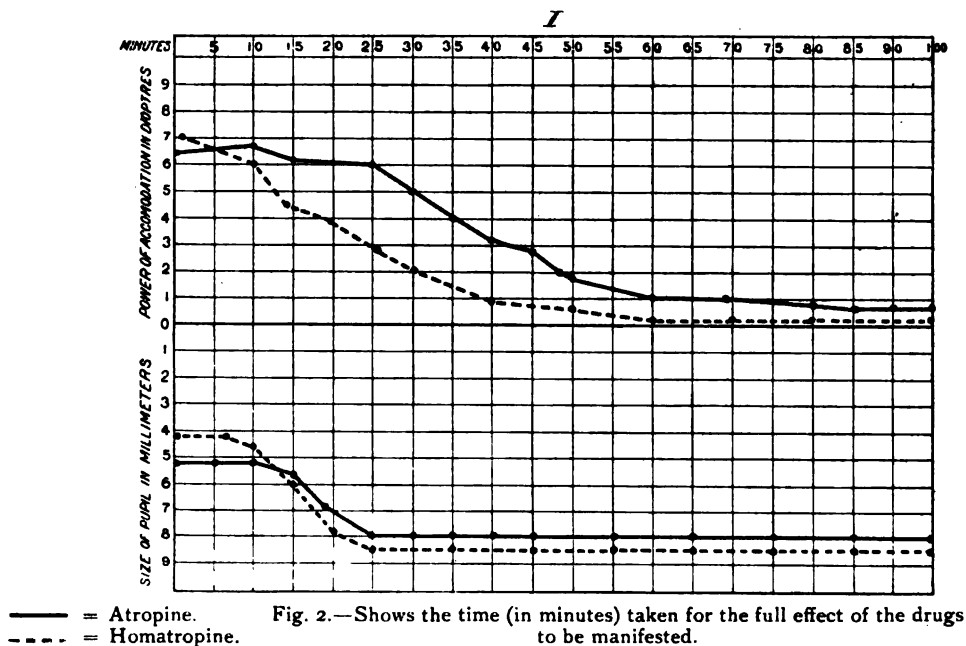


Fig. 3.—Shows the time (in days) taken for the effect of the drugs to entirely disappear.

much more severe, and extreme prostration may result. More than $\frac{1}{250}$ gr. should never be used.

Hyoscyamine behaves very much like Duboisine, and like it may be substituted for Atropine. In solution it does not keep well.

Both these drugs should be used in the form of tabloids or discs.

The Uses of Mydriatics in Ophthalmic Surgery.

Foremost I put the enormous benefit of rest, the "putting the eye into splints." This enforced rest lessens pain and tends to subdue inflammation, and hence is invaluable in iritis and cyclitis. In all inflammations of the cornea, wounds of the cornea, and the severe inflammation of the conjunctiva, especially in phlyctenular conjunctivitis,

a mydriatic is the most important part of the treatment. As I have previously shown, blepharitis and phlyctenular affections in children are almost always associated with some error of refraction which produces strain. This strain is removed by the paralysis of the ciliary muscles, and when the child is too young to wear glasses, atropine is the treatment.

Photophobia in children is at once lessened, and sometimes completely removed in a few hours by the effective use of atropine, and especially is this the case when combined with cocaine. In older patients cocaine alone is very useful.

Again, the majority of cases of convergent squint in children are due to over-accommodation produced by hypermetropia; and if the child is too young for glasses, atropine prevents the ciliary muscle from being used, and thus lessens or cures the squint. In all forms of spasm of the accommodation mydriatics are invaluable.

In estimating errors of refraction it is of first importance to have the ciliary muscle at rest, and in all young patients atropine or homatropine must be used.

The *mechanical* effects of mydriatics are also of great importance; they are—

1. Prevention of synechiæ in iritis and penetrating wounds or perforating ulcers of the cornea.
2. In certain operations, such as needling.
3. In central cataract, to enable the patient to see round the opacity.
4. To enable the surgeon to make a thorough examination to see lens and vitreous opacities, &c.

The Abuse of Mydriatics.

Mydriatics should not be used in *acute glaucoma*.

The fluids secreted by the glands of the ciliary body nourish the various structures of the eye, and the greater part of this fluid passes from the posterior chamber through the pupil into the anterior chamber, and thence out at the filtration angle into Schlemm's canal. If this angle is not widely open the outflow is arrested. Dilatation of the pupil draws the iris into this angle, compresses the spaces of Fontana, and narrows Schlemm's canal also, and thus increased tension or glaucoma may result from the use of atropine, especially in those predisposed, as hypermetropes and persons over fifty. Priestley Smith has shown that a small cornea and a shallow anterior chamber predispose

to glaucoma. Unfortunately iritis and acute glaucoma are often mistaken for one another, and many a glaucoma which might have been cured in its early stage had it been recognised is treated with atropine, and the eye irretrievably lost.

I would remind you of certain signs which may help in distinguishing these two affections.

Acute Glaucoma.

1. Increased tension.
2. A dilated pupil.
3. Pain very severe, not only in the eye, but in surrounding parts.

Iritis.

1. Tension normal.
2. Pupil contracted.
3. Pain less severe and more local.

In glaucoma secondary to iritis we generally do not see the acute form, and atropine will probably do little if any harm, as the pupil will probably not dilate because of adhesions.

In such cases use *homatropine* carefully and watch the result: if the pupil dilates well, do not repeat it; and if the tension increases, use eserine.

If it is necessary to dilate the pupil of elderly people in order to make a more thorough examination of the lens, &c., it is wisest *not* to use atropine. Homatropine will answer the purpose just as well, and is much safer.

In perforating ulcer of the cornea eserine sometimes does more good than atropine, probably because it reduces the tension. If the ulcer is at the margin, on no account use atropine, as prolapse of the iris may result. The same advice applies to marginal *wounds* of the cornea.

In neuritis, retinitis, and retinal asthenopia mydriatics are contra-indicated, because their use causes more light to enter the eye.

In what form should mydriatics be used?

I believe the days of "drops" are over.

It is impossible to know when using drops or solutions how much of the drug is absorbed, and how much is wasted. They produce toxic symptoms by passing down through the tear passages into the throat, and they do not keep well.

On the other hand, ophthalmic tabloids and discs have been brought to such a state of perfection that they form the most scientific, efficient, and safe method of administering a mydriatic.

The most useful tabloids are—Atropine $\frac{1}{200}$ gr., and Homatropine with Cocaine $\frac{1}{50}$ gr. each.

Tabloids or discs should dissolve quickly when placed on the inner surface of the lower lid, and should cause little or no irritation or pain.

CONSTIPATION AND ITS MODERN TREATMENT.

BY

GEORGE HERSCHELL, M.D.Lond.

(Continued from p. 79.)

WE have now got so far in the treatment of our case that we are quite sure that there is no retained faecal matter in the large intestine.

(a) *We have now to restore Tone to the Muscular and Nervous Tissues of the Bowel itself, and to the Abdominal Walls.*

We have five practicable means of doing this: hydrotherapy, electricity, massage and its substitutes, gymnastics, and drugs.

Hydropathy.—Water of different temperatures is an extremely valuable means of restoring tone to muscular and nervous structures, if we know how to use it. No amount of simple warm or cold baths will produce the slightest effect for good upon the stomach or intestines; but by taking advantage of the opposite effects of water at different temperatures we can intensify the reaction of each respectively. For instance, the primary effect of immersion in hot water is to dilate the capillary vessels of the skin. This is shortly followed by contraction. If we seize this moment when the capillaries are about to contract and apply cold water we shall intensify this contraction, and exercise a distinct tone and permanent tonic effect. Likewise if we immerse the whole body in cold water we produce very little effect; but if we limit the application to one part, such as the spinal column or the epigastrium, a marked effect is produced. Personally I make use of three hydrotherapeutic procedures in the treatment of constipation, and I am quite satisfied with the results they give me.

(1) *Ice massage of the abdomen.*—This we owe to Professor Turck, as well as several other valuable methods of treatment. The patient first of all takes a hot bath at as high a temperature as he can comfortably bear. He then lies upon a flat surface, and the whole of the abdomen is rubbed and massaged with a cake of ice. The effect is a very powerful stimulation of the stomach and in-

testines, and of the solar plexus. Instead of holding the cake of ice in the hand, the operator may place it in a rubber bag; and Professor Turck has designed a special one for this purpose.

(2) *The cold abdominal douche.*—I have found this of great use in the treatment of constipation, and my method of applying it is the following:—I make the patient lie down in an ordinary bath containing enough warm water to cover the whole of the body except the abdomen. An attendant then pours from a height of two or three feet a jug of water upon the abdomen. At first this water may be tepid, but day by day the temperature must be reduced as the patient becomes accustomed to it, until it is finally icy cold. The height above the patient is also gradually increased, and also the number of jugs used.

(3) *The cold spinal douche.*—The patient kneels in a tub containing a little warm water, and leans slightly forward. Jugs of cold water are poured down his back in exactly the same way as in the abdominal douche.

Electricity.—It is a remarkable fact that electricity alone will not cure constipation. Whilst a patient is going through a course of electrical treatment his bowels will become perfectly regular, but he will invariably relapse when he discontinues its use. And yet this agent is of extreme value in the treatment of this condition, and fulfils indications that nothing else will. Used in combination with the other means at our command, it is indispensable. We use it in the treatment of constipation for three purposes:

(1) To tone the innervation of the intestines.

(2) To restore tone to the musculature of the bowels themselves.

(3) To strengthen the abdominal muscles.

To restore tone to the nervous supply of the intestines.—For this purpose we make use of the continuous current. The method which I adopt is extremely simple, and consists of the application of the electrodes stable to the different ganglia from which the abdominal viscera receive their nerve-supply with frequent reversals. It is, however, necessary to be provided with a proper battery, without which nothing can be done. The absolute minimum with which good work can be done is a set of thirty cells, preferably connected up in series, with a current reverser, rheostat, and galvanometer included in the circuit. The best

rheostat that I have ever seen is Willm's current controller, and is made by the Chloride of Silver Dry Cell Company, of Baltimore. It is perfectly efficient, permits of the finest gradations of current, and possesses the great advantage of being applicable also to the faradic current. I usually commence by applying the cathode to the epigastric region, where it acts presumably upon the solar plexus. The anode is then placed in succession upon the ganglia of the sympathetic in the neck stable for one or two minutes each, with a current strength of 1—3 m.a. The anode is then placed on the back of the neck over the seventh cervical spine, and about 10 m.a. of current allowed to pass for 10—15 minutes, the direction of it being reversed every few minutes. The abdominal electrode is then placed in succession for a few minutes on the abdomen on each side of the umbilicus.

To restore tone to the musculature of the intestines.—Recent experiments appear to show that it is impossible to produce contraction of the muscular walls of either stomach or intestines by either the galvanic or the secondary faradic currents when applied to the body by means of external electrodes. But my observations lead me to believe that this can be done by a coil of a special kind. I am alluding to the primary coil wound with very thick wire. I believe that this was introduced to the profession by Dr. de Watteville, and was designed for use in giving the faradic bath. A lengthened experience of its use has proved to me that it is of very great value in the treatment of atonic conditions of the stomach and intestines, and with it anyone can demonstrate to his own satisfaction that peristaltic movements are really set up. The two cases recorded are examples of the really marvellous results which can be produced by its intelligent use. In using it I first of all place a large electrode of at least fifteen square inches in area on the lumbar region of the patient or under the buttocks, this being connected to the anode. To the other pole is fastened a flat sponge or felt-covered electrode of nine square inches in area. This is placed in succession upon points in the course of the colon, commencing at the cæcum and finishing in the right iliac region. The current strength is to be just sufficient to throw the abdominal muscles into visible contraction. The interruptions are not to

exceed 200 a minute. The whole procedure is then to be repeated several times, making the *séance* of about ten minutes' duration.

To strengthen the abdominal muscles.—For this purpose the ordinary faradic secondary current may be used, preferably with a roller electrode, but I am rapidly coming to the conviction that the primary coil wound with thick wire used with slow interruptions is far superior for practical purposes.

Massage.—I very rarely use ordinary massage in the treatment of atony of the intestines, as it has many disadvantages. The two chief ones I will enumerate.

1. To be of use in constipation it requires such special technical knowledge that unless administered by the physician himself it is useless. Few, if any, of the operators of massage with which the market is at present flooded have had an opportunity of acquiring this.

2. Massage administered by the average operator may be actually dangerous in cases of constipation. Few of the operators who have been trained in the usual way know enough to render their manipulations of the abdomen even reasonably safe in some of the conditions met with in constipated patients. That my contention of the danger of massage even given by an expert is correct will, I think, be apparent to my readers if they will peruse the following extract from the latest published work on constipation.* The directions are for "manipulation intended to break up accumulated *feces*," presumably in the cæcum. We are instructed that "the extended fingers of both hands are placed over the cæcum, and then with the pulps of the fingers deep pressure is made, so as to break up the accumulated and hardened material by pressing it down against the posterior wall of the pelvis."

Yes, by these means we may certainly break up accumulated and hardened material in the cæcum, but only at the risk of forcing some of it into a widened appendix, or rupturing the cæcum itself. Besides, there is danger that what the innocent masseuse takes to be a mass of accumulated and hardened *fecal* material may really be an inflamed and thickened appendix, or a mass of intestines matted together by a previous local peritonitis. A far better and safer method of dealing with such a

* Illoway, 'Constipation in Adults and Children.' Macmillan and Co., London, 1897, p. 221.

collection in the cæcum would be a series of large douches already described. The advocates of massage in the treatment of constipation claim that it stimulates the circulation in the intestinal walls, that it will provoke fibrillary contraction of their muscular fibres, and that it will stimulate and exalt the functions of the nervous system which presides over the functions of the abdominal viscera. I believe that all these effects can be secured better, safer, and more rapidly by the use of mechanical vibration.

The earliest systematic use of vibration as a therapeutic agent was probably by Zander, who had already been for some time treating various nerve troubles with it when in 1877 Hovath* made a preliminary communication upon the subject to the Société de Biologie. In the following year he published a systematic paper dealing with the influence of rest and movement upon life, and in which he claimed to be able to sterilise a fluid containing germs by the action of prolonged shaking. In the same year Vigoroux,† working in Charcot's clinic at the Salpêtrière, commenced to try the effect of systematic vibration in certain forms of nervous disease. As he appears to have worked chiefly upon hysterical subjects, his results must be discounted, but on more than one occasion he appears to have materially relieved the pains of tabes. In 1880 Reinke published a valuable paper upon the effect of vibration, in which he stated that the growth of germs was distinctly retarded by acoustic vibrations. In the same year Baudet‡ pointed out that it was easy to produce local anæsthesia by mechanical vibrations, and that in this way various pains could be efficiently treated.

The first English worker in this field appears to have been Mortimer Granville, who, after several articles in the journals during 1882, published in the following year a small book dealing very fully with the technic of the subject.§ The instrument which he devised for the purpose of producing the vibrations consisted essentially of a powerful electro-magnet, which was fitted with a

vibrating armature on the principle of the contact-breaker of a faradic coil. To this armature was attached a rod terminating in a disc or brush for application to the patient. The whole contrivance was portable, the electro-magnet being covered with leather and held in the hand. It was possible with this instrument, by shifting the platinum contact, to get vibrations at two different speeds. The disadvantages attaching to the instrument were—

1. The difficulty of maintenance. Owing to the large current required to work the machine the platinum contacts required to be frequently renewed.

2. The effect of its application was not a true vibration, but really a rapid succession of blows. When pressed firmly upon a bony surface the subjacent parts were thrown into vibration, but this was an antero-posterior one, and not a lateral movement.

Since Granville's book the chief writings upon the subject which have appeared are articles by Schmidt,* Meltzer,† and Charcot.‡ The most important of these is that of Meltzer, whose experiments are of great interest. He shows us conclusively that vibration produces a direct shaking up of molecules of which the ground substance of cells are composed. It is therefore *a priori* most probable that vibration will affect metabolism in some way or other. He concluded as the results of his observations that vibration modifies nutrition, and maintains that growth is stimulated by mild vibration, and that strong vibration produces rapid catabolism.

Although on searching the literature of the subject we find that vibration has been extensively used in the treatment of functional nerve disorders, yet no attempt appears to have been made to apply it locally for the relief of atonic conditions of the gastro-intestinal tract.

About a year ago I was led to investigate its effect in such cases from the necessity I experienced in my practice for an agent which would act directly upon the muscular substance of the

* Alexis Hovath, "Ueber den Einfluss mechanischer Erschütterung auf die Entwicklung der Spaltpilze," 'Pflüger's Archiv,' Band xxiii, 1880, p. 434.

† Vigoroux, 'Progrès médicale,' 1878, p. 746.

‡ Baudet, "Traitement de la douleur par les vibrations mécaniques," 'Progrès médicale,' 5 février, 1881.

§ Mortimer Granville, 'Nerve Vibration and Excitation,' London, 1893.

* Schmidt, "Ueber den Einfluss der Bewegung auf das Wachstum und die Virulenz der Microben," 'Archiv für Hygiene,' 1898, Bd. xiii.

† Meltzer, "The Importance of Vibration to Cell Life," 'New York Medical Journal,' December 24th, 1892.

‡ Charcot, "La Médecine vibratoire," 'Le Progrès Médical,' 1892, p. 131.

stomach and intestines. My first applications were made with the electro-percuteur of Mortimer Granville, but I found that it was necessary to have an instrument which would produce lateral vibration, not successive blows, and which would produce the same in a more purely mechanical manner.

Such an apparatus I found to be in existence in a contrivance invented by a Swedish physician, and patented by him in his country. In its form as sold by him it was run by a system of geared

the cable (D) is screwed by means of a suitable socket. This bar carries at its distal extremity a socket to receive the pin or stud which projects from the centre of the contact (K), to be presently described.

The contacts.—These are the parts of the apparatus which is actually applied to the body of the patient, and are of various forms—disc, rubber bulb, metal bulb, and leather pad. The one most generally useful is figured in the illustration (K), and consists of a disc of corrugated metal, carrying



Fig. 3.—A. Motor. B, C. Geared pulley. D. Cable. E. Wire. F. Choking coil. H. Thumb-piece. I. Handle. J. Oscillating bar. K. Contact. L. Cable.

wheels turned by hand by means of a crank ; but by discarding this part of the machine, and attaching it to a suitable electro-motor, I found that I had a very efficient machine, which would do everything that I required.

The apparatus, as will be seen from the accompanying photograph (Fig. 3), consists of the following parts :

The handle (I).—This contains an oscillating bar (J) to which reciprocal motion is communicated by means of an excentric on the axis, into which

at its centre a pin for attachment to the moveable part of the handle.

The cable (D).—This serves to communicate motion from the motor to the axis of the handle, and consists of an overplaited spiral. At each end is a male screw for attachment to the driving wheel of the motor and the axle of the handle respectively.

The motor (A).—The motive power in the original Swedish instrument consisted of a box containing a geared train of cog-wheels, by which

the revolution of a handle on the outside of the box would communicate a velocity of about 200 revolutions a minute to the cable. The drawbacks to this arrangement are the necessity for the services of a third person to turn the handle and to hold the box steady, the impossibility of securing anything like a uniform speed without a prolonged period of practice, and the difficulty of obtaining a suitable velocity. To overcome these difficulties I had an electro motor constructed which with a 100-volt current, such as is supplied to houses for lighting purposes, gives a velocity of the main shaft of 2000 revolutions a minute. This by the geared pulley (C) attached is reduced to a uniform velocity of 200 revolutions actually communicated to the driving cable. The addition of a choking coil (F) enables the number of revolutions a minute to be varied at will between 50 and 200. The choking coil is actuated by the thumb-piece (H), and connected with the motor by the wire (E), and with a wall-plug by the cable (L).

The motor was manufactured by Mr. Geere Howard, of 10 Berners Street, W., who is prepared to supply the complete apparatus.

The technic of vibration as applied to the cure of constipation.—The patient lies upon a flat and firm couch, the operator standing on his right hand. The apparatus motor, screwed to suitable table, is placed at such a distance from the couch that when the vibrating disc is applied to the abdomen of the patient the cable is kept on the stretch. With the left hand the operator holds the handle of the vibrator, with his right he controls the choking coil. I always commence by placing the disc on the epigastric region and holding it lightly applied to the skin,—in fact, resting by its own weight, cause it to vibrate rapidly for about two minutes. It is now placed on the region of the cæcum and vibrated much more slowly. After a stay of a couple of minutes in this position it is moved gently along the course of the colon, moving at the rate of an inch a second, until it reaches the right iliac region. Here it is allowed to remain for several minutes. It is then replaced in the right iliac region, and the process repeated. The sitting is finished usually by passing the vibrating disc once or twice up and down the spine, the patient being in a sitting position, firm pressure being made.

The results of this method of treatment in my hands have proved very gratifying, and not unfrequently an immediate action of the bowels has been produced shortly after the sitting. Many cases of habitual constipation have been successfully treated by vibration alone, but it is usually better to use at the same time the galvanic current for its action upon the splanchnics and solar plexus. If there is evidence of weakness of the abdominal muscles the patient must also execute daily the gymnastic exercises to be presently described.

Gymnastics.—The object of gymnastic exercises in the treatment of constipation is to strengthen the abdominal muscles, which perform so large a part in the act of defæcation. Weakness of the abdominal muscles is a much commoner condition than is generally supposed, and may be accounted for by the fact that very few of the ordinary forms of exercise act to any extent upon them. There are three special exercises which are of great value in restoring power to the muscular walls of the abdomen.

1. The patient, lying flat on his back, places some pillows on his feet. He now places his hands on his abdomen and raises himself slowly into a sitting position. He now slowly lowers himself until he is lying flat, as at first. This is one of the most powerful exercises for the rectus muscle.

2. The patient lying on his back raises both legs held stiff from the couch until they are pointing upwards at right angles to the trunk. They are then slowly lowered.

3. The patient hauls at a rope fastened to a ring or staple in the floor. This will throw into action the oblique muscles of the abdomen, which it is very difficult to influence in any other way.

These exercises should be performed every morning and night, at first only once each, gradually increasing the number of times as the weakened muscles regain strength.

(To be continued.)

Dysmenorrhœa.—Dr. H. Talley recommends a mixture of caffeine, potassium bromide, and tincture of gelsemium as of much value in the treatment of dysmenorrhœa. This should be administered for a few days before menstruation.

Indian Lancet.

CHAPTERS FROM THE TEACHING OF DR. G. V. POORE.

No. XII.

GENTLEMEN,—With regard to the detection of the acids, I shall only attempt to show you the qualitative tests, and only those qualitative tests which are easily applied, and the materials for which you are likely to have at hand, speaking to you as general practitioners of medicine.

I may, perhaps, usefully remind you of the regulations in force with regard to the practical examination at the University of London. Those rules are duly set forth in the Calendar as follows:

The Chemical portion of this Examination will be limited to subjects selected from the following list: those taken from Class A to be given as simple solutions; those from Class B in combination with various organic substances.

A. Morphine and its salts and meconic acid. Strychnine and brucine and their salts. Cocaine and its salts. Eserine or physostigmine and its salts. Alcohol. Chloral; chloroform. Aniline.

B. Mineral acids. Caustic alkalies. Oxalic acid. Phenol (carbolic acid). Prussic acid and other cyanides. Chromates. Compounds of—arsenic, antimony, tin, mercury, zinc, lead, barium, copper, silver.

You must remember that a simple solution may be anything in the whole list, but an organic mixture cannot contain anything in the first class.

I put strong sulphuric acid into that test-tube containing distilled water, and I need not say that I have generated a considerable amount of heat by so doing. Here is a simple fluid, and in analysing it you have got to use your tests and your senses. First of all it is colourless. This fact excludes the salts of iron and copper and chromates. The next point is the absence of odour. This practically excludes chloroform, chloral, alcohol, nitro-benzol, aniline, carbolic acid, and prussic acid. Next we find that blue litmus-paper is turned so vigorously red that we must have a free acid or an acid salt to deal with.

Next let us evaporate some of the solution I showed you yesterday to dryness on a piece of platinum foil. We get no residue. That is a

very important matter indeed. You get no residue with any simple solution of a mineral acid, neither do you get any permanent residue with a simple solution of oxalic acid, although you may get something upon your platinum foil before the oxalic acid is finally decomposed into carbon dioxide and carbon monoxide. Having, then, this very acid solution leaving no residue, it is highly probable that you have got a free acid to deal with. You may not be so certain, especially if you have your acid in an organic fluid. There are certain tests proposed, one in particular for the free mineral acids as a group. I will try and show you this test. Here is a solution of acetate of iron, and I add to that a few drops of a solution of sulphocyanide of potassium. I get a very deeply coloured red solution, which I freely dilute with water, and into each of the four tubes I have here I will put some of this mixture. This dilute mixture of ferric acetate and potassium sulphocyanide is uniform in tint, and looks like pale sherry. I next make ready a solution of each of these acids: sulphuric, nitric, hydrochloric, and oxalic. I now moisten this glass rod with sulphuric acid and transfer it to the first tube. You see I have immediately a dark streak of blood-red falling through the mixture. I now do the same with the nitric and hydrochloric, and get the same result. It is said you do not get it with oxalic acid, I am using the oxalic acid very much stronger; it is a saturated solution, for a weak solution yields nothing. But you see that even the stronger solution yields nothing which is very characteristic. That is not a test for any acid in particular, but for a free mineral acid, and may be used with organic mixtures. Now what acid have we here,—nitric, sulphuric, or hydrochloric? There are certain tests for the mineral acids which are exceedingly simple, and which you are no doubt quite familiar with. The test for sulphuric acid is the obtaining of an insoluble precipitate with a soluble salt of barium. I have here some barium chloride, and on putting it into the first tube I have immediately a white precipitate, but none in the second tube and none in the third. (I warn you when you are mixing acids with tap water, as we are here, that you may get with the tap water a slight turbidity caused by calcium sulphate in the water.) This white precipitate is insoluble then, as you see, even when I add some strong

nitric acid. You may want to carry the test a little further, and to demonstrate that there is sulphur in your precipitate. I take some of the barium sulphate precipitate and mix it with some pure sodium carbonate. Next I place it in a hollow scraped out of a lump of charcoal, and then use the inner flame of the blowpipe upon it. After a time you observe that it melts into a glassy bead. I now put this bead into a test-tube, add a little water to it, then a little hydrochloric acid, and then you can detect the smell of sulphuretted hydrogen. If you cannot trust your nose, expose a little acetate of lead paper to the fumes, and you get a black coloration. Thus we have proved clearly that the acid we had to deal with contained sulphur.

The next acid I will take is hydrochloric, and I do so for this reason, that the tests are positive ones, and not negative. Here I have the hydrochloric acid in a dilute solution. Here we have a solution again which is colourless and odourless, and acid in reaction. I put a drop of that liquid on the platinum foil and hold it over the spirit lamp. It evaporates and leaves no residue. That is a very important matter. To show you what that means here is some solution of binoxalate; you see it is colourless, odourless, and in dipping a blue litmus-paper into it I have an intensely red coloration, just as I did with the free mineral acid. But when I put a drop on the foil and evaporate, a white residue remains. To my solution of hydrochloric acid I add barium chloride, and get no precipitate; but with silver nitrate a very dense clotted precipitate is produced, which instantly falls to the bottom of the tube. With oxalic acid I get a dense precipitate of silver oxalate, but it is not so clotted, and does not fall in such masses to the bottom of the tube as does the silver chloride. The way in which it falls is not a test, but is one of those points which you should take notice of. To the white precipitate of silver chloride I now add some solution of ammonia; you see it is dissolved instantly. I now take the precipitate of silver oxalate and again add some ammonia solution; that also is dissolved instantly. Next I filter off some of my precipitates and collect them on filter-paper, and to what I have left in the tube I add some nitric acid; you now notice that the silver chloride, which was soluble in ammonia, does not dissolve in nitric

acid, but the silver oxalate dissolves after a short time. I have now got my two precipitates with silver nitrate, which have been filtered. One of these was clotted and the other not clotted, one soluble in cold nitric acid, the other not, but both soluble in ammonia. I will now take some of that precipitate of the silver chloride and heat it upon platinum foil, and for that I will use the stronger flame of a Bunsen burner. The water is driven off first of all, and you will notice how quietly this silver chloride behaves and submits to the heating process. My platinum foil is now red-hot, and I see the precipitate melting and spreading all round. I then take it off the burner, and find it is dried up to what the text-books call a "horny sectile mass." I get my knife to it, but cannot get it off the foil, though I can cut it with the knife. I will now take the oxalate from the filter-paper, and note that the oxalate is not so bulky. I now put that over the Bunsen burner, and ask you to watch it. There are fumes, and the substance spits a good deal, and explodes with little rockety offshoots in all directions.

I now go to the third mineral acid—namely, nitric. Here is some strong nitric acid. Notice the fumes. Next I dilute it. Here we have, as we have said with the other acids, a clear solution, without odour, leaving no residue on evaporation, and intensely acid, giving the test for a free mineral acid. The next thing is to determine which acid it is. The tests for nitric acid are mainly negative, that is an important point; there is no precipitant for it. The nitrates are soluble. You get no precipitate with the barium salt, nor with nitrate of silver, as I will show you. Now, how are we to proceed? One of the best ways is to neutralise. I will put that dilute nitric acid into a dish, and add to it some potassium carbonate. You see it is effervescing. The reason I advise you to neutralise with potassium carbonate rather than with potassium hydrate is, that the subsidence of the effervescence is a sure sign that you have accomplished your end. Next take a piece of bibulous paper, and moisten it with the solution, and dry it over the flame. You will find it will dry tolerably quickly. You now see that as it becomes dry in the flame it burns and fizzes like touch-paper. Another test which is sometimes useful is to take a piece of gold-leaf and put it into an evaporating dish with the dilute nitric acid, and then add a little hydrochloric acid.

We immediately get solution of the gold-leaf taking place, fumes are given off, and a deep yellow liquid left.

There is yet another test. I have here some of the solution of potassium nitrate, which I have put into a tube, and to the tube I add a crystal of sulphate of iron. Then, in order to liberate the nitric acid from the potassium nitrate, I let a drop of strong sulphuric acid slide down the side of the tube. In a little time you see the crystal gets discoloured, and there are dark fumes accumulating round it. That is not a bad test. What I want to insist on is that the best test for nitric acid is the negative test. We have got a free mineral acid which gives no precipitate, and we can form touch-paper with the nitrate.

Oxalic acid is a poison of considerable practical importance. It is largely used in trade, especially by leather workers and straw workers, and is easily obtained. It has occasionally been taken in mistake for Epsom salts, or other crystalline saline purgative. It is usually sold as a crystalline solid, and is readily soluble in water. Like the mineral acids it is corrosive and irritant in a high degree, but in addition to its local effect it acts, after absorption, as a powerful cardiac depressant. It is less corrosive, and less likely to cause perforation of the stomach than, let us say, sulphuric acid, but it must be remembered that it kills more quickly than any of the mineral acids. The following is a case in which the symptoms were severe, but ended in recovery. It is taken from the 'British Medical Journal' of April 23rd, 1881.

Oxalic acid poisoning.—C. P.—, æt. 30, at 9.30 a.m. on February 28th, 1881, swallowed one and a half ounces (3*℥*. worth) of crystallised oxalic acid dissolved in hot water. This was followed by heat and burning of throat and belly, and in ten minutes by vomiting of a yellow fluid mixed with blood.

At 11 a.m. he was admitted to King's College Hospital. There was a griping and a "feeling of swelling of the belly." Throat swollen and sore. Skin of lips swollen and raw. Tongue swollen and red, ditto fauces, uvula as big as a forefinger, general prostration. Pulse small and thready, tympanites, a few reddish papules over chest and back, but no regular rash. At 4 p.m. and at 7 p.m. he vomited, at 9.30 p.m. the expression was more bloated. Pain in back. Bowels acted once

in the day, tenesmus, no blood. He had passed twenty ounces of pale urine with difficulty, containing abundance of crystals of oxalate of lime, a decided trace of albumen, but no blood. At 9.30 p.m. pulse fell to 108, temperature 100°6'. From this time he improved. The sickness continued four or five days. The vomit never contained blood. Bowels constipated. Dysuria and albuminuria ended on seventh day. There was some dysphagia at first.

Treatment.—Chalk and lime salts. Opiates and bromide to relieve symptoms.

It is characteristic that nervous symptoms and cutaneous eruptions often occur. Here is a case in point. An individual took half an ounce of oxalic acid in solution instead of salts. He instantly became conscious of the mistake from perceiving the acid taste. Pain and vomiting ensued, and although they were mitigated in some degree by alkaline remedies, yet they recurred with violence. Spasms, impeded respiration, and general numbness were complained of; the pulse was scarcely perceptible at the wrists or temples; the extremities were cold, and the matter vomited became tinged with blood; after a short time he brought up a large quantity of blood. Diluents were freely administered, together with anodynes, and his condition gradually became more tolerable. Numbness, however, occasionally occurred, and was relieved by warm applications, and a drink of sago and wine. On the second day, vomiting, retching, spasms, and singultus supervened; the pulse was nearly 100 and feeble, and numbness and chilliness of the feet were present. A repetition of previous remedies gradually moderated these; but the hiccup continued for several days. On the sixth day he felt himself so well as, contrary to directions, to ride out in a gig. After this debility came on gradually, an eruption appeared over the whole body, and hiccup was occasionally present. He retained his senses until the day before his death, and complained often on swallowing any article which was not perfectly bland. He expired fourteen days after taking the poison in a state of perfect exhaustion.

The post-mortem signs are those of irritation and corrosion. There is often a good deal of blood effused in the stomach or elsewhere, and the altered blood in the stomach is often black, so as to remind one of sulphuric acid poisoning.

After absorption it is eliminated by the urine in the form of oxalates, and occasionally there is albuminuria or hæmaturia with strangury and pain in the loins.

The treatment consists in the administration of lime salts. Alkali must not be given, as the oxalates of the alkali are scarcely, if at all, less poisonous than the pure acid.

With regard to the tests for oxalic acid, it may be well to remind you that although it contains carbon it is entirely dissipated by heat. Placing some of the crystals on platinum foil over a spirit lamp you will see that it is entirely dissipated, and that potassium binoxalate treated in the same way leaves a white residue, but no blackening.

Oxalic acid disappears in the form of CO and CO₂.

Oxalic acid discharges the colour of ink, and it is largely used for removing ink stains.

Oxalic acid forms a characteristic silver oxalate with nitrate of silver, and when treating of hydrochloric acid I showed you how to distinguish between silver chloride and silver oxalate, so that I need not now repeat the test.

Solution of calcium sulphate added in considerable quantity to a solution of oxalic acid or an alkaline oxalate gives a white precipitate, which is somewhat slow in forming, and which is soluble in nitric and hydrochloric acids. Lead salts give a white precipitate, and copper sulphate a greenish-white precipitate.

Oxalic acid is easily separated from organic matter by dialysis.

The fixed caustic alkalis.—Potassa and soda are seldom used as poisons. Cases of poisoning by them are generally accidental. They cause pain and violent irritation, just as do the corrosive acids. The amount of mucus brought up is often very large, and their destructive power on the mucous membrane of the gullet and stomach is very marked.

The following cases from Beck's 'Jurisprudence' are fairly illustrative.

Two females, of the age of sixteen and twelve, each took, by mistake, half an ounce of subcarbonate of potash. Violent sickness immediately ensued, but the error was not discovered until two hours and a half afterwards. The vomiting and sickness scarcely ever ceased entirely with the elder, and she also experienced pain in the epigastric region.

Leeches were applied, and various curative means, but with little success. The vomiting, though occasionally checked, yet returned with violence, and she died in about two months after taking it. The other suffered under sickness for three days, and it then ceased. She appeared to grow better, but in a few weeks the sickness returned, and she was confined to her bed. Death ensued about three weeks after that of her sister.

The appearances of disease were similar in both, although most striking in the elder. The stomach was much thickened, and the villous coat was almost wholly destroyed; what remained was in a state of high inflammation. The pylorus in one was much ulcerated, and in the other contracted and gangrenous. The intestines were gangrenous, and adhered together by thin threads of coagulable lymph.

The treatment is to give dilute acids. The stomach-pump must be used, if at all, with the greatest caution.

Strong ammonia is a very dangerous poison. The vapour is extremely irritating, and often causes death by blocking the trachea and bronchi by the growth of a false membrane. Ammonia is among the alkaline corrosives what fuming nitric acid is among the corrosive acids, and the strong vapour has killed.

A case confirming this statement is mentioned in the 'Edinburgh Medical and Surgical Journal,' vol. xiv, p. 642. "A patient was recovering from a severe attack of fever; during convalescence he was, without any evident cause, seized with convulsions apparently of the epileptic kind, which became more and more frequent, and ultimately were so severe as to cause great apprehension of a fatal result. In order to rouse him from the stupor succeeding one of these fits, an attendant most imprudently held aqua ammoniæ to his nose, with such unwearied but destructive benevolence that suffocation had almost resulted. As it was, dyspnoea, with severe pain in the throat, immediately succeeded, and death took place forty-eight hours afterwards. In the actual condition of the patient there was little else than death to be expected, yet there is equally little room to doubt that the fatal event was hastened by this unhappy ministration."

Hamilton and Godkin mention the following cases:

Three men were killed by NH₃, caused by

upsetting of an ammonia ice machine. They were exposed to the gas for three minutes.

No. 1 was comatose and unconscious, and died in fifteen minutes.

No. 2 was delirious and unconscious (as from CHCl_3), and died in two hours.

No. 3 was conscious and walked home, but died suddenly in a fit of dyspnoea in five hours' time.

Finally I will read you a case recorded by Hilton, and given by Taylor.

A gentleman liable to attacks of fainting died in three days, after swallowing a quantity of a liquid administered to him by his son. This liquid, which was at the time believed to be sal volatile, was, in fact, a strong solution of ammonia. The deceased complained immediately of a sensation of choking and strangling in the act of vomiting. Symptoms of difficulty of breathing set in, with other signs of irritation in the throat and stomach. The mucous membrane of the mouth and throat was corroded and dissolved, and it was evident that the liquid had caused great local irritation. The difficulty of breathing was such as to threaten suffocation, and at one time it was thought that an operation must be resorted to. The state of the patient, however, precluded its performance, and he died on the third day. On inspection the viscera presented strong marks of corrosion. The covering of the tongue was softened, and had peeled off; the lining membrane of the trachea and bronchi was softened and covered with layers of false membrane—the result of inflammation; the larger bronchial tubes were completely obstructed by casts or cylinders of this membrane.

I will now take one of the caustic alkalies and go through the tests. Here is a solution of potassium carbonate, which you may be confronted with. You find that to the eyes and to the nose it is quite negative; that it is alkaline in reaction. I hold it over the spirit lamp on a piece of platinum foil, and find that it leaves a very distinct residue. There is no blackening—a fact which excludes all the alkaloids and some organic salts, such as lead acetate and tartar emetic. You know that in testing solutions you proceed methodically. I add hydrochloric acid, with which I get no precipitate, but I get effervescence, so that there is carbonic acid present, and I may practically assume that the body I was dealing with is a carbonate. The fact that there is no precipitate

with hydrochloric acid excludes lead, the sub-salts of mercury, and silver—three things which you have to deal with. Then I add a solution of sulphuretted hydrogen to the acid solution, and get no precipitate. Then I add ammonium sulphide to a solution which I know to be alkaline, and I get no precipitate. That excludes practically all the metals with which we are concerned. Next I add some ammonium carbonate, and again I have no precipitate; which fact excludes all the alkaline earths. Therefore it can only be one of two things: it is not ammonia, because it leaves a fixed residue; so that it can only be soda or potash. One of the tests for potash is platinic chloride, with which I get a precipitate; therefore I am perfectly sure that the base is potassium. Both potassium and ammonium give precipitates with platinic chloride, also some of the alkaloids.

Another test for potash is a saturated solution of tartaric acid, which will give you a precipitate. The tests for soda are mainly negative. If I had found a body which gave me a solid residue, alkaline in reaction, and I was quite sure I had fixed the alkali, and if I proved that that alkali was not ammonia and not potash, what could it be but soda? The best positive test for soda is, as I mentioned yesterday, probably the shape of the crystals of sodium nitrate.

Potassium gives a violet colour, and sodium a yellow colour to flame.

Ammonia is precipitated also by Nessler's reagent, and the alkaloids by Mayer's reagent. The difference between Nessler's and Mayer's reagents is that while they both contain potassio-mercuric iodide, Nessler's reagent contains in addition some caustic soda, whereas Mayer's reagent does not. The two reagents are very much alike, and you must remember that alkaloids are so called because in their reactions they resemble the alkalies.

The Production of Abortion by Nitrate of Silver.—Perlssee was called upon to bring about miscarriage in four cases of pregnancy complicated by nephritis and uncontrollable vomiting. He did this successfully by the introduction of a stick of nitrate of silver above the inner os uteri. The stick should project about one-half inch from the holder so as to disinfect the cervical canal as it is introduced. Pains came on in from two to six hours after the cauterisation.

University Med. Mag., November, 1898.

The Diagnosis of Nephritis without Albuminuria.—Arthur R. Edwards, M.D., at the close of the discussion of this interesting subject, summarises the chief considerations and conclusions of his paper as follows:—1. Carefully repeated routine chemical and microscopical examination of the urine every twenty-four hours usually, but not invariably, detects acute and chronic nephritis. 2. The diagnosis of the albuminuric and non-albuminuric types of the nephritides is aided by searching examination of other viscera and parts, e. g. by a disturbance of cardio-vascular changes, retinal involvement, &c. 3. These visceral or somatic changes usually present in nephritis may be lacking in concrete instances, or be capable of other or diverse interpretation as atheroma, &c. 4. The urinary findings most essential to the diagnosis of nephritis may be lacking, as many other signs and symptoms of minor dignity. 5. Nephritis may be unattended by albuminuria; such nephritis is usually interstitial in type. 6. While certain instances of non-albuminuric nephritis correspond to the type described by Dr. D. D. Stewart, yet non-albuminuric nephritis may not exactly correspond to the said type, since acute nephritis, chronic parenchymatous nephritis, and chronic interstitial nephritis may occasionally occur without albuminuria. 7. Casts should always be searched for; they are more constantly found than is albumin, yet they seem in certain instances to betoken renal degeneration rather than inflammation. They are not invariable in nephritis, nor are they invariably nephritic. 8. Future clinical caution and pathological examinations will probably increase the number of cases of non-albuminuric renal inflammations of acute, subacute, and chronic types. 9. Non-albuminuric nephritis is of a special importance in life insurance and kindred examinations and in practice, since prophylactic measures may be instituted, and the prognosis obviously influenced.

Montreal Medical Journal, October, 1898.

Phototherapeutics of Measles.—Chatinière describes four cases of measles commencing with sudden and violent symptoms, which he cured by simply having the windows hung with red curtains, shutting out the white rays entirely, and using a photographic red lantern at night. The

disease completed its evolution in twenty-four hours to four days, every symptom vanishing, and the children were up, bathed, and out of doors by the sixth day. One child complained that the light was too dim for him to play, and the parents took down the curtains, followed by an immediate return of all the symptoms, reappearance of the eruption and fever, with general lassitude, all passing away in two hours after replacing the curtains.

Presse Med., September 10th.

Massage in Cutaneous Affections.—The 'Journal de médecine' for October 9th, quoting the 'Archives médicales Belges' for July, says that Dr. Hatschek, of Kaposi's clinic at Vienna, treats prurigo successfully by centripetal effleurage of the limbs administered for from ten to fifteen minutes. With the disappearance of the condition from the limbs the cutaneous lesions of the rest of the body are rapidly cured.

Dr. Pospelow, of Moscow, has recourse to massage practised twice daily in acne of the face. Various ulcers are also said to be amenable to massage, systematically practised, in the absence of any medication other than a moist boric dressing. Dr. Bekerievitch obtained cures in about four fifths of two hundred and sixty-four cases so treated. He followed up each massage with the application of a roller bandage. Dr. Erdinger proceeds as follows:—After a complete disinfection of the limb, effleurage with the tips of the fingers, anointed with boric vaseline, is practised. Then massage of the upper part of the limb above the ulcer approaching the edge of the ulcer. Centripetal effleurage is next practised at the sides and below the ulcer. The degree of energy used is proportioned to the induration of the edges of the ulcer. When the base of the ulcer is greyish, torpid, and sanious, Erdinger advises massage of the ulcer itself. The surface is covered with a piece of linen smeared with boric vaseline, and light effleurage is practised over the dressing. Daily applications are required at first, the intervals being lengthened as the wound heals. This treatment, which permits the patients to continue their avocations, is said to effect a cure in from thirteen days to two months.

N. Y. Med. Journ., November 12th, 1898.

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* Specially reported for The Clinical Journal. Revised by the Author.

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PUERPERAL ALBUMINURIA AND ECLAMPSIA.

A Post-Graduate Lecture delivered at Charing Cross Hospital on November 17th, 1898, by

AMAND ROUTH, M.D., B.S., M.R.C.P.,
Obstetric Physician to Charing Cross Hospital, and
Lecturer on Midwifery at the Medical School;
Physician to Samaritan Hospital for Women.

ALBUMINURIA is a serious complication of pregnancy, occurring most frequently in primiparæ, and is more common in those who, from their unmarried state or from some other trouble, are in great mental distress.

To-day I am dealing with albuminuria in its relation to eclampsia, for true eclampsia is rarely, if ever, present without albuminuria. It is useful, therefore, to consider the two together.

Onset.—Albuminuria in pregnancy may have existed as a chronic nephritis before conception, in which case it is not serious, but may increase owing to the greater work given to the kidneys.

Albuminuria of pregnancy may come on at any time, but usually after the period of quickening, when the uterus is nearing the level of the umbilicus. We do not know why albuminuria should so often occur, but there are many theories which are advanced to explain the causation of this serious complication.

Dr. Herman believes that "there are at least two kinds of renal disease to which a pregnant woman is specially liable. One of these is a very acute disease, in which premonitory symptoms are either absent or of duration measurable by hours or days. It attacks chiefly primigravidæ; it often causes intra-uterine death of the child; it is attended with extreme diminution of the quantity of urine, and the small quantity of urine that is passed is greatly deficient in urea, but contains enough albumin to make it solid on boiling. This disease is accompanied with rapidly recurring fits. If it run a favourable course the fits cease; then the urine increases in amount, and the percentage

of urea in it rises. If the excretion of urea be not re-established the case quickly ends fatally. Such cases seldom, if ever, pass into a chronic Bright's disease.

"The other is a disease which attacks older subjects, chiefly those who have had children before. Its premonitory symptoms extend over a period measurable by weeks or months. It often leads to intra-uterine death of the child. It is accompanied generally by increase in the quantity of urine, with copious loss of albumin, but not so much in proportion to the urine as in the more acute disease, and with diminution in the elimination of urea, but not nearly so great a diminution as in the more acute disease. Delivery is followed by temporarily increased diuresis and increase in the urea elimination. When this increase is considerable the albuminuria usually diminishes and disappears, and the patient gets well. When the increase is only slight the albuminuria persists, and the case becomes one of chronic Bright's disease. This form of disease is sometimes attended with fits, but generally not. The presence of albuminuric retinitis affects the prognosis unfavourably. When the pressure within the abdomen is greater than usual the amount of urine may be diminished, but in such cases the diuresis and the augmentation of the urea elimination after delivery are proportionately greater."*

A brief review of some of the most plausible theories of the causation of the albuminuria of pregnancy will not be out of place.

Causation.—Halbertsma's view was that it was due to *pressure on the ureters*, and Löhlein found that eight out of thirty-two cases of eclampsia had their ureters dilated. These statements are not held to be convincing, as none of the ordinary changes in the kidney, usually following obstructed ureters, have been described, and this theory can only occasionally be the true cause. There is no doubt that there must be during pregnancy some increased *pressure on the renal vessels*, and of course the veins suffer most, and there may be some resulting congestion of the kidney capillaries. Such pressure is more probable also in first pregnancies owing to the greater tonicity of the abdominal walls, and therefore presumably a greater intra-abdominal pressure. Cases are recorded of albuminuria being diminished when

the patient was encouraged to assume Sims' or genu-pectoral position, and this backward pressure in that way lessened.

The fact that the albuminuria of pregnancy is rarely permanent, and that the deposit on boiling with an addition of nitric acid is often more paraglobulin than serum-albumin, points (in such cases, at all events) to some vascular change in the cortex of the kidney rather than to any progressive organic disease.

It must be remembered, too, that in many cases where a post-mortem has been made no structural alteration of the renal tissues has been found, or, at least, only "congestion." True "acute nephritis" is much rarer in such cases.

We cannot, therefore, exclude "pressure on the renal veins" from being one of the possible causes, though the fact that albuminuria is not common in ovarian and fibroid tumours is evidence against it. The left renal vein crosses the second lumbar vertebræ to reach the vena cava, and this is where uterine pressure would be most felt, for the veins in the rest of the course are protected by the spinal vertebræ. It is quite certain, therefore, that if this is an occasional cause it is not the sole one, for it is very unusual to find only the left kidney showing evidence of congestion. *Increased work thrown on the kidneys* is necessarily always present in pregnancy, and this theory is also advanced to explain the albuminuria; but the work is so gradually increased, and the kidneys are known to be capable of doing double work almost at a moment's notice, that the theory is not a very reliable one.

Case 1.—Mr. Swinford Edwards* narrates a case of a man whose lacerated kidney he removed after a fall from a third-floor window. On the fourth day the patient was passing 45 oz. of urine, containing 475 grains of urea.

The following case shows that *one kidney* is capable of holding its own during pregnancy:

Case 2.—Alice S—, aged 27, had her right kidney removed in April, 1896, for tuberculosis by my colleague, Mr. J. H. Morgan. Her first child was born June, 1895. She became again pregnant Christmas, 1896, and was carefully watched at this hospital for any evidence of failure in her renal functions, but no albuminuria

* 'Obstet. Soc. Trans.,' vol. xxxvi, 1894, pp. 10, 11.

* West London Medico-Chirurgical 'Trans.,' 1898.

was found at any time. Labour was induced at the thirtieth week owing to an inflamed lump, probably a tubercular gland, or perhaps a tubercular ureter, in the hollow of the sacrum, which was seriously reducing the available obstetric diameter of the pelvis. She convalesced normally.

Heightened arterial pressure does not explain the albuminuria, for the artificial production of very high renal blood-pressure by division of the renal and splanchnic nerves does not produce it. The main points in favour of all these theories is the effect of treatment, for it is known that reduction of the size of the uterus or the delivery of the child often relieves the albuminuria and prevents or arrests the eclampsia, and that venesection is found very useful in plethoric patients as a relief to arterial pressure.

It seems more probable that the albuminuria is the result of some *toxic products in the blood* which cause irritation and inflammation of the renal epithelium. This poison is known not to be urea, but may be the result of some abnormality in the process of metabolism either in the foetus or in the placenta, for albuminuria lessens and eclampsia ceases not infrequently after the death of the foetus, even before its expulsion is effected.

Schäfer* states that Schröder has proved that urea is not formed exclusively in either the kidneys or muscles, but very largely in the liver, and that urea is mainly derived from sarcosylactate of ammonia. It is possible that this or some other precursor of urea accumulated in the blood is the cause of the convulsions associated with albuminuria, especially where the liver is known to be affected, as in cases of acute yellow atrophy.

Experiments to prove the presence of toxic elements in the blood have been made by Doléris;† thus if 3 c.c. of blood-serum from an eclamptic patient be injected into a rabbit per kilogramme of body-weight, it will die, whereas it would take 10 c.c. of healthy blood-serum to produce the same effect. The following is an instance of disordered function of both liver and kidneys:

Case 3.—On October 21st, 1897, Dr. Howard Murphy,‡ of Twickenham, was sent for to see a lady, five months pregnant, complaining of cramps

in the abdomen, with faintness and vomiting. The epigastrium was tender, pulse 75, temperature normal. An enema acted well, but the colic became more severe and the vomiting more frequent. These symptoms were relieved by morphia. When she awoke after four hours' sleep these symptoms recurred, and severe convulsions ensued lasting off and on for an hour. These convulsions recurred, severe epigastric pain and jaundice were present, and the urine became very scanty, and was slightly albuminous. I then saw the patient in consultation, and, finding the os uteri dilated, ruptured the membranes and brought down a leg; morphia was injected hypodermically, and a nutrient enema administered. In three hours the uterus was emptied and washed out. The convulsions ceased, and she had a quiet night without convulsions, vomiting, or pain, so that the morphia was discontinued. No urine, however, was secreted for twenty-four hours, when 16 oz. of urine, deeply coloured with bile and loaded with albumin, was drawn off. The liver dulness was diminished, temperature 100° F., pulse 120 and very weak. On the 24th patient took liquid food and was quite quiet, but no urine was passed, and there was some diarrhoea. There was also complete loss of vision, and she rambled and was semi-conscious. Next morning 1½ oz. of urine, containing blood and bile, was drawn off, after which she died quietly in half an hour. No post-mortem could be obtained.

This was almost certainly a case of acute yellow atrophy, complicated by eclampsia.

Analysis of the urine should be made by the doctor when he is engaged to attend a pregnant woman, and, if albuminuria is found, the sulphate of magnesium test* should be tried to decide how much of the precipitate by heat and acid is serum-albumin, and how much is paraglobulin. If there is much of the latter, it points to puerperal albuminuria as opposed to chronic Bright's disease, and is therefore of much greater prognostic significance. The significance of paraglobulin forming the bulk of the precipitate in the deposit

* 'Text-book of Physiology,' 1898, p. 906.

† Dr. Blacker, 'Clinical Journal,' Oct. 30th, 1895, p. 12.

‡ 'Lancet,' May 21st, 1898.

* *Sulphate of Magnesium Test.*—To a known quantity of the sample of urine add sufficient of the salt to make a concentrated solution, and shake very briskly for some minutes. On standing a precipitate of paraglobulin is deposited. Filter, and test the filtrate with heat and nitric acid, when serum-albumin is further deposited. The deposits can be compared.

by heat and nitric acid with regard to prognosis is not yet determined, but Herman* makes the following remark:—"In the acute disease which causes eclampsia, and in the chronic disease when it is associated with excessive intra-abdominal pressure, much of the albumin is paraglobulin. The cases in which the albumin is mainly serum-albumin generally either die or pass into chronic Bright's disease." This may be the usual course of events, but fortunately is not always so, as the following cases testify.

Cases 4 and 5.—I saw two cases on the same day in a charitable institution for unmarried women about to be confined of their first child. One woman was five and the other seven months pregnant. They both had head symptoms, with about equal albuminuria and oedema. The woman five months pregnant had some retinal hæmorrhage, a somewhat severe epigastric pain, and the deposit on boiling her urine was proved to be almost pure serum-albumin. The woman who was seven months pregnant had no retinitis, no epigastric pain, and a large proportion of the deposit, on boiling her urine, was proved by the magnesium sulphate test to be paraglobulin. The former case developed eclampsia the day after she was first seen, miscarried, but did well, the albumin disappearing in a month. The latter case had labour induced at Queen Charlotte's Hospital, had no bad symptoms, and all albumin disappeared shortly after labour. The amount of urea was unfortunately not estimated.

The following is the history of a patient admitted into the hospital two days ago.

Case 6.—J. S.—, æt. 28, married, six months pregnant.

November 11th, 1898.—Felt shivery and had headache.

12th.—Headache, retching, sleepless.

13th.—More headache, vomiting and epigastric pain.

14th.—Stayed in bed, very weak, constant retching.

15th.—Headache, retching, dizziness, soreness of eyes. At 11 a.m. left arm began twitching, and then a convulsion ensued, epileptiform in type, lasting fifteen minutes. This recurred half an hour afterwards, and again at 3 p.m. and at 5 p.m. Her face was then pale and swollen. She

regained consciousness soon after, but was very weak and unable to walk.

On admission, November 15th, 9 p.m., face was swollen and pale; no retinitis. Tongue tremulous, coated; temperature 98.4°, pulse 120, full and hard. Is slightly over six months pregnant, and foetal heart is 144 per minute, regular and strong, below and to right of umbilicus. Cervix soft and slightly patent. There is some oedema over legs. Patient was ordered calomel gr. v, and bromide of potassium gr. xxx, and had a good night. Urine drawn off, and proved to have five sixths albumin on boiling and adding nitric acid.

16th.—Bowels freely relieved. Very little headache, but has been retching occasionally. Ordered liq. amm. acet. ʒj, solution of nitro-glycerine 1 per cent. ʒj every two hours.

Is much better, oedema of legs much less, no retching, is perspiring well.

Urine contains only three quarters albumin, with Esbach 6 per cent. Is cloudy, acid, free from urates, 1022. Amount urea = 2.25 per cent., no sugar. No paraglobulin. Quantity of urine is 17 ounces.

Microscopically triple phosphates and amorphous urates, and a few solitary granular casts.

[22nd.—The patient has had no more fits, has no epigastric pain nor headache. Has passed 86, 86, 68, 64 ounces of urine these last four days. Urea = 1.25 per cent. Albumin now about 1½th. The pregnant uterus has been uninfluenced, and the foetus is living.]

There are certain symptoms in a pregnant woman which suggest albumin, such as headache, giddiness, sleeplessness, impaired eyesight, oedema (especially facial), and any of these should demand an analysis of the urine.

A transient albuminuria may indicate the onset of insanity of pregnancy, or some form of paralysis, and if it is very marked and progressive it more usually indicates eclampsia. Only four or five out of each twenty albuminuric patients become eclamptic (Parvin).

Albuminuria often causes death of the foetus, which is probably productive of good to the mother by preventing further bio-chemical changes in the blood, and, by leading to abortion, is a cause of hæmorrhage, which lowers arterial pressure, and lessens the risk of eclampsia.

* Obstet. Soc. 'Trans.,' vol. xxxvi, 1894, p. 49.

The routine *treatment* of the albuminuria of pregnancy should be rest in Sims' position and warmth. Wet packing or Turkish baths are useful. Milk diet. Reduction of arterial pressure by hydragogue purgatives, and encouragement of perspiration by liquor ammoniæ acetatis in ounce doses every two hours. Solutions of nitro-glycerine 1 per cent. ℥ every two hours, with bromide of potassium and ammonia, may usefully be given to reduce arterial pressure and to lessen the nervous tension of the patient.

If the albumin gets more abundant and the urea excreted is seriously and progressively diminished, and especially if a rather severe epigastric pain should supervene, or if albuminuric retinitis or retinal hæmorrhages are found, premature labour should be promptly induced to prevent the onset of eclampsia. It must, however, be remembered that not infrequently eclampsia comes on without any premonitory symptoms having been observed (Case 6).

Eclampsia.—One undoubted reason why eclampsia is so frequent in the albuminuria of pregnancy is because the nervous system of the patient is proved to be then very hyperæsthetic, and nerve-storms are apt to arise. This nervous tension of excitability is also seen in the tendency to insanity and other nervous affections in women who have, till the pregnancy, resisted their inherited tendencies to those troubles (Dakin).

The Traube-Rosénstein theory of cerebral anæmia has not been proved. The convulsions are, I believe, usually due to accumulation of toxic elements in the maternal blood, derived, as formerly stated, from some abnormality in the process of foetal or placental metabolism. Clifford Allbutt* points out that "toxines in such cases tend to expend their malignant powers largely upon the nervous system, and in proportion as the nervous system is developed, these effects become the more perilous."

Diagnosis of eclampsia.—The convulsions of eclampsia have to be distinguished from those of hysteria, epilepsy, and apoplexy.

How should we proceed to determine the nature of the attack in a woman who has a convulsion during an otherwise normal first stage of labour, or before labour has really begun?

If it is eclampsia you will probably get no history of past epileptic fits, but there may have been recent head symptoms, and there is usually œdema. Even if partly comatose, give the patient chloroform before examining further. Then pass a catheter, and you will probably succeed only in obtaining a few teaspoonfuls of blood-stained urine, which becomes nearly solid on boiling. This alone would almost prove it to be a case of eclampsia; and if the arterial tension is high, if retinitis is seen with the ophthalmoscope, if the fits recur and the temperature rises, you may with absolute certainty exclude epilepsy and hystero-epilepsy, and if there is no paralysis you can exclude convulsions of cerebral origin.

Prognosis.—The maternal mortality of eclampsia is 50 per cent. in cases beginning before the onset of labour, 25 per cent. in cases which begin during labour, and 8 per cent. in those which begin after labour (Galabin). The death of the mother may be due to exhaustion whilst in a state of coma due to the recurrence of the convulsions or to the intensity of the toxæmia. Sometimes patients die from hyperpyrexia, as in the following case:

Case 7.—Mrs. T—, primipara, was seen by me in 1882, during the first stage of labour. There were then no premonitory symptoms of eclampsia, and her urine had been found normal four weeks previously. In two hours I was again sent for, and found she had been convulsed, and was then semi-comatose. Chloroform was given, and about an ounce of bloody urine drawn off, which on boiling deposited three-fourths albumin. Fits recurred every half-hour, and the patient became deeply comatose. During the fifth fit (three hours after the first) the child (living and healthy) and placenta were born. Despite the ice-cap, the temperature rose steadily to 105.5° F. ten hours after the first fit; and, in spite of packing and ice-sponging, rose during the twelfth hour to 107° F., with pulse 150 and respirations 44. An ice-bath reduced these figures to 102.5°, 120, and 32 respectively, but the coma was still profound. The convulsions, fifteen in all, now ceased, and chloroform was discontinued. The recurrence of the fit was marked only by a change of colour in the face and twitching of the fingers. Two teaspoonfuls of tincture of digitalis caused the secretion of a pint of albuminous urine in about three hours, none having been secreted during the

* 'Lancet,' February 27th, 1897.

previous twelve hours. During the next twelve hours the temperature varied from 102° to 105° , and was controlled by cold sponging. Venesection had failed owing to the semi-solid state of the venous blood. Forty-eight hours after the onset pulmonary thrombosis occurred, and she died suddenly in twenty minutes, although one had by this time become quite hopeful of a happier termination. Two hours after death the temperature in the axilla was 109° F.

More rarely such patients die of asphyxia during the tonic stage of the convulsions, or as the result of being treated rashly by the administration of jaborandi, or its active principle pilocarpine, which cause such large quantities of rosy mucus to be secreted from the bronchial tubes that suffocation has been produced. The coma of eclampsia increases in depth and duration after each fit, until the patient has no interval of consciousness at all. Towards the end, as coma deepens and weakness increases, the convulsions themselves get much less violent (see Case 7).

If patients recover from the eclampsia they may die of other complications, such as septicæmia, insanity, post-partum hæmorrhage, pulmonary thrombosis, or embolism. The children are in great danger, at least 50 per cent. dying if the eclampsia supervenes before labour has begun. They usually die of asphyxia, but may die as a result of deficient maternal excretion of (?) urea, though there is some reason to believe that the foetal liver and kidneys to some extent protect the foetus against this maternal uræmia. If there be hyperpyrexia the foetus soon succumbs, as its temperature is normally about a degree and a half above that of the mother.

Treatment.—If labour has not begun, or is only just beginning, adopt palliative measures till you are sure whether the fits recur. After making the diagnosis of eclampsia keep up the chloroform inhalations, and give either chloral 30 grains *per rectum*, or $\frac{1}{2}$ grain morphia hypodermically. The Sims position is a useful one to relieve backward pressure of the uterus.* If the patient is very plethoric, bleed freely (15 to 25 ounces) from the

arm, and give two drops of croton oil in glycerine. As soon as the cervix is dilated and relaxed (and this soon occurs, for the uterine muscles partake of the muscular contractions) puncture the membranes; and if the fits do not lessen or cease, and prompt delivery is indicated, a De Ribes bag should be inserted and delivery effected by forceps under deep anaesthesia. Puncturing the membranes and subsequent delivery of the child (if further indicated) diminishes any direct pressure on the renal vessels and reduces the intra-abdominal pressure, and with it the general blood-pressure.

If the head of the child is low down in the vagina, and the fits are continuing, forceps should be unhesitatingly applied under deep anaesthesia, and the child delivered promptly. Some would even perform craniotomy to facilitate delivery in severe cases, especially if there is reason to believe the child is dead. It is remarkable, however, as already stated, that the death of the child *in utero* is sometimes sufficient to arrest the convulsions without delivery being effected.

Acute Articular Rheumatism.—As early as 1891 P. A. Achalmé announced that he had found the bacterium of acute articular rheumatism. He published in the 'Annales de l'Institut Pasteur,' 11, 1898, p. 845, "Recherches bactériologiques sur le Rheumatisme aigu," a more detailed report concerning the bacterium in question. He describes it as an anaërobic bacillus, which resembles in its general morphology the bacillus of anthrax. One interesting fact relative to its biology is that when grown in urine it decomposes this, throwing down extensive deposits of urates. In a more recent communication — "Recherches sur l'Anatomie Pathologique de l'Endocardite Rheumatismal," 'Archives de Médecine Experimentelle et d'Anatomie Pathologique,' 10, 1898, p. 370, he reports having found the organism again in two cases, and he controverts the statement that this bacillus is one of the putrefactive post-mortem anaërobes. Finally, in the 'Archives Russes de Pathologie, de Médecine Clinique, et de Bactériologie,' 5, 1898, p. 613, Sawthenkow completely confirms this. The organism elaborates negative chemotaxic substances, and also products which produce necrosis in animal tissues. The characteristic endocardium lesions he believes to be due to these latter substances.—*Medical Record*, Nov. 12th, 1898.

* Case 8.—Dr. C. H. F. Routh (Obstet. Soc. 'Trans,' vol. xxiv) relates the case of a woman who had eclampsia during labour, and to whom he administered chloroform without benefit. Prolapse of the cord led him to make the patient assume the genu-pectoral position, when the fits ceased, and did not recur.

THE CAUSES AND TREATMENT OF SLEEPLESSNESS.*

BY

G. BERTRAM HUNT, M.D., M.R.C.P.

I HAVE chosen, Sir, to talk about the treatment of sleeplessness not because I have anything particularly new or original to say on the matter, but because it is a subject of great practical importance notoriously difficult to deal with, of which some of you will have considerable experience when in residence in a hospital. For many, too, the subject has a personal as well as a professional interest. There are few conditions which are so common, and at the same time so distressing and harmful if left untreated, as sleeplessness. It is said that want of sleep, as produced artificially on their victims by those epicures of torture, the Chinese, will by itself kill a healthy man under a fortnight. In dogs, absolute loss of sleep, produced experimentally by Hanassein, proved fatal in four or five days, the animals dying more quickly than if they had been completely starved. One knows, too, from clinical experience that two or three sleepless nights will often give an unfavorable turn to an acute disease previously without danger.

Insomnia being such a frequent and urgent symptom, we have for it a long and rapidly increasing array of remedies known as hypnotics. Most of them are drawn from organic chemistry, the eagerness with which they are sought being indicated by their extremely complex chemical composition—that of trional, for instance, being dimethyl-sulpho-methyl-ethyl methane. When first introduced, each of these drugs is usually vaunted in a delightfully impartial way as an almost infallible cure for sleeplessness, so that a student thinks that he can treat insomnia by trying the drugs recommended, if necessary, one after the other on the unfortunate patient. But as insomnia is merely a symptom caused by many different and often opposite conditions, there can evidently be no specific remedy for it; but the rational treatment must depend entirely on the cause. I propose this

evening to mention the chief causes of sleeplessness, and briefly consider the treatment appropriate to each class.

For our purpose we may consider sleep to be physiologically a state of quiescence of the higher nerve centres, associated with, perhaps due to, anæmia of the cerebral arteries. As most observers deny any local vaso-motor mechanism to the cerebral vessels, such cerebral anæmia can only be brought about by the dilatation of the arterioles of the splanchnic area and skin, a certain amount of blood being thus withdrawn from the cerebral vessels. This dilatation of the peripheral vessels has been proved to take place during sleep by Mosso's plethysmograph experiments on the limbs. The fact that sleep is accompanied by anæmia of the retinal arteries was first shown ophthalmoscopically by Hughlings Jackson; and it is generally agreed that the state of the retinal vessels is a fair indication of that of the intra-cranial circulation. You may notice that I have been careful to speak of anæmia of the cerebral arteries only. The cranial cavity being an enclosed space by the Munroe-Kelly theory, supported by Dr. L. Hill, it must always contain approximately the same amount of blood, so that if there is less in the arteries there must be more in the veins. In fact, direct inspection of the cerebral cortex in a rare case, where an accident rendered it possible, showed that during sleep the arterioles were small, while the veins became fuller, so that the brain substance was darker. That this anæmia of the cerebral arteries is a cause of sleep, and not merely a concomitant, is suggested by the fact described by Dr. Leonard Hill in his book on the cerebral circulation—that if some of the arteries supplying the circle of Willis are ligatured, the animal becomes drowsy.

Any condition, then, which interferes with the state of rest of the higher nerve centres, or prevents the normal lessening of blood-supply to the brain, will *ipso facto* inhibit sleep.

This vaso-motor theory of sleep, which rests on the basis of experiment, has considerable importance, as we shall see, from a therapeutical point of view; while other theories, such as that of withdrawal of the dendritic processes of the nerve-cells during sleep, rest on a less secure basis, and do not help to explain therapeutic results.

Drugs producing sleep have been divided into

* An Address delivered before University College Medical Society, November 2nd, 1898.

two classes, viz. the direct hypnotics, the best examples of which are the bromides, acting directly on the nerve centres, probably on the nerve-cells themselves; and the indirect hypnotics, a much larger class, which act by cutting off peripheral stimuli or by controlling the circulation and inducing cerebral anæmia. This classification, however, has very little practical importance. Cases of insomnia fall naturally into two main groups—(1) cases of simple insomnia, in which there is no definite disease present; (2) sleeplessness dependent upon some disease of one or other of the organs of the body.

To consider first the group in which sleeplessness itself is the chief departure from health. It must be remembered that much depends on the personal factor or idiosyncrasy of the patient, and just as some people can do with much less sleep than others, so some are kept awake by trifling causes which would not affect persons with more stable mental equilibrium. In some insomnia may be traced to want of proper exercise, and sufficient sleep may be obtained if the sufferer takes up some regular form of exertion in the open air, such as walking, riding, or cycling. Others who sleep miserably in London more successfully woo "nature's sweet nurse" after a few days' continuous exercise in the country. Another frequent cause of insomnia, especially in old people and anæmic women, is cold. On going to bed such persons may be kept awake for hours by coldness of the skin, especially by cold feet. Coldness of the skin leads to contraction of the peripheral vessels, with accumulation of blood in the internal organs, which is said to interfere with the anæmia of the cerebral arteries necessary for sleep. Such people can usually obtain sleep by the use of hot water bottles, or if necessary by fomentations and poultices to the abdomen. Sleep may sometimes be procured by taking a light meal and a glass of hot milk or toddy, or by having a hot bath just before going to bed; the explanation being that blood is drawn to the abdominal viscera or skin by these measures, which thus favour cerebral arterial anæmia. Another method of dilating the peripheral vessels, more applicable to young and vigorous patients, is the temporary application of cold to the surface; the constriction of the cutaneous vessels thus produced giving place to marked dilatation as soon

as the subject enters the warm bed. Some apply cold in this way by walking about the room for a time in a nightshirt; others dip the feet in cold water and then use vigorous friction. Dr. Donald MacAlister, of Cambridge, states that he has frequently cured the insomnia of over-worked students by ordering a nightshirt soaked in cold water to be worn under flannel pyjamas, the shirt thus acting as a temporary cold pack.

Worry, mental anxiety, and overwork are, however, very common causes of insomnia in all classes; the habit of sleeplessness in many cases being aggravated by brooding over the troubles and work of the day after retiring to rest. Many such people fix their attention on some mechanical mental process such as counting imaginary sheep, or repeating verse in order to divert their thoughts from their troubles.

Regular hours, daily exercise, the avoidance of work in the evening, and if necessary change of air and scene, will usually relieve such persons without having recourse to drugs.

Another class of patients who complain much of insomnia are those suffering from hysteria. In dealing with such persons it is as well to make sure of catching your hare before proceeding to cook it; I mean to ascertain if the patient is really sleepless before commencing treatment. Nothing is more common than to have such persons complain bitterly of not getting a wink of sleep, when on inquiry one learns that there really were several hours' good sleep.

In cases of true insomnia with hysteria, massage, especially when combined with the strict regimen of Weir Mitchell's system, will often work wonders; the massage acting as a form of exercise for people who will not take it in any other way.

In all these cases of insomnia apart from organic disease, drugs must be avoided as far as possible, owing to the great temptation to recourse to them again and again in increasing doses till at last a pernicious habit is formed. For this reason morphia and chloral, the two drugs which are most likely to be taken habitually, should never be used. If a hypnotic is absolutely essential, perhaps the best, simplest, and most free from toxic effects are bromide and trional, and their usefulness is rather increased by the fact that they both have a salt, rather unpleasant taste. But even these drugs must be discontinued as soon as

possible; bromides, especially when taken continuously in large doses, tending to produce gradual mental deterioration. Sulphonal is a drug which is freely used in such cases, as it seldom fails to procure sleep; but it is by no means free from drawbacks, and should be reserved for very obstinate insomnia. Alcohol, especially when taken hot at night, is a most valuable drug, but it too must be used cautiously. Vague directions must be carefully avoided, and the exact dose carefully prescribed like any other drug. If not, it will be often found that the doctor's advice is used as an excuse for constant nipping. It probably acts mechanically by dilating the splanchnic and peripheral vessels and so drawing blood from the cerebral circulation, and also psychically by calling up pleasing thoughts to replace the previous gloom.

Of the treatment of insomnia by hypnotic suggestion I have no experience, but in proper hands it may be useful in certain cases.

We must now turn to insomnia of the second class—that caused by some definite organic disease, in the treatment of which drugs may be more freely used. One of the commonest varieties of this form of sleeplessness is that due to pain. The strong peripheral stimuli either directly irritate the higher nerve centres, preventing the lapse into sub-consciousness, or perhaps, as suggested by Dr. L. Hill, pain produces insomnia indirectly; painful stimuli, by their action on the vaso-motor centre, preventing the general vascular dilatation necessary for the production of cerebral arterial anæmia. If the pain is due to visceral disease, such as cancer, acute inflammation, biliary or renal calculi, nothing relieves it so quickly as one third or half a grain of morphia subcutaneously, sleep often appearing five minutes after the injection. In chronic cases, especially of malignant disease, chlorodyne at night in half-drachm doses is very valuable. In dealing with certain nerve pains, however, such as severe headache, neuralgia, lightning pains of tabes, pain of cerebral tumour, one should not use morphia if possible, these cases being more chronic, and therefore liable to contract the morphia habit. Here we should try such drugs as phenacetin, antifebrin, exalgin, butyl-chloral hydrate, and gelsemium, of which I have seen most good from phenacetin (ten grains) or tincture of gelsemium (half a drachm

frequently repeated). The treatment of pain being a large subject, I must pass on to consider the insomnia of the acute fevers. One should carefully inquire for the amount of sleep obtained by every fever patient; and if that is deficient, try and remedy it promptly. The first signs that the patient is not sleeping properly, as typically seen in certain cases of enteric fever, are increase of the delirium, which may become maniacal, rise of the pulse rate, drying and furring of the tongue. If the sleeplessness continues for two or three nights, collapse appears, and the patient sinks into the so-called ataxic or adynamic condition. The delirium, which was previously active, becomes of the low muttering type; twitching and tremor of the limbs appear, the pulse becomes running in character, and, in fact, the whole aspect of the case changes for the worse. Valuable aids in procuring sleep are measures directed towards the temperature. As you probably know, it should be a general rule that all fever patients should be sponged all over every evening with tepid water, to which a little spirit or eau de Cologne may be added, the slight cooling of the skin thus produced greatly favouring sleep.

In some cases, especially when delirium is present, this will not be sufficient, and the patient should then be sponged with ice-water, and an ice-bag or Leiter's tubes applied to the head; or an ice-pack may be employed. It should never be forgotten that these cases of febrile insomnia always require stimulants, eight or ten ounces of brandy in the twenty-four hours being a very efficient aid in producing sleep. The best of the simpler hypnotics for these cases, as far as my experience goes, are trional or chloralamide in thirty-grain doses, with half an ounce of brandy to dissolve it, the latter drug having many of the advantages of chloral with no depressing action on the heart. In case of failure, too long trial must not be made of them; if the patient remains sleepless for two or more nights, opium or morphia should be at once given. It is said that opium is best given by the mouth, but it seems to me to be less likely to interfere with digestion and to be more certain in its action if given hypodermically. You must remember, however, that many authorities will not allow opium for the sleeplessness of fever, especially in typhoid fever. Some raise the objection that opium does not produce a

natural sleep. Well, it may be quite true that, as Iago says—

" Not poppy, nor mandragora,
Nor all the drowsy syrups of the world,
Shall ever medicine thee to that sweet sleep
Which thou owed'st yesterday."

Still, half a loaf is better than no bread, and sleep obtained by opium is much better than none. Any one who has ever seen the beneficial result of a night's sleep obtained by opium on a previously sleepless fever patient, will not hesitate to use it when required. For the delirium and insomnia of acute pneumonia Sir D. Powell lays stress on the value of free stimulation and morphia, and Dr. Fowler records a case in which morphia apparently saved the life of a patient suffering from apical pneumonia with delirium and hiccough. Chloral should, as a general rule, be avoided in fever cases, but may sometimes be advantageously combined with opium; the depressing effect of chloral on the cardiac system being counteracted by that of opium. The danger of cardiac depression from chloral seems, however, to be greatly exaggerated. Chloral lowers arterial tension not by depressing the heart, but by causing vascular dilatation. I have never seen any bad effects from chloral in several hundred cases in which it was employed. During convalescence from acute diseases some persons suffer much from nocturnal restlessness, which may be met by forbidding much sleep during the day, and, if necessary, by bromide of potassium—a very useful drug in such cases.

Patients with cardiac disease, especially when failure of compensation supervenes, often suffer much from insomnia. They are unable to lie down in bed, and are kept awake both by their dyspnoea and by a peculiar nocturnal restlessness. It will be often found that these patients will sleep better leaning forward in a chair than propped up in bed. Paraldehyde in drachm doses is here a very serviceable drug; it is very safe and fairly rapid in its action, its great defect being the unpleasant taste and after-odour. This may be partly met by giving it in capsules or by the rectum. It does not, however, relieve pain; and if this is severe, morphia, which agrees well with heart cases, will be invaluable.

The treatment of insomnia in diseases of the respiratory system needs very careful management. The insomnia is frequently due to cough. When

expectoration is profuse, as in some cases of phthisis or acute bronchitis, if the cough is suppressed for long the secretion accumulates, and the patient may be actually drowned by his own expectoration. For this reason strong hypnotics are contra-indicated; I have certainly seen the end of several phthisical patients hastened by the use of morphia to produce sleep. Only those drugs, such as bromide, trional and chloralamide, which produce a fairly light sleep, must be employed; so that the patient can be roused occasionally to expectorate. Hypnal, a combination of chloral and phenacetin, is specially recommended for these cases; but I have had no personal experience of it. In phthisis, sleep is often disturbed by night sweats; and if these can be checked by atropine, oxide of zinc, or picrotoxin, the patient will have a better night. It will often be found that a patient with febrile phthisis will sleep better if the fever is controlled at night by such antipyretic drugs as phenacetin, antipyrin, or guaiacol, which are here quite admissible, though in my opinion harmful in the more acute febrile diseases. The sleeplessness due to spasmodic asthma is only relieved by treatment directed to the asthmatic paroxysm.

Insomnia is a common symptom of chronic Bright's disease, especially of those varieties with marked cardio-vascular changes; the high arterial tension and rigid vessels probably interfering with the requisite cerebral anæmia.

Insomnia with severe headache and dyspnoea is often seen at the commencement of the more chronic forms of uræmia. Chloral is said to be the ideal hypnotic for patients with Bright's disease, as it reduces arterial tension as well as acts as a sedative to the central nervous system. Morphia is, of course, generally tabooed in nephritis, but I think from tradition only, and without good reason; I have certainly seen very good results with it in many cases in Dr. Ringer's ward. Dr. Ringer tells me he has used morphia for years with benefit for the insomnia of kidney disease even if due to uræmic dyspnoea, and has kindly referred me to a short paper of his on "Morphia in Bright's disease," in the 'Journal of the American Medical Association' for October, 1898, in which he quotes such undeniable authorities as Loomis, Austin Flint, Bartholow, Stephen Mackenzie, Park, Douglas Powell, and

Osler, as using morphia with benefit even in acute uræmia. It seems, then, that the prejudice against opium in such cases has no good foundation; the only contra-indication for its use is dyspnœa due to hydrothorax or pulmonary œdema; but that the majority of cases of uræmic dyspnœa are due to such pulmonary conditions, and therefore unsuited for opium, as some would persuade us, seems to me absolutely unproved.

Akin to the insomnia of Bright's disease is that met with in old people with very diseased and rigid vessels, and chloral is recommended for the same reason. Sir Russell Reynolds had good results in senile insomnia with extract of cannabis indica in one quarter to one third grain doses. Bromidia, a therapeutical blunderbuss containing tinctures of cannabis indica and hyoscyamus, chloral, and the bromides of ammonium and sodium, being so highly charged, can hardly fail to hit the mark sometimes.

Many patients with chlorosis or any severe anæmia sleep badly at night, though throughout the day they are drowsy. This is explained by the loss of vascular tone and feeble cardiac action, allowing the undue accumulation of blood in the lowest part under the influence of gravity, so that during the day, while the body is upright, the blood collects in the legs and abdomen, the brain being anæmic and the patient therefore sleepy; on the other hand, when the patient lies down more blood passes through the cerebral circulation, consequently wakefulness is produced. One obvious objection to this theory is that, although arterial tension is low in many forms of grave primary or secondary anæmia, yet in chlorosis, where insomnia is equally common, it is usually raised. Whether we accept or not this rather too mechanical explanation, the indications for treatment are clear. Hypnotics must only be given temporarily, and free use made of tonics, especially iron, arsenic, and digitalis. Closely allied to this form is the insomnia so often met in exophthalmic goitre. The patient is restless, nervous, and anæmic, all which factors tend to produce sleeplessness.

The cause of many cases of sleeplessness may be found in dyspepsia, especially the chronic form met with in middle-aged people, and often associated with chronic gout. The patient frequently gets to sleep successfully, but wakes in

the early morning, sometimes with intense craving for food, sometimes with flatulent distension of the abdomen, perhaps accompanied by vomiting. A few drops of capsicum or oil of cajuput on sugar, or if necessary a draught of tincture of nux vomica with twenty grains of bromide, will usually give relief. Patients with nocturnal bulimia or craving for food, usually have to sleep with biscuits and milk by their bedside, which they can consume on waking. Digestive troubles are far the most common cause of sleeplessness in infancy. A healthy infant, by a merciful decree of nature, ought to be nearly always asleep when not engaged in taking nourishment, and if anything interferes with this beneficent arrangement it is usually gastro-enteritis, which must be treated in the usual way.

Sleeplessness connected with disease of the nervous system is usually either due to pain, as the headache of cerebral tumour or meningitis, or to constant muscular movements or spasms, such as that of chorea gravis, tetanus, and hydrophobia, and more rarely in paralysis agitans, in any of which diseases sleep may be entirely prevented, with fatal result. The movements of disseminated sclerosis being an intention tremor, usually cease when the patient is quiet, and so do not interfere with sleep; but I have seen two cases of post-hemiplegic chorea in which the jerky muscular contractions were so constant and severe as to greatly interfere with repose. For checking all these muscular contractions no drug is so valuable as chloral. In a severe case, such as some forms of chorea gravis, it may be given whenever the patient wakes up, so as to procure sleep almost continuously for a week or ten days.

Mental disease is a frequent cause of insomnia, but the only form which I have had much opportunity for watching is delirium tremens. This disease usually is ushered in by two or three sleepless nights, and there is no doubt that if sleep can be procured at this stage the attack may be averted. For this purpose full doses of bromide of potassium with chloral are very successful. In the stage of fully-developed delirium, sleep is much harder to procure. Chloral and bromide are now practically useless, and I cannot say that I have seen much good from morphia, though it is generally recommended. Opium having a stimulating effect on the heart, often produces excitement

instead of sleep in cases of active delirium; and on this account it is well to recall the dictum of Graves that opium should never be given alone in conditions of excitement. He combined it with antimony to depress the heart; but we should hardly admit such a depressing drug now-a-days in the treatment of delirium tremens. The drugs that I should rely on most to produce sleep in delirium tremens or in any very intractable form of insomnia, are sulphonal and hyoscine; but it must be understood that they are both dangerous drugs, and must be reserved for very severe cases. Sulphonal should be given in full doses of forty grains, either in an emulsion or hot solution. Owing to its insolubility it is so slowly absorbed that it must be given four or five hours before night; and one dose will often make the patient sleep twenty-four hours or even longer. Not infrequently, however, toxic effects such as cardiac failure or coma are seen, which may even prove fatal. The appearance of a dark pigment, hæmatoporphyrin, in the urine is of less importance. Hyoscine should be given subcutaneously in doses of $\frac{1}{100}$ to $\frac{1}{50}$ of a grain. In some cases it acts speedily; but unfortunately its effects are very variable, and toxic symptoms, especially respiratory failure, are common.

I have neither sufficient experience nor time at my disposal to embark on the subject of insomnia of mania and melancholia or that seen in many cases of skin disease, but must bring these rather disjointed remarks to a conclusion. I am only too well aware that I have considered this important subject in a very incomplete manner, which was partly necessitated by the endeavour to confine myself as far as possible to methods of treatment which I have personally observed. I can only hope that some of you who have so kindly listened to me will fill up the gaps by laying before the Society the results you may have observed with these or other methods of treatment.

Local Anæsthesia.—Guaiacol in solution in oil has been used by Lucas Championniere as a local anæsthetic, especially for the turbinals and for the removal of polypi in the nose and ear. Anæsthesia is obtained in ten minutes.

Cleveland Journal of Medicine.

CONSTIPATION AND ITS MODERN TREATMENT.

BY

GEORGE HERSCHELL, M.D.Lond.

(Concluded from p. 90.)

THERE are several other exercises which are of extreme use in strengthening the abdominal muscles. One of the best is the following:—Take a moderately heavy pair of dumb-bells, and place one on each side of you on the floor close to the outer side of the feet. Stoop and grasp one in each hand. Gradually straighten the back until you are quite erect, letting the hands hang at full length by your sides. Now bend each arm until you have brought each dumb-bell into the corresponding armpit. Still keeping them in this position, raise yourself upon your toes. Lower yourself gradually, and reverse the successive movements until the bells are again upon the floor.

Another exercise I have found of especial value in those inclined to *embonpoint*:—Take a moderately heavy pair of dumb-bells, one in each hand, and lower them in the direction of your toes until they are as near the floor as you can manage to get them. All the bending of the body to effect this must take place from the hips. Now try and imagine that you are turning the handle of a crank *towards* you. Keeping the arms stiff and well away from you, straighten your back as if you were rowing. Now bend the arms and bring the dumb-bells in to your chin, and lower them until they arrive at the first position. You must make the movement in as circular a direction as you are able.

The effect of this exercise upon the abdominal muscles is very marked.

A very good and simple apparatus for strengthening the abdominal muscles may be made with very little trouble as follows:—Procure two pieces of thick rubber cord of about a couple of feet in length, and attach a small metal ring to the two ends of each. Screw a couple of hooks about three feet apart into the wooden framework over any ordinary door. Cut a length of broomstick of three feet in length, and drive a brass-headed 4-inch nail into each end, so that it projects about an inch.

To put up the apparatus for use, open the door, hook the rings at one end of the rubber cords over the hooks above the door, pass the heads of the nails at the extremity of the wooden bar through those at the other ends, and you have a kind of trapeze hanging in the doorway. To use the apparatus, grasp the bar in your hands, walk through the doorway until the bar is raised above your head, and bend the body downwards, bringing the arms forward in front of you. The arms must be kept extended in the line of the trunk, and the pull must be given with the abdominal muscles, and not by contracting those of the arm. The effect is that the abdominal muscles are thrown into firm contraction by the resistance of the rubber cord.

The best time for doing exercise is immediately on rising in the morning, and again on retiring at night. No one movement should be performed such a number of times as to produce subsequent stiffness, the rule being to leave the particular exercise upon which you are engaged the moment a slight sensation of fatigue is felt, and pass on to the next. In patients with very weak abdominal muscles, once, is quite sufficient for each exercise at first, and it must be carefully borne in mind that the movements must be executed slowly and smoothly, and without any jerking. As the patient gains in strength the number of times for each movement may be cautiously increased. It is well to follow the morning exercise by any hydrotherapeutic procedure of which you may be making use.

In the treatment of constipation and other conditions of atony of the abdominal viscera cycling is of the greatest service, and this may perhaps be accounted for by the fact, discovered over twenty years ago by Braune,* that flexions and extensions of the hip-joint greatly increase the flow from the crural vein into the inferior vena cava. This is apparently effected quite mechanically by the arrangement of the layers of fascia with which these vessels are in relation. Not only is the lymphatic circulation facilitated, but the functions of the abdominal viscera are stimulated by the increased amount of oxygen which is carried to them. Incidentally the peristalsis of the intestinal tract is increased, as it is a well-known fact that the same is hindered by carbonic acid and heightened by oxygen.

* Braune, 'Die Oberschenkel Vene in anatomischer und klinischer Beziehung,' Leipzig, 1873.

The next point to be considered is

The Regulation of the Diet.

It is impossible to arrange a diet for any patient unless we have a clear idea of the *raison d'être* of our recommendations. In treating constipation the indications obviously are to avoid those articles which are likely to produce constipation, and to secure a sufficient quantity of substances known to have the opposite effect. But we may save ourselves a great deal of trouble if we will try and reason out *why* some articles of diet are constipating in their tendency and some the reverse. We generally know, in a vague kind of manner, that such and such a food is constipating, but how many of us can give a coherent reason for the faith which is in us? A little study will do much to elucidate the subject.

We will first take substances which are known to constipate. They will probably owe this property to one or more of the following characteristics:

1. They contain tannin or analogous astringent substances. As examples of this class we may take the red wines of Burgundy or Bordeaux.

2. They may be deficient in cellulose. In this group we find the prepared cereals and other farina—rice, tapioca, sago, and potatoes.

3. They possibly are articles difficult of digestion, which tend to form accumulations in the intestines, and yet are not irritating enough to increase peristalsis. Most of these—such as nuts, dried beans and peas—contain vegetable albumin.

4. They may contain lime salts. Lime appears to usually produce this effect, even in very small doses, probably through some action upon the nervous system, and it is probable that many more cases of obstinate constipation are directly due to drinking hard water than we think.

5. They may be examples of articles of diet which produce constipation without any apparent reason in certain individuals having an idiosyncrasy towards them.

Per contra, other articles of food are laxative because—

1. They contain a considerable amount of cellulose. Rye, oats, bran, cabbage, asparagus, spinach, and carrots are examples of this class.

2. They contain purgative salts and sugar. In this group we find prunes, apples, tamarinds, sauerkraut, and orange marmalade.

3. They contain hepatic stimulants. As an example we may cite the dandelion, which forms one of the most wholesome salads with which we are acquainted, and one withal of a slightly medicinal character.

4. Articles of food which are rich in fat are distinctly laxative.

With the knowledge which we have now systematised we shall have no difficulty in arranging such a diet table as will place the patient in the best conditions *quâ* diet for procuring a daily action of the bowels. We must not forget to give plenty of fluid, and to take care that the water which the patient drinks is not hard. In places where the water-supply is of this nature it is as well to insist that the patient drink only distilled water, and we must take care that this is used not only for actual beverage, but also to wash all vegetables and salads, for boiling potatoes in, and, in short, for all culinary purposes, or we shall do very little good by our prohibitions. Buttermilk is an excellent laxative drink, as it contains both fat and milk-sugar.

But although the problem of finding a suitable diet is not by any means a difficult one when the stomach is healthy, it is far otherwise when we have to do with atony or dilatation or gastric catarrh. It is unfortunate that almost all food which is rich in cellulose is of a bulky nature, and, as a rule, not dealt with in the stomach. We are, therefore, in the awkward dilemma that if we prescribe the diet which is theoretically indicated by the constipation, we shall be giving the very worst food which we can possibly select for the gastric condition. We must, however, do the best we can, and after taking care that the diet we give contains the minimum of starch, and plenty of fat, administer the necessary cellulose in as concentrated a form as possible. Fortunately the last indication is rendered possible by the administration of common bran. The use of the substance for the purpose in question has been persistently advocated by Professor Turck for many years, and I can warmly endorse his recommendation. It must first of all be heated in an oven at 150° C., to partially cook it and to render it perfectly sterile. It is then passed through a coarse wire sieve. It may now, with the aid of a little flour, &c., be made into small biscuits and baked in iron patty pans, or, as I prefer, a measured dose may be stewed

with apples or other fruit and given once or twice a day. It may also be taken suspended in milk or taken in cachets. Its action upon the gastrointestinal tract is very marked, removing mucus mechanically and stimulating peristalsis.

In these cases of constipation *only*, which are complicated with a dilated or atonic stomach, it is good scientific practice to give a daily laxative drug. As we are precluded in many cases from giving food of a character that will stimulate the intestinal peristalsis, we must add to the food a small dose of some substance such as aloes or cascara, which will just supply to the food the quality which it lacks.

The Use of Drugs.

I am afraid that the rising generation of medical men are in danger of losing much of that finesse which enabled our forefathers with but a limited selection of drugs to adapt them as perfectly as they did to the circumstances of the case. I may say that the art of writing a prescription for a perfect laxative pill—a pill whose action should be at once pleasant and adequate—is almost a lost one. There was generally a reason for each ingredient in their time-honoured formulæ. Take for example the *Pil. Aloes et Myrrhæ* of the British Pharmacopœia. How many young practitioners could tell you off-hand the utility of the gum-resin? And in consequence, because they cannot see the use of it (practical therapeutics having been relegated to a very secondary place in their scheme of education), they rarely prescribe it. And yet, as a matter of fact, it is of extreme value, and gives to the pill its characteristic action—that of a laxative without a tendency to be followed by subsequent constipation. It was found by clinical experience that the addition of a resin, devoid of active purgative properties, prevented to a great extent reaction in the direction of constipation by possibly acting as a tonic to the mucous membrane, and so the *Pil. Aloes et Myrrhæ* came into being. In the treatment of chronic constipation drugs are of extreme use, but to make them reliable tools we must thoroughly understand how they act and what we are going to do with them. The medicinal substances which we shall find advantageous to use may be arranged in the following groups:

1. Those which excite peristalsis. Of these we have nux vomica and Calabar bean.

2. Those which relieve spasm. In this group we find belladonna, the bromides, valerian, and perhaps assafoetida.

3. Drugs which increase the secretions of the gastro-intestinal tract. The most useful of these are chloride of ammonium and phosphate of soda.

4. Medicines have a beneficial effect upon the neurasthenic condition. Valerianate of zinc, nitrate of silver, ergot, arsenic, and the glycerophosphates of soda. The glycerophosphate of lime must be avoided, as it has a distinctly constipating effect.

5. Laxatives. These are never to be used except in special circumstances.

With these few drugs at our command we are in a position to very materially assist the action of the other means which we are using. If there is enterospasm we shall give belladonna or one of the bromides. If the peristalsis is defective, nux vomica will be found useful. When the stools are dry and hard, we shall employ with great advantage drachm doses of phosphate of soda in combination with a bitter, and perhaps taraxacum, or we shall take advantage of the undoubted properties of chloride of ammonium in increasing the intestinal secretions. We owe not a little of the knowledge which we possess as to the action of the latter drug to the valuable researches of Lockhart Gillespie.* In addition to its action upon the intestinal tract it appears to have the remarkable power of removing any dyspeptic symptom which may be present. This is probably due to its action in increasing the amount of hydrochloric acid in the gastric secretion.

But the question will now probably be asked, how are we to secure adequate actions of the bowels during the course of the treatment?

The Regulation of the Bowels during Treatment.

From the first we must make up our minds to abandon the use of purgatives. We must procure the necessary actions—

1. By the use of a small daily enema of cold water. Whilst enemata of hot water distinctly

relax the intestinal tissues, cold water acts as a tonic, and may be used with practical impunity. At first we may give a small injection every day, and as by degrees the intestines take on their normal function it will be required less frequently until we can dispense with it altogether. For slight cases this will be sufficient.

2. The oil enema. This has been already alluded to in the paragraph upon removing the contents of an impacted rectum. But it has even a more important use as a means of procuring a daily action of the bowels, and should be immediately resorted to in cases which resist the simple enema of cold water. For these cases the best way, I think, of administering it is by means of an ordinary glass funnel. A long colon tube is passed up into the bowel about twelve inches, and is connected by a short length of tubing to the funnel in question. The patient is lying on his side in bed with the pelvis elevated a few inches. The physician, standing by his side, holds the funnel with the left hand a foot or so above the patient, while with the right he slowly pours into it the oil, which has been previously warmed. The oil should be introduced into the bowel very slowly, at least fifteen minutes being taken up in doing so. About ten ounces will be sufficient for an adult, and two or three for a child, according to age. The oil should then be retained as long as possible, and if the injection be given at bedtime the action of the bowels will often not take place until the following morning. The effect of one injection will usually last for several days, and often for a week, the patient having a daily stool during that period. When the effect passes off another should be given. In any case where it is desirable to procure the maximum effect these injections may be given every day. It sometimes happens that in certain cases the oil will be found to produce an unexpected purgative effect. In these cases there is probably some increased peristaltic action which hurries unduly the bile and pancreatic juice along the intestine, and these, coming in contact with the oil, form irritating soaps. It is advisable when using oil enemata to now and then administer a large alkaline douche.

The following case will serve as an example of the result of oil injections on average patients:

Mrs. C—, aged 34, consulted me in March, 1898. She had been for many years troubled with

* "The Action of Acids and Alkalies and of some other Drugs on the Secretions of the Body in Health and Disease," *Medical Press and Circular*, Oct. 3rd, 1894.

constipation, and for some time past the bowels had never been opened except as the result of purgative medicine. Latterly nothing less than ten grains of colocynth and hyoscyamus pill would produce an evacuation.

On examination fecal masses were felt in the cæcum and in the descending colon. The rectum was also blocked with feces. Treatment was commenced by a series of large douches, which removed a large quantity of fecal material from the bowel. As soon as the large intestine was perfectly emptied daily treatment was commenced in the form of local application to the abdomen of the slowly interrupted current from a coarsely wound primary coil. At the same time she was instructed to make use of the usual exercises. The bowels not having moved after the sixth application, another large douche was administered. On the eleventh day of treatment an oil injection of half a pint was given at bedtime. The bowels were opened on the following morning, and also on the two following days. Two days were then allowed to elapse without another injection, and then the oil douche was repeated. The bowels were now open every day for a week. For the next three days there was no action of the bowels, and a large douche of half a gallon of water at 100° F. containing a little bicarbonate of soda was given. On the third day after this there was a natural motion, and also on the next day. An oil douche was then given, with the result that a motion every day or so resulted for a fortnight. Three more oil injections were taken during the course of her treatment, which extended over three months, and since then none have been required, the bowels remaining regular. The only drug administered by the mouth was a little nuxvomica.

PROGNOSIS.

As regards the probability of permanent relief, it is my experience that constipation, which depends solely upon atony of the intestines and on weakness of the abdominal muscles, can be almost always cured if we use the right means, AND CAN PERSUADE THE PATIENT TO SUBMIT TO A COURSE OF TREATMENT OF SUFFICIENT LENGTH.

But it is here where the difficulty comes in. The majority of those who do us the honour to consult us expect us to write a prescription which will

magically restore the natural functions of the bowels; or to suggest a course of galvanic or mechanical treatment which they can carry out for themselves or with unskilled assistance. As a matter of fact, in order that the condition which I am now discussing may be cured, it is absolutely necessary that any galvanic or other manipulative treatment must be applied by an expert who has had especial experience not only in the use of the methods which he is employing, but also in their especial application to cases of this nature. It is also a *sine quâ non*, if any satisfactory progress would be made, that the physician should have an opportunity of watching and checking from day to day the effect of the drugs, diet, and other means which he is using. Only in this way and under these conditions can you hope to really alleviate these intractable affections; and if the patient be unwilling to give you a free hand in this direction, and cannot be induced to offer you the necessary facilities, it will be best to decline the case altogether, or your client will be disappointed and your treatment suffer unmerited discredit. Over and over again have I seen patients relapse who, in their own wisdom, had relinquished treatment as soon as it had commenced to prove beneficial. The restoration of all weakened tissue must of necessity be a somewhat lengthy process, and the gastrointestinal tract is no exception to this rule.

In concluding this brief and imperfect sketch of a most interesting and important subject I can only further impress upon you the conclusion to which its study must inevitably lead us—that the cases of constipation which come under our notice in our daily practice are not to be lightly regarded, and dismissed with a prescription for a routine laxative, but should be made the subject of earnest study. We shall thus preserve our patient from possible dangers in the future, and greatly increase his present comfort.

Shellacked Paper for Air-Cushions versus Rubber.—In the manufacture of air-cushions and the like for the sick, very tough paper, heavily shellacked and varnished in the Japanese fashion, is becoming very popular among German physicians. Such articles resist moisture and the deteriorating action of body-secretions much better than does rubber; they stand quite as rough usage, are generally as durable, do not become so hot and uncomfortable, and are less than half as costly. *Philadelphia Med. Journal.*

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A CLINICAL LECTURE

ON

SPLenic LEUCÆMIA.

Delivered at St. Bartholomew's Hospital,

BY

T. LAUDER BRUNTON, M.D., F.R.S.,

Physician to the Hospital.

GENTLEMEN,—The case to which I intend to draw your attention to-day is that of a boy, H. S—, æt. 12½ years, who was admitted to this hospital on May 7th, complaining of pain in the abdomen, in which region there was also swelling. He has been a delicate boy all his life; he has had measles and chicken-pox, and on three occasions—namely, when he was three, nine, and eleven years of age—has suffered from a swelling in the perinæum between the rectum and the scrotum. Otherwise he has had no definite disease. On each occasion the perinæal swelling subsided in about three weeks with quiet and rest in hospital, without operation. About fourteen months before admission he had hæmorrhage from the mouth, so profuse that, according to his mother's statement, he lost about two or three pints of blood in this way. This hæmorrhage ceased on the extraction of two carious teeth. Nothing more was noticed until about Christmas of last year, when he began to complain of pain in the head and stomach, of swelling in the stomach, and of curious attacks of feverishness, very much like ague. During these attacks of pain he had shiverings, with coldness of the skin, and this was succeeded first by heat and afterwards by sweating. On admission his temperature was found to be high, and he complained also of some dimness of vision. On examining the abdomen a tumour was found (see Fig. 1), and I draw a diagram to show you the position and extent of this tumour. You see that the tumour fills up the whole of the lower part of the abdomen; it extends into the right iliac fossa, and above the level of the umbilicus. It is smooth on the surface,

dull on percussion, slightly moveable on pressing in the lumbar region, and it moves a little with respiration. It has a sharp edge, with a distinct

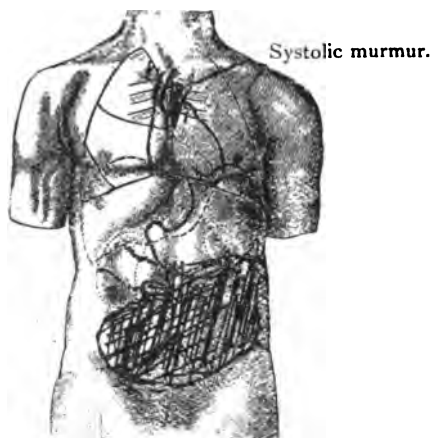


Fig. 1.

double notch, close to the umbilicus. This notch is directed to the right and somewhat upwards.

Diagnosis of splenic tumours.—From these characters you will see at once that there can be no doubt about the diagnosis—namely, that it is an enormously enlarged spleen. The characters seem so distinct that probably most of you may think that it is quite impossible to mistake an enlarged spleen for anything else, or anything else for an enlarged spleen. This, however, is not the case. When you meet with a tumour in the abdomen you sometimes require a good deal of care in order to make certain that you are dealing with a spleen, and not with anything else. In a lecture I gave here some time ago on ulcerative endocarditis, I had to confess to you that I had mistaken a spleen for a malignant tumour. My reasons for supposing the spleen on that occasion to be a malignant tumour were that the surface of it was nodulated, that the texture of it was not hard and resistant, as it is usually in the spleen, but was somewhat softish, that the edge of it was not sharp, and that there was no distinct notch (see Fig. 2). It turned out afterwards, at the post-mortem, that this tumour which I had considered to be malignant was really a large spleen with a number of infarcts in it. This case, therefore, shows that a spleen may be mistaken for something else. On another occasion I met with a case where there was a large tumour very like what I have drawn

here, with a distinct notch and with a fairly distinct edge (see Fig. 3). It had, curiously enough, a double edge, for which I could not very well account. The feeling, however, was so much like that of a spleen that I considered that it probably was the spleen. I was led to do this a good deal by the distinctness of the notch. There was one point about it, however, which I could not exactly make out. In all the other cases of enlarged spleen that I had seen, the notch was directed to the right and somewhat upwards, and in this case the notch

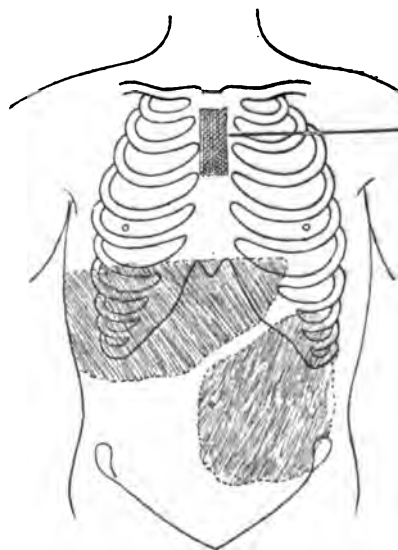


Fig. 2.

was directed downwards. Still, so distinctly did this seem to be a spleen that I neglected the position of the notch, and so did a brother of the patient, who is also a doctor, and who is exceedingly good in the diagnosis of abdominal diseases. At the post-mortem examination, however, this tumour turned out to be a large mass of new growth. Ever since then, therefore, I have paid particular attention to the direction of the notch. Now, having confessed to you those two mistakes, you may possibly think that making mistakes in diagnoses of this sort is an individual characteristic of mine, but I must tell you that this is not altogether the case, because other men have also made mistakes, and a very curious example of this occurred in a case I saw some months ago. It was a man who had been in Japan for several years and had returned with a tumour in his abdomen.

I will draw a diagram of the appearance (see Fig. 4). The whole of the left side was completely dull. I diagnosed the case as one of greatly enlarged spleen and enlarged liver. The friends were not satisfied, and took the patient separately to two consulting surgeons, both of whom have got a great reputation for their treatment of abdominal diseases. They both agreed that the tumour was not a spleen, but they were unable to determine

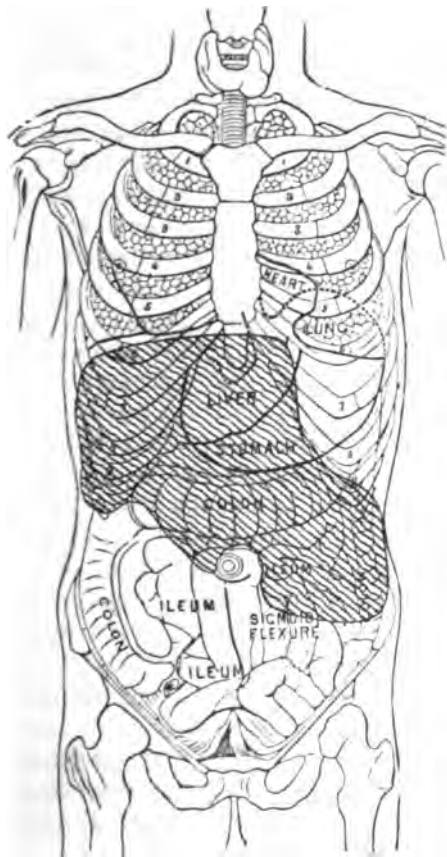


Fig. 3.

the nature of it. One of them, however, thought that some information might be gained by inserting a trocar, and so a long trocar was run into the left side of the patient and into the tumour. Nothing was withdrawn, however, but a few drops of blood. About a fortnight afterwards I was asked by the doctor with whom I had seen the patient to be present at the post-mortem examination, and we found an enormously enlarged spleen and an abdominal cavity filled with fluid blood.

We could trace the track of the exploring trocar through the spleen. We could not find any lesion in the splenic vein, but from the direction of the track it seemed that the needle had gone right through the spleen and punctured the splenic vein, making a minute aperture. The blood had not clotted, but had simply gone on oozing through this solitary opening until the blood was entirely drained into the peritoneal cavity. This case

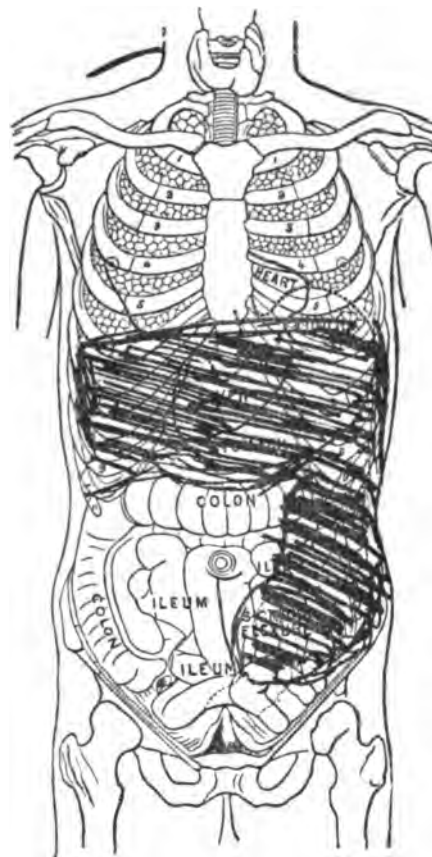


Fig. 4.

shows not only the difficulty of arriving at a correct diagnosis in cases of enlarged spleen, but also the advisability of opening the abdomen and seeing what the condition of affairs really is, rather than of running a needle blindfold into the tumour. I may mention also that large spleens have been mistaken for ovarian cysts, but this, of course, could not happen in the cases I have narrated, because they were males.

Nature of tumour.—In the patient we are speci-

ally discussing not only have we got all the points which indicate the tumour to be an enlarged spleen, but we have certain indications as to the nature of the enlargement. In the case of ulcerative endocarditis which I have mentioned in which an enlarged spleen occurred, the size of the spleen was due to infarcts, and to inflammation of the spleen consequent upon the infarcts. Occasionally malignant disease has been found in the spleen, but it is by no means common, and one could hardly come to a correct diagnosis without the presence of malignant disease in some other part of the body to substantiate the diagnosis. From the size of the spleen, and also of the liver, in this boy, you might think there was some obstruction to the venous circulation which affects both the liver and spleen. If this were of the nature of a thrombosis, however, we should certainly find the vessels of the mesentery affected also, and we should then find a large effusion of fluid into the abdominal cavity; but there is no fluid present in this case.

On examination of the blood, however, we find that the red corpuscles are diminished in number, the white are very considerably increased. The proportion of hæmoglobin is also diminished to below the normal. From these two conditions—the presence of considerable leucocytosis in the blood, and the coincident enlargement of the spleen—we may fairly come to the diagnosis that we have got to do with a case of leucæmia.

Concurrence of cases.—Now when we find several hot days coming together at this time of the year we are not astonished, and if we find several consecutive cold days in winter we are not astonished, because these things happen in the regular course of things. If you go to the wards to-day you will probably find very few cases of typhoid fever. If you go into the wards in autumn you will probably find a large number of cases of typhoid fever. Typhoid fever we associate with the presence of certain microbes, and these microbes require certain atmospheric conditions for their development and rapid propagation; therefore, at certain seasons of the year we find typhoid fever prevalent, and we associate it with atmospheric conditions of a certain sort, and are not surprised to find several cases of typhoid fever coming together. But there are cases of other diseases which come together, and for their doing so we can find no reason. I

remember as a student being very much struck by a prophecy which was uttered by the late Professor Syme. One day a case was brought in, and he said, "I have not seen a case of this sort for twenty years; we will probably have another within a fortnight." Another case did come within a fortnight. The disease which we are considering to-day is not very common, but in another ward of this hospital I believe there is at present another case of the same sort. A similar coincidence of cases occurred when the disease was first described, and in consequence of this a strife arose in regard to the priority of naming and describing the disease. A case was described by Professor Hughes Bennett in October, 1845, and another case was described in November of the same year by Professor Virchow. The name leucæmia was given by Professor Virchow. The name leucocythæmia was not given by Professor Bennett until some time afterwards. The case which he described before Virchow was supposed by him to be one of suppuration of the blood, on account of the great number of corpuscles present in it which were identical in character with pus corpuscles.

Characters of the blood.—Now the corpuscles that we find in leucæmia vary in their characters, and that is the case with this boy. There is one kind with a large nucleus and only a small amount of protoplasm; another very large one with a single nucleus; others with a horseshoe-shaped nucleus, and again others with several nuclei (polynuclear). One specimen of the blood showing all four kinds of corpuscles will be put under the microscope for your inspection.

Functions of the spleen.—Now the functions of the spleen have given rise to a considerable amount of discussion, and even now we do not know, perhaps, as much about its functions as we could desire. There is a fairly general consensus of opinion that the spleen during health has a two-fold and perhaps a three-fold function, namely, that (1) of making white corpuscles, (2) of making red corpuscles, and (3) very possibly of destroying either white or red corpuscles or both, and using the constituents of the corpuscles thus destroyed in building up new ones. Besides this, however, the spleen has got the important function of destroying microbes in disease. It seems to have the power of forming substances which are destructive to microbes; and an extract of the spleen

contains certain globulins which have a destructive action upon microbes. At the same time, however, these globulins have a toxic action upon a man or an animal, and the blood of the spleen appears to contain some kind of an enzyme which has the power of dissolving albuminous substances, so that the clot from the splenic vein tends to break up again and not remain hard like the clot from another vein. In all probability it was this want of coagulability in the blood of the splenic vein which led to the great accumulation of blood in the abdominal cavity of the patient to whom I have just referred.

Symptoms.—This enzyme when injected into the blood seems to have the power also of raising the temperature, and you will notice that in the case of the boy before us there has been a rather striking rise of temperature. When he first came in his temperature went up daily, and in such a way as to remind one strongly of the temperature in ulcerative endocarditis. The position of the apex beat was somewhat outside the nipple, and there was a loud murmur at the base of the heart, extending both to the left like an ordinary anæmic murmur, and upwards along the arteries, so that one came to the conclusion that although part of the murmur was certainly anæmic, there might be in addition an organic murmur due to the roughness of the aorta. This point I have not quite decided even yet, though as the murmur over the aorta is getting less, there seems some probability that the murmur may be entirely due to anæmia. Now you will notice that although the boy did not complain of pain or abdominal swelling until last Christmas, he presented a very suspicious symptom fourteen months ago, for he lost about two pints of blood from the mouth, and, as I should have mentioned before, the mouth was inflamed, and a number of white patches were observed in it. This bleeding appears to show, then, that the blood at that time had begun to undergo change, and that the boy was already suffering from leucæmia, and from the irritation in the mouth which is a common concomitant of leucæmia, and is known as *stomatitis leukæmica*.

The dimness of vision of which he complains has been attributed to a *retinitis leukæmica*, in which condition the veins are generally found to be enlarged and tortuous, and the disc more or less œdematous. In this boy the disc was found

to be only very slightly œdematous. The veins were very enlarged, and were tortuous.

Not infrequently it is found that the urine shows some curious characters, and especially that the uric acid is greatly increased. This was so much the case in another patient (see Fig. 5), who curiously enough had lived in Japan like the one whom I have already mentioned, that I thought he was suffering from a collection of calculi in the kidney.

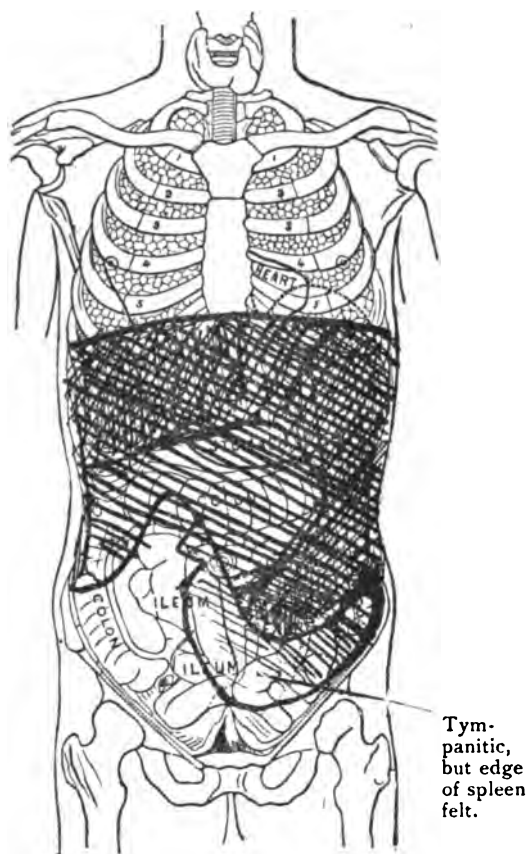


Fig. 5.

He passed several calculi, and had all the symptoms of renal colic. I thought probably he had got a collection of calculi in the pelvis of the kidney, but on examining the body post-mortem none were found in the kidney; but he had evidently been forming uric acid calculi and passing each one as it was formed. Now, the occurrence of uric acid in excess is looked upon by some authorities as an important point in the diagnosis of leucæmia. The Germans have distinguished between leucæmia and pseudo-leucæmia, but the only points of real

distinction between them, even if there be such a distinction existent, are that in true leucæmia you find increase of the white blood-corpuscles and increase of uric acid accompanying the enlargement of the spleen, whereas in pseudo-leucæmia you do not find increase of white corpuscles nor increase of uric acid.

Prognosis.—The prognosis in regard to this condition is usually very poor. The duration is usually only about one to three years, and although one finds temporary improvement, as a rule the cases fall back again, and death is the ultimate result.

Treatment.—The treatment in such cases is very unsatisfactory. Practically one may attempt to treat them in the same way as you would a case of malaria, in which disease also, as you are aware, the spleen is also very much enlarged. So much enlarged and so soft is it in some of the cases which occur among the natives of Hindostan, that, as a rule, a white man should be very careful about striking a native, especially over the region of the spleen, lest he should burst it, and the native should die in consequence. Such a case did occur some time ago, when a white man was tried for the murder of a native by striking him over the region of the spleen. In one case, at least, large doses of quinine are said to have cured a patient suffering from leucæmia. Whether this is so or not, of course it is hard to say, because those who believe that leucæmia is very rarely if ever cured would say that it was not a case of true leucæmia, but only a case of malaria with enlarged spleen. Still, quinine in large doses is one of the remedies which is generally used. Other cases improve on iron and cod-liver oil, but the remedy that is most in favour is arsenic, and this should be given in doses increasing as far as the patient will comfortably stand it, beginning say with two or three minims of either liquor arsenicalis or liquor arsenici hydrochloricus, or liquor sodii arsenatis three times a day after a meal. You will remember that when giving arsenic in large doses, it must be taken after a meal; if given before a meal it is liable to cause gastric derangement and irritation, which would lead to its having to be discontinued. Other means which have been adopted for relieving this complaint are local applications to the spleen, intended to cause contraction of the organ. These are—cold suddenly applied, faradisation, and

gentle massage. Ergot is said to have the property of causing contraction of the spleen, and has therefore been employed.

We have been giving our patient a mixture of the remedies I have alluded to,—hydrochlorate of quinine $1\frac{1}{2}$ grains, liquor arsenici hydrochloricus 3 minims, nitro-hydrochloric acid dil. $\mathfrak{m}\text{ij}$, aq. ad $\mathfrak{z}\text{j}$. Besides these we have been giving the boy some salicylate of soda because the temperature was so high. After remaining in bed about three weeks and taking this medicine, the temperature went down. I do not think this is to be attributed to the salicylate, but rather to the rest in bed apart from the medicine. In these cases rest should always be insisted upon. The further treatment will consist in pushing the arsenic a good deal further than we have done hitherto, and combining it with iron and cod-liver oil, and then I think it possible that by gentle massage over the spleen, combined with the use of a faradic current, we may get some improvement, at least of a temporary nature, although the ultimate result of treatment is, unhappily, not likely to be very satisfactory.

Use of Saline Infusions in Uræmia.—Whatever our belief may be as to the exact causative agent in uræmic conditions, it has long been admitted that some toxic agent does accumulate and circulate in the blood. Bouchard found that while it required ten cubic centimetres of normal human blood to kill a guinea-pig, from three to six cubic centimetres of blood from a uræmic patient would accomplish the same result.

Now as this agent is contained largely in the blood serum, the most logical sequence would be to get rid of as much of the blood containing the poisonous agent as possible without directly endangering life, and then to dilute the remainder with some harmless agent. Such an agent we have in the decinormal salt solution. Furthermore, the saline solution has a powerful osmotic action on the kidney cells themselves, and thus increases the eliminative processes (the researches of Bosc would indicate that its action is mostly manifest upon the cells of the convoluted tubules). In addition, clinical experience has proved that as a result of its introduction into the circulation diaphoresis and purgation occur, so that it fulfils all indications for treatment in a case of uræmia.

DR. REILLY, *Medical Record*, Nov. 12th, 1898.

A CASE OF INFLUENZA ILLUSTRATING THE IMPORTANCE OF A BACTERIOLOGICAL EXAMINATION.

BY

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A TYPICAL case of any of the varieties of influenza is easy to diagnose, but there are many cases in which the diagnosis is extremely difficult. The catarrhal variety may be mistaken for a simple "cold," or the latter may be mistaken for influenza. Considerable constitutional disturbance, with muscular pains and prostration is usually taken to be the criterion for distinguishing influenza from a "cold." The case which I am about to record shows that influenza with marked catarrhal symptoms may exist without much in the way of constitutional disturbance.

The case was that of a medical man 35 years old. Ever since he can remember he has been liable to one or two attacks of catarrh in the year. These attacks begin in the nasal mucous membrane, and spread in two or three days to the pharynx, trachea, and bronchi. The inflammation subsides in a few days, and by the end of a week or ten days from the commencement the patient is well. In addition to these attacks of catarrh, he has suffered from a large number of attacks of influenza. None of these attacks have been sufficiently severe to necessitate keeping in bed, but there has been no doubt about their nature. In some of the attacks muscular pain has been a prominent feature; in others, catarrhal symptoms; in others prostration and depression. In one attack, which began suddenly in the night, a large basinful of mucus was expectorated in a few hours, but the cough had completely ceased by the morning. Many of the attacks could be traced to infection from other cases.

The history of the present attack is as follows:—On the evening of October 30th he felt a little tired, but this he attributed to having taken an unusual amount of exercise.

On October 31st he felt rather languid all day. He was, however, able to work until half-past eleven at night, with an interval of half an hour for lunch and an hour and a half for dinner.

On November 1st he suffered from a severe nasal catarrh, with profuse discharge of thin mucus. He was at work all the morning at home, went out immediately after lunch to attend to his duties, returning to dinner at seven o'clock. Immediately after dinner he went out again, and did not return until nearly midnight.

On November 2nd the nasal catarrh was worse, and the discharge purulent. He had a heavy morning and afternoon's work. In the evening he felt rather tired, and went to bed at nine o'clock. During the night he suffered from cough, and felt a little feverish.

On November 3rd the nasal catarrh was better, but the cough was worse, the expectoration consisting at first of glairy mucus, and later of muco-pus of a yellowish-green colour. He worked all day, with only a short interval for lunch, and went out to dinner in the suburbs, returning home at half-past twelve at night.

On November 4th both the nasal discharge and the expectoration were less, and by November 9th they had practically ceased.

All through the attack the appetite was unimpaired. The temperature was only taken on November 4th, and was then normal.

On November 7th inflammation, starting in a hair-follicle, occurred over the front of the knee, which required to be treated by incision.

From the above account it will be seen that the somewhat severe catarrhal symptoms were accompanied by very slight constitutional disturbance. The patient was able to do his work as usual, and appeared to be suffering from an ordinary catarrh. The occurrence of the inflammation of the leg during convalescence was suggestive of influenza, and this was borne out by the result of a bacteriological examination.

On November 3rd a little sputum was coughed into a sterile capsule. It was of a yellowish-green colour, and on microscopical examination it was found to consist chiefly of pus cells. Cover-glass films were prepared, and were stained in weak carbol-fuchsin for several hours, and in other ways. A few minute bodies, looking like diplococci, were seen in some of the pus cells, but no other bacteria

were found. It was impossible to say from their appearance whether these bodies were influenza bacilli.

The surface of the sputum was washed in sterile broth, and a blood-agar tube was then sown with a little from the interior of the mass. After twenty-four hours' incubation at 37° C. the surface of the blood-agar was covered with discrete semi-transparent colonies of the size of a pin's head. These colonies, when examined microscopically, were found to consist of influenza bacilli. By the next day the tube was overgrown with other bacteria.

On November 4th a fresh sample of sputum was examined. This was of a greenish-yellow colour, and on microscopical examination was found to consist of fibrin and pus cells. Cover-slip films showed the presence of diplococci resembling the pneumococcus in morphology. A blood-agar cultivation was made, and, when examined next day, many colonies of pneumococci and a few colonies of influenza bacilli were found. A rabbit, which was inoculated into the peritoneal cavity with some of the cultivation, died of typical pneumococcic septicæmia, the blood being crowded with capsulated diplococci.

There are several points of interest in this case. In the first place, the clinical aspect was that of a simple catarrh, while the presence of influenza bacilli in the bronchial secretion on the occasion of the first examination proved that it was a case of influenza.

Secondly, the presence of pneumococci in the bronchial secretion on the occasion of the second examination is interesting in connection with the not infrequent occurrence of pneumonia as an early sequela of influenza. Such a pneumonia is usually a true croupous pneumonia caused by the pneumococcus. Now it is well known that pneumococci are frequently present in the mouths of healthy individuals. As long as the individual remains in health these micro-organisms can do no harm, but when the vitality is lowered they can invade the tissues. In the case we have recounted the vitality was sufficiently lowered to allow the pneumococci to invade the trachea and bronchi, but it was not sufficiently lowered to allow them to invade the lungs. Consequently our patient escaped an attack of pneumonia. It is interesting to note that his mouth was examined on February 12th, 1898, and was then free from pneumococci, as was proved

by the inoculation of rabbits. He had, however, been working with cultivations of the pneumococcus, besides attending patients suffering from pneumonia, so that there was plenty of opportunity for infection to take place.

Lastly, this case is of interest as showing the importance of a bacteriological examination in the diagnosis of doubtful cases of influenza.

The Motor Symptoms of Chorea.—Mitchell and Rhein ('Philadelphia Medical Journal,' Jan. 22nd, 1898) say that in this study they attempt to elaborate more fully the descriptions of the movements of chorea. From the text-books one is led to believe that the movements follow a definite type in all cases, with only occasional variations. It is claimed that they are continuous when the disease is advanced, rendering voluntary muscular acts impossible or capable of completion with the greatest difficulty; that the movements are increased by voluntary acts, though sometimes for a time quieted during such effort.

First, eighteen cases are reported in abstract, in all of which there were continuous inco-ordinate movements when the patient was at rest, but in which the execution of finer movements (writing, bringing two points together, drinking from a glass) was impaired, and at once caused a cessation of the involuntary movements during the intended movement. The second type, illustrated by seven cases, embraces the patients whose choreal movements persist during rest but are markedly aggravated by voluntary motion. In such cases writing is quite impossible. In another type the inco-ordinate movements are entirely or practically absent when the patient is at rest, but immediately appear on the slightest attempt at voluntary movement, such as drinking a glass of water or bringing two points together. In yet another class of cases volition has no effect whatever upon the abnormal movements. One of the cases used to illustrate this type had rhythmic movements—a great rarity in chorea. It is possible for the same case, at different periods of its course, to show the characteristics of more than one of the above types.—*Medicine*, November, 1898.

SOME PRACTICAL POINTS IN THE TREATMENT OF HEART DISEASE.*

BY

A. E. SANSOM, M.D., F.R.C.P.

A HYPOTHETICAL case was assumed of a patient under the age of thirty-five suffering from organic disease of the heart, but able to walk about, the disease therefore being, or seeming to be, chronic. In such a case the probability is very great that the morbid affection is of rheumatic origin. After the age of thirty-five the factors of heart disease are multiform, and rheumatism plays but a small part in the causation. Before any question of treatment be considered it is essential that a clear picture be formed of the condition of the diseased structure and of the organism in its entirety. No treatment can be considered practical without such preliminary knowledge. Rheumatic disease of the heart may occur in the very earliest periods of life; it has been detected in the foetus *in utero* by auscultation of the foetal heart. Instances were quoted to show that foetal endocarditis is by no means a rare affection. It may occur when there are no signs of rheumatism in the mother. The morbid changes are found to be identical with those of the rheumatic form of endocarditis. In the period of early childhood the clinical associations with rheumatism are more manifest, though the articular inflammation may be very slight, and in a very considerable number of cases no signs or symptoms of implication of the joints exist, but in childhood the whole heart is profoundly affected by the rheumatic process. When we come to consider the conditions of heart disease in adolescence, and in adult life in its earlier and later periods, the evidence of morbid anatomy goes to show that the changes are more widely spread than would seem at first sight.

The structures in the neighbourhood of an inflamed valve in rheumatic endocarditis, the fibrous tissues in the interstices of the muscular fibrillæ, are found on microscopical examination to be in-

filtrated with inflammatory exudation cells. The careful observations of Drs. D. B. Lee and F. J. Poynton have recently shown that an acute dilatation of the heart is a common occurrence in a rheumatic attack, even when arthritis and pyrexia are slight, and when there is neither distinct pericarditis nor endocarditis. An acute dilatation of the heart may be superadded to pericarditis and endocarditis. In the rheumatic affection of children the enlargement of the heart has been demonstrated in a large number of cases, and this enlargement receded as improvement set in. After convalescence from rheumatism, when the heart affection seems to be chronic, there are, nevertheless, changes going on in the diseased structures; these are slow and insidious, and are not betrayed by symptoms. Furthermore, in the apparently chronic changes there may be dilatations from time to time. The first practical point in regard to treatment is that we should realise that the problem is not the simple one of restoring a lack of power in the heart muscle, but a complex one of overcoming the influence of various factors and varying phases of disease. Take first the medicinal treatment in regard to the administration of digitalis; there are *pros* and *cons*, the drug often works admirably in truly chronic cases when difficulty of breathing and signs of commencing dropsy appear; it is inert or even harmful when there are recrudescences of the febrile signs of rheumatism; when there are no symptoms to indicate this, the heart muscle and tissue are infiltrated with inflammatory products, not only during periods of rheumatic pyrexia, but also when the heart structures are actively infiltrated during rheumatism, though it may be in the apparently chronic stages. Digitalis is often inert or harmful; if two or perhaps three days have passed without improvement, the drug should be omitted, and the patient advised to take absolute rest in bed. Of course the withholding of digitalis should be earlier if signs of intolerance arise. An ice-bag is probably the best topical application, but the ice-bag suspended from a cradle should rest lightly over the heart region, a thin layer of flannel intervening. The ice-bag may be applied for periods of half an hour, and removed for like periods; usually the patient is made comfortable by this means, and the heart tumult allayed. The second practical point is that

* An abstract of the Purvis Oration delivered to the West Kent Medico-Chirurgical Society, Dec. 2nd, 1898.

any sign of rheumatic storm in the heart should be treated by placing the patient at rest in bed. A warning should be given against the indiscriminate use by the non-medical public of methods of muscular exercise. In many cases of rheumatic heart disease in their truly chronic stages, those methods of increasing the physiological activity of the heart muscle are of very high value, but there is much danger in the case of a rheumatic heart, even though the conditions may seem to be chronic. If there be the rheumatic swollen heart, a condition of which the patient himself may be unconscious, the use of muscular exercises cannot be otherwise than harmful; to permit them would be equivalent to a direction to a patient with inflamed joints to run. After a period of such absolute rest, exercise should be commenced gradually, for too long inactivity is of course baneful; but the question of the duration of the period of complete repose should be left to the medical adviser. A small hypodermic injection of morphia acetate or morphia hydrochlorate, one tenth to one fourth grain, is a great aid to the calming and comforting of the patient. Dr. Clifford Allbutt's opinion was confirmed that though by the mouth opium is behind other sedatives in value, its use being attended by grave drawbacks, hypodermically in doses beginning at one tenth of a grain, and gradually ascending to a quarter of a grain if necessary, it is a precious means of relief. Dr. Toogood's views, as recently expressed, emphasised the value of morphia judiciously used in heart affections. The administration of alkalies is useful, but the salicylates, though they produce comfort during the period of joint inflammation, have probably no influence upon the rise and progress of heart disease of rheumatic origin. There is much to be said for the administration of calomel in small half-grain doses three times a day for periods of three or four days together. Instead of blisters, belladonna, and other plasters, or the local application of iodine, the inunction of ointment of iodide of ammonium, one drachm to the ounce of benzoated lard, may be systematically employed with advantage. In cases presenting crises of grave significance the employment of the continuous galvanic current as an adjunct to treatment may be of great value. A case in point was narrated. The third practical point suggested was that special treatment of the nervous system is of very high

importance in the treatment of many of the rheumatic diseases of the heart. In many cases of rheumatic heart disease nervous disturbances of the heart become epi-phenomena. It can scarcely be doubted that the morbid irritation conveyed to the vagus centre, whereby control power and the other endowments of the nerve are impaired, is in many cases a very important contributory factor to produce the *ensemble* of symptoms. It seems, therefore, that an attempt to influence the vagus by the continuous galvanic current is a very reasonable procedure. When muscular movements are permitted, and attentive training has taken place, changes of scene and climate and the use of baths and exercises are potent agencies for good.

THE VACCINATION QUESTION.*

"SOME months since the leaders of the Government dismayed their supporters and astonished the world by a sudden surrender to the clamour of the anti-vaccinationists. In the space of a single evening, with a marvellous versatility, they threw to the agitators the ascertained results of generations of the medical faculty, the report of a Royal Commission, what are understood to be their own convictions, and the President of the Local Government Board. After one ineffectual fight the House of Lords answered to the whip, and, under the guise of a "graceful concession," the health of the country was given without appeal into the hand of the 'Conscientious Objector.'"

Such are the remarkable words used by Rider Haggard in his "Author's Note" to the latest and cleverest novel yet produced by this gifted writer. It is impossible to say more of this novel than that the more widely it is read the more admirers will the author gain, and the more will the country benefit. In his perplexity it has occurred to an observer of these events—as a person who in other lands has seen and learned something of the ravages of smallpox among the unvaccinated—to try to forecast their natural and, in the view of many, their almost certain end. Some who are very competent to judge say that this strange paralysis of "the most powerful ministry of the generation" must result hereafter in much terror, and in the sacrifice of innocent lives.

* 'Dorset Thorne,' by H. Rider Haggard. (London: Longmans, Green & Co. 1898.)

MEETING OF THE SOCIETY OF ANÆSTHETISTS,

At 20 Hanover Square, November 17th, 1898;

The President, Dr. DUDLEY BUXTON,
in the Chair.

* * *It was announced that at a Special Meeting of the Society, held before the Ordinary Meeting, it had been decided to hold all future Meetings on the third Friday in the month instead of the third Thursday.*

THE President said that as this was the first meeting for business in the present Session, it became his pleasant duty to welcome the members of the Society, and to hope that the meetings in the present year would be as successful and instructive as those which had occurred in the past. He thought they must all feel that the work of the Society had, in spite of the fact that the Society was a young one, been of such a kind as to show that the meetings of the Society were in themselves valuable, and had been of distinct benefit to each and all of its members. The future success depended upon the members themselves, if they were willing to work in the same way as they had done in the past, to throw the same amount of zeal and energy into the work they would realise what they hoped for—a continual period of utility and prosperity for the Society.

It would be a waste of time for him to add anything further that evening, as they had before them, he hoped and had no doubt, a most interesting evening. But he would add that the pleasure and the success of their meetings must depend upon themselves very largely. They could not always expect to have help from those who were good enough to come from outside. They must have papers or discussions, and especially cases of difficulty and danger brought before them. No case of difficulty was so trivial that it was not of value to them, and those of them who were busy with practice all the week knew only too well that such cases were perpetually cropping up. Moreover, if they discussed their cases of difficulty openly before their fellow-workers, they would have the opportunity of knowing what others thought of

them, and perhaps learn that the methods which they had thought to be the best were not considered by others to be quite immaculate. The Secretaries were always ready to receive papers, and they would seize at the opportunity of any new matter being brought forward.

He need not detain them by introducing Professor Ramsay. His name was too well known in the world of science to require any encomiums from him.

Professor WILLIAM RAMSAY, F.R.S., read a Paper on—

"Pure Anæsthetics."

GENTLEMEN,—My apology for intruding into the ranks of specialists must be that anæsthesia has a chemical aspect, and having been brought into contact a considerable time ago, as the chemical member of the British Medical Association Committee for investigating Anæsthetics, with the subject, I was led to direct some attention to it, and that attention has culminated in the remarks I am about to lay before you to-night.

Whenever an operation is conducted in a gaslit room, anyone who has administered chloroform has been conscious of an acrid, disagreeable, pungent odour, which fills the eyes and irritates the nose. That odour is due to carbonyl chloride. I show you a specimen of it. It is gas at the ordinary temperature, but under a little pressure it is condensed to a colourless liquid. Its formula is COCl_2 , and it can be produced by the direct addition of chlorine to carbonic oxide. It decomposes with the moisture of the air, or with the moisture of the lungs, into carbonic acid and hydrochloric acid, and the hydrochloric acid is an irritative substance which produces violent coughing, and makes it impossible to respire. The equation showing the change is $\text{CHCl}_3 + \text{O} = \text{COCl}_2 + \text{HCl}$. Besides this change, the chloroform burns, and the oxygen of the air unites with the carbon to form carbonic acid, while the chlorine is liberated. It is also known that if chloroform is kept in a partially filled bottle, exposed to sunlight, or even to diffused daylight, a change takes place. It may be taken for granted that the anæsthetic as it leaves the makers is a pure substance. But a change identical with that above occurs, and the anæsthetic becomes somewhat difficult to respire.

The makers are in the habit of adding alcohol, which is understood to prevent such a change taking place. This is a correct assumption; for carbonyl chloride cannot exist in presence of alcohol, inasmuch as it reacts with the alcohol, forming ethyl chloride, C_2H_5Cl , and ethyl carbonate, $(C_2H_5)_2CO_3$, together with water. A change in all probability does take place, even in presence of alcohol, but it is a much slower one, and one which does not proceed far, so that only minimal quantities of these compounds are produced. Indeed, it has been stated, and in all probability with truth, that samples of chloroform, portions of which were used as long ago as the Crimean war, are still "sweet," and suitable for inducing anæsthesia. Still, there can be no doubt that some change occurs, for the anæsthetic, even the usual commercial article, which contains alcohol, is less "sweet," and somewhat "harsh" to respire. It is difficult to apply terms which quite describe the feeling of respiring anæsthetics, but "sweet" and "harsh" sufficiently characterise the qualities which I wish to indicate. I have here samples of chloroform, which I beg to submit to the Society, showing—

1. Chloroform which was pure in July, 1898, and which now, as I show you, fumes in air.
2. Chloroform containing a trace of alcohol which was pure in July, 1898, and which is now "harsh."
3. Pure chloroform.

I might say that perfectly pure chloroform is sweet; it goes into the lungs easily, and there is no feeling of coughing or oppression of any kind; whereas chloroform containing a small quantity of carbonyl chloride has a harsh feeling, and irritates the bronchial tubes, and altogether produces a different sensation. The breathing of the substance is by far the most delicate test of its quality that I know of. As regards poisonous and other impurities, I think they are excluded. I have investigated samples of it from many different makers; in some there is a very small quantity of carbon tetrachloride and some chlorinated products, but these are present in such small quantities that inhalation is not rendered by any means disagreeable. This change which I have spoken of does not take place unless the chloroform has been exposed to light. Here again I show you some perfectly pure chloroform procured to-day from chloral,

and you will notice that it has a very sweet smell, and produces no harsh feeling in the pharynx when it is inhaled. Carbonyl chloride is at once destroyed by contact with alkali. The products are harmless and non-volatile, namely, a carbonate, a chloride, and water. The addition of slaked lime, an alkaline substance, to the bottle of chloroform therefore keeps it "sweet." The reaction which occurs between the lime and the carbonyl chloride is—



The lime settles out before long, and the chloroform remains faintly turbid, but absolutely free from any of the carbonyl chloride. Though the chloroform is not now quite clear, there is still no reason why it should not be administered. The lime, as I have said, is non-volatile, it is non-corrosive, and has no action of any kind. But as this transformation into carbonyl chloride is not very rapid, the bottle might be shaken in the morning, and it may stand for three or four hours without any change taking place.

My friend Dr. Newman, of Glasgow, has been making use of such chloroform for the last three years, and he has kindly sent me the statistics of the cases in which he has employed such chloroform. His experience has been that the administration of such purified chloroform is attended by greatly diminished sickness and retching. The sickness which so frequently follows the giving of ordinary chloroform, and which is so disturbing to the operator, is without doubt a result of stimulation of nerve centres, and it would appear that such a stimulus is at all events largely to be attributed to the impurities. Dr. Newman says in his letter, "I think my cases show very clearly the advantages of using pure chloroform. The results are taken from hospital and private cases—over 700 in number—and are given in percentages, so as to make the comparison easy. The table, of course, does not show the amount of sickness after administration, but I may say that I have kept a note showing the duration of sickness when ordinary shop chloroform was used, and when that treated with lime was employed on different occasions in the same individual; and that while the former caused much sickness for hours, the latter caused very little or perhaps no sickness. The chloroform was always given by the Scotch, the best method."

TABLE showing the comparative results of chloroform administration in columns. Those marked "O" give in percentage the cases in which chloroform (sp. gr. 1.497) prepared from pure alcohol by Duncan, Flockhart, and Co., was used, as supplied by that firm; and those marked "N" give the cases in which similar chloroform was administered after being treated with slaked lime :

Time under Chloroform.	Sickness.				Trouble.			
	During.		After.		During.		After.	
	O. %	N. %	O. %	N. %	O. %	N. %	O. %	N. %
Under 15 min.	21	8	43	19	11	0	4	0
" 30 "	27.5	7	51	18	13	7	5	0
" 45 "	29	8	62	21	12	8	7	3
" 60 "	31.5	10	78	18	14	7	5	2
" 75 "	30	10	78	18	13	6	9	3
" 90 "	36	12.5	81	22	15	5	11	4
Over 90 "	36	11	77	26	14	8	9	2
Average ...	30	9.5	67	20.3	13	6	7	2

The average of the whole comes in this way to 30 per cent. of sickness during the anæsthesia with the ordinary chloroform, 9 per cent. with the purified. After the operation 67 per cent. of sickness with the ordinary chloroform, 20 per cent. with the purified. Trouble during an operation:—With the ordinary chloroform, 13 per cent.; with the purified, 6 per cent. Trouble after the operation:—With the ordinary chloroform, 7 per cent.; with the pure, 2 per cent. These figures strike me as being very convincing. For my own part, I should never contend that all trouble will be avoided by the use of pure chloroform; the only point I wish to make is that here is a means of lessening the difficulties which do occur, but not necessarily of avoiding them altogether. Again, it would be very far from my wish to be understood to mean that death is always to be attributed to the administration of impure chloroform, but it may sometimes be caused by administering impure chloroform; and if that difficulty can be avoided, so much the better.

I have frequently noticed that ether, too, which is frequently administered as an anæsthetic, tends to acquire a "sharp" smell, and when breathed to produce uncomfortable effects. On evaporation there is left a sticky residue, which residue shows the reaction of hydrogen peroxide strongly. This

leads one to suppose that the change is due to the formation of ethyl peroxide, a body which has been very little investigated, the formula of which is $(C_2H_5)_2O_2$, whereas pure ether is $(C_2H_5)_2O$. The reason I suggest this is that it gives the same tests as peroxide of hydrogen. Peroxide of hydrogen gives a blue colour when mixed with weak solution of bichromate of potash and sulphuric acid. Here I have such a mixture. That characteristic is shown by old ether, but not by freshly distilled ether. To purify ether, distillation does not suffice. I have found that the best way is to add a few drops of mercury, and to shake well. The mercury then becomes coated with a black powder, probably oxide, and the sharp feeling of the ether disappears. Pure ether does not tarnish mercury. Ether thus treated can be used without producing the disagreeable effects of impure ether. I have had occasion to require perfectly pure ether for determining its physical properties, and no other plan of purification was found to answer. I think it might perhaps be equally good to use binocide of manganese instead of mercury, but I have never tried it. The only objection would be perhaps that the manganese would be in the form of a very fine powder, which would dirty the inhaler. Neither mercury nor manganese peroxide is volatile, hence no fear need be caused of producing any harm by using ether for anæsthetic purposes from which the finely divided mercury or dioxide has not settled.

One other matter I wish to refer to is the use of a mixture of ether and chloroform. The A.C.E. mixture, as is well known, is sometimes employed as an anæsthetic. I thought it would be worth while to see what substances were really being employed under the name of such an anæsthetic. The problem of investigating gas evolved from all three would be a very difficult one from an analytical point of view, for to estimate the amount of alcohol in ether is a very troublesome proceeding. Hence it would have formed an almost insoluble chemical problem. Therefore we confined our attention to two of them—namely, ether and chloroform, because they are the important constituents of this mixture. To estimate the amount of chloroform in such a mixture was easy; it was put into a tube fitted up like a wash-bottle, and kept cold at the ordinary temperature of the atmosphere, but not allowed to get cold by evaporation. Air was slowly blown through it, and various amounts

were left behind. The mixture was analysed after certain quantities had been removed from it by evaporation. Mr. Samuel Smiles was good enough to carry out those experiments, and I should like to hand in to the Society the actual results which Mr. Smiles has obtained.

A mixture of chloroform and ether, in proportion to their molecular weights, was evaporated at ordinary temperatures by drawing a continuous stream of air through it by means of a pump. The temperature varied from 12° to 15° during the whole process. The chloroform was estimated in the mixture at certain stages in the operation. The method used was as follows:—1 c.c. of liquid was heated in a sealed tube at 120° for four hours, with an amount of alcoholic potash whose Cl content was known. Then the resulting solution was made up to 200 c.c., and titrated with standard AgNO_3 solution, thus giving the chlorine in 1 c.c. of evaporated solution.

I will merely quote a few figures, and give an idea of how much ether and chloroform are given off at various stages of evaporation. They refer to the percentages of chloroform in the vapour after certain percentages of the mixture have evaporated.

Percentage Evaporated.	Percentage of Chloroform in Vapour.
10	14
20	22.5
30	30
40	37
50	44
60	49
70	53
80	57
90	60
100	62

We see, therefore, from these experiments that, even at the end, ether always remains with the chloroform. It may be a good material for producing gradual and safe anæsthesia. The presence of alcohol can be, I think, of no advantage. If it were present, its boiling-point being about 78°C ., while that of chloroform is 62° , the alcohol would probably remain for the most part behind. The problem would be complicated, but the ratio between chloroform and ether would probably not be altered. The vapour of the alcohol would, however, mix with the other two in small proportion, and would thus diminish the proportions of ether and chloroform in the total vapour.

I have some diffidence in bringing these results before a Society of specialists, as I have had little experience in their domain; yet if I have offered any hints likely to prove of use to fellow-workers in another region of science, I shall feel most amply rewarded for any trouble I may have taken.

ULCERATION OF THE STOMACH.

DUNCAN, in the 'Intercolonial Medical Journal of Australasia' of March 20th, 1898, says that in France at least this malady is receiving a great deal of surgical attention. M. Hartmann, at a late meeting of the Société de Chirurgie, gave his experience of the treatment of this grave malady by operative measures. The symptom chiefly calling for operation was the presence of grave hæmatemesis. The results quoted are not encouraging, since in a series of twelve operations no less than eight fatalities occurred. The operation adopted is that of gastro-enterostomy—an operation which at first sight seems scarcely calculated to attain the end in view. It is claimed, however, that the operation gives repose to the stomach, and lessens or does away with contraction of the pylorus—two conditions which favourably influence the ulcerative process. M. Hartmann has also adopted the same measure with success in a grave form of dyspepsia, with pyloric spasm, pain, and vomiting. So far as the spasm is concerned, he holds that it is benefited, whether it may be due to an ulcer, or to hyperchlorhydria or anachlorhydria. He prefers an anterior gastro-enterostomy, and insists on the importance of fixing the bowel over a large extent of the anterior surface, so as to avoid a spur. It is in addition fixed obliquely from below upwards, and from left to right. He discards any form of anastomotic button. He first of all makes a double suture with silk (*en sujet*). The first suturing includes all the tunics of the stomach and intestine, and is hæmostatic. The second suture is sero-muscular, and covers the first.

In the discussion which followed, M. Routier related the case of a patient whom he had had under observation for a time. The patient was aged 45 years, and had been the subject of grave dyspeptic troubles for a considerable period, with extreme pain and vomiting. He had thought of

both ulceration and cancer in connection with the case; but as the patient continued to live, he found it difficult to conclude what was really the matter. Gastro-enterostomy was finally performed in May, 1897, and, singularly enough, no observable lesion whatever was detected. The gastro-enterostomy was made posteriorly with Murphy's button. Since then the patient's health has continued excellent, and all his symptoms have quite disappeared.

M. Tuffier raised the question of the diagnosis between ulcer and erosion in cases of grave hæmorrhage from the stomach. He considered that clinically it was impossible to differentiate the two conditions. During an operation the ulcer could, of course, be felt, but an erosion would only be seen during an autopsy. He preferred to do the operation posteriorly, as the anterior method involved a risk of too much flexion of the intestine unless the union was made over a considerable extent. One objection to posterior gastro-enterostomy was that a breach was made at the level of the mesocolon, which might ultimately allow strangulation of intestine to take place; but if care was taken to accurately close the opening that objection was done away with.

Therapeutic Gazette, November 15th, 1898.

GONORRHOEA IN THE FEMALE: RECTAL COMPLICATIONS.

By J. B. BACON, M.D.

It is very well known that authorities upon rectal diseases have little to say on the subject of gonorrhœa of the rectum. It is true they speak of it as occurring, and recommend some simple method of treatment, without going into the pathology or the results of gonorrhœa. It is very uncommon in our country. I have seen only two cases in the male. Most of them have been in females, where I could trace it directly to the nurse in using the same syringe for an enema as was used for vaginal douches. I understand it is comparatively common in France and other foreign countries. In my personal experience I have seen serious results from gonorrhœa. It is now conceded that in gonorrhœa of the rectum we have the columnar epithelial lining of the follicles involved, and the glands elsewhere, since Wertheim demonstrated

that it not only involves the mucous membrane of the bladder, but submucous tissues and the muscular structures and walls of the bladder. We even find it in the veins external to the muscular coats of the bladder. We have no reason to doubt, although it has not been well established, that ischio-rectal abscesses or that pelvic peritonitis occur at the same time a patient has gonorrhœa of the rectum or the vagina, and that sometimes pelvic peritonitis may be due to gonorrhœal extension from the rectum instead of from the uterus and Fallopian tube.

As to the dangers of gonorrhœa of the rectum, the disease probably results in seventy per cent. of the cases in the formation of stricture of the rectum. The best authorities upon rectal diseases have claimed only that about eighteen per cent. of strictures of the rectum are due to syphilis. Formerly it was claimed that fifty or more per cent. of the cases were due to syphilis. They are becoming more and more convinced that strictures of gonorrhœal origin are produced in this way. In gonorrhœal infection of the rectum the follicles as well as the glands are involved in the pathological process, and an ulcer forms. When an ulcer forms in the rectum it is impossible to cleanse the parts by any method of treatment, so that the ulcer having once formed in the rectum becomes chronic; new fibrous tissue is formed as the result of inflammation in the neighbourhood of the ulcer, the chronic inflammation produces hyperæmia of the fibrous tissue in the vicinity of the rectum also. The chronic irritation at the seat of the ulcer upon the nerves adjacent produces a spasm of both the circular and longitudinal muscular fibres in that locality, and this spasm being continued indefinitely, finally the muscle from overstimulation dies, and, according to Cripps's idea, we have two pathological conditions that produce strictures of the rectum. The reason we have not found more cases of gonorrhœa of the rectum is easily explained. According to the statistics from the large hospitals in London strictures of the rectum in females are ten times as frequent as in males; hence we can readily see how these strictures would be produced by careless people, particularly the lower classes of people, who never stop to think when they have a gonorrhœal vaginitis, but use a syringe which infects the rectum. Again, if the patient has gonorrhœa ex-

tending to the endometrium, then into the Fallopian tubes and developing a pelvic peritonitis, the pain from the inflammation and ordinary symptoms of gonorrhœa in the rectum are so much less intense that the attention of the physician is rarely called to the matter. On the other hand, an intense pain in the pelvic peritoneum would call the attention of the physician to it, and an examination would be made. In many cases the rectum is overlooked. The methods of the last few years in matters of diagnosis are so accurate that the future will give us more exact statistics.

The treatment of gonorrhœa of the rectum does not differ materially from that given to any other part. The rectum has an absorbable mucous membrane, so that liquids can be taken up as readily by it as by the stomach, and it is dangerous to use bichloride or the different antiseptics. A better plan of treatment is to keep the patient at rest, and to use a double tube just within the sphincters for irrigation purposes, gradually inserting the tube slowly with an outlet for the flow of water to prevent washing or carrying the infection higher up in the intestinal tract. The bowel should be thoroughly irrigated several times a day with hot water. You can use the milder forms of antiseptics, but there is always danger of absorption. There is another reason why gonorrhœa of the rectum is so often overlooked. Except for the first few days, while it is acute, there will be a red, swollen, and inflamed condition of the parts, and a burning pain with diarrhœa from the extra secretion. If the ulcer is above the sphincter muscles we have to do with the sympathetic nerves, and the patient may not complain of much pain. If the ulcer is within the course of the sphincter muscles or near the anus or anal canal, then the pain from the nerves is intense. Such patients will require occasionally an anæsthetic. The anal ulcer is to be treated by divulsing the sphincter muscles, efforts being made to overcome the spasm, and then treating the ulcer locally the same as we would an ulcer elsewhere.

I remember distinctly a case that had been treated for cystitis for three weeks, and after irrigating the bladder the physician treated the symptoms by giving internal medication. As soon as I examined the urine I made up my mind that the bladder was not involved. In making an examination of the rectum I

found an anal ulcer and reflex symptoms. You get these symptoms sometimes where there is no pain—where an ulcer in the anus does not occur perhaps in one case out of twenty. The pain is being reflected back by the sacral plexus or pudic nerve, and the result is we have pain in some other branch of the sacral plexus of nerves instead of at the seat of the ulcer. This is a practical point to remember, but one which is frequently overlooked. When we have a case of extreme spasm of the neck of the bladder—I do not care what the case is—it is a good idea to see if we have an ulceration within the sphincter.—*The American Gynecological and Obstetrical Journal*, Nov., 1898.

Treatment of Scarlet Fever.—The treatment of this disease must be symptomatic. In mild cases it should be the same as that of other fevers. The patient should be placed in a well-ventilated room, with a light diet and careful nursing. The body should be sponged with tepid water, simple salines given internally, and the sore throat relieved by sucking ice. If the tonsils are much swollen, and covered with secretions, the latter should be removed, and disinfectant or astringent solutions applied. Perchloride of iron, chlorinated soda, and dilute hydrochloric acid may be used, and similar remedies may be also used to syringe the nose when that is involved. Hot fomentations or linseed poultices may be used when there is much swelling, or pain in the neck and about the angles of the jaw. Abscesses should be opened early. If otorrhœa is present, the meatus may be syringed with warm water, a solution of boric acid (1 in 20), or dilute peroxide of hydrogen. The salicylates may be used when rheumatism is present. In severe typhoid forms, with quick, feeble pulse, stimulants, as ammonia and brandy, are called for. Where there is high fever, with delirium and restlessness, cold affusions to the head and body often give relief. When necessary the patient should be quieted by a solution of the five bromides. Antipyretics should be used with care. During convalescence the patient should have the greatest of care. As the chief danger lies in the renal complications, care should be taken that the patient is not exposed to the cold or draughts as long as desquamation is going on.—Dr. POTTER, *Annals of Gynecology and Pediatrics*, November, 1898.

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FRACTURES AND THEIR TREATMENT.

With reference especially to Fractures of the First Rib and Injuries of the Hip Joint.*

BY

W. ARBUTHNOT LANE, M.S.,

Surgeon to Guy's Hospital and the Hospital for Sick Children.

GENTLEMEN,—When your President, Dr. Parkin, did me the great honour of asking me to read a paper before this Society I accepted the invitation with very much pleasure. I did so without judicious consideration, not recognising at the time that I was merely affording myself gratification, and forgetting that I had nothing sufficiently interesting or new to justify me in taking up your valuable time.

Still, gentlemen, I trust that you will deal lightly with me in this matter, and accept my apologies for my shortcomings. Casting about in my mind what to talk about, I thought that the question of fractures and their treatment must always be one of considerable interest to you. Therefore I purpose making a few observations on fractures generally, and then passing on to the consideration of one in particular.

As you may perhaps know, it is now several years since I showed that the accepted teaching on the subject of the mechanics and treatment of fractures was false, and was founded on no scientific basis whatever. At first this met with almost universal opposition, your president being, I believe, the only supporter I had. Fortunately the advent of the X rays, about three years after, furnished me with undeniable evidence that could be applied to any and every case. I need hardly remind you of the teaching I refer to, since it still passes for truth in the works on surgery.

* Read before the Medical Society at Hull on 9th December, 1898.

Briefly I demonstrated—

1. That the supposition that the surgeon is able to restore the broken bone to its normal form, or, as it is termed, to set the fracture by manipulation, &c., and to retain it in position by splints and other means, is, except in a few cases of transverse fracture, quite incorrect.

2. That muscular contraction exerts practically no influence in opposing restoration of the broken bone to its original form.

3. That the whole of the soft parts surrounding the bone, whether muscles, skin, or fascia, act as ties in its length, and that the hæmorrhage into and beneath these ties, with the inflammation consequent on it, are the mechanical factors which prevent accurate apposition of its fragments.

4. That the only manner in which the bone can be restored to its original form is by an operation. When the fracture has been exposed, it is possible by the exercise of great traction upon the fragments, assisted by powerful lion forceps and elevators, to fit them accurately together. The difficulty in doing this is very much greater in the case of the lower extremity than in that of the upper; indeed, there are few operations which are more difficult than the restoration in form of the tibia and fibula broken by indirect violence. It is the very great difficulty that surrounds the operation in this particular locality that will always stand in the way of its general application. Amongst many evidences of this I have seen exposed in the shop windows in London radiographs of such fractures treated in some cases by wire and in others by screws, and the somewhat startling results they show must prove a curious object lesson to those gentlemen who used to assert positively that however unsuccessful others were, they themselves were able to restore these bones when broken to their original forms. Fortunately the radiograph enables the facts to be clearly recognised. I will merely refer to two of these radiographs which were taken after operations, done presumably by a capable surgeon possessing very considerable dexterity.

In each case both bones were broken above their centre. In one the overlapping ends were shown connected loosely by wire loops, and in the other they were retained firmly in this abnormal relationship by a screw, a considerable portion of whose barrel projected from the fragment it entered,

losing therefore any approximating capacity it should exert. Clearly in neither case did the patient benefit in any way from the operative interference. You recognise from an examination of them that very great obstacles may have to be overcome under these circumstances. A complication of considerable importance is that of comminution of the ends of the fragments. This often adds largely to the difficulties that surround surgical interference, and will sometimes tax your ingenuity to the utmost.

You can generally fasten the fragments to the shaft and to one another with silver wire, while sometimes a loop encircling the fragment and shaft may be of more service. These are, however, details. What I learnt from this particular complication was that one could utilise fragments of bone which were quite loose in the wound providing they were secured firmly in position. This led me to employ the bones of other animals in these and similar conditions when additional support was required. I found that it was necessary to fix such bones sufficiently firmly to prevent any movement in order to ensure their living. Then I determined to go a step further, and introduce an articulation. This I did about a month ago, when I resected an ankylosed elbow-joint and put in its stead the radio-carpal and metacarpal joints of a sheep. This animal was killed in the immediate vicinity of the hospital, and the part came into my hands a minute after its death. Every precaution was taken against infection of the wound. The opposing surfaces of the sheep's and of the human bones were fastened together as securely as possible by means of screws and loops of silver wire, when the new elbow-joint permitted of a range of movement around a transverse axis which was quite as extensive as in the normal arm. After the bone had been in position for about ten days some suspicion arose as to whether the wound, which was quite healed, was sterile; consequently it was opened at one point, and some of the fluid which escaped was cultivated. This was found to contain *Staphylococcus aureus*. Fearing to expose my patient to any avoidable risk I exposed the bone, which was clearly dead. It was removed at once. There can be no reasonable doubt that the infection took place through some loophole in our precautions, and I can see no reason why the operation should

not succeed if we render our precautions against infection more perfect. I certainly intend to make another attempt as soon as I get a suitable opportunity. The difficulties that are inseparable from this operation are those of securing the bones immoveably together, and these are greater than would perhaps occur to you. In the operation I performed I sawed through the humerus and ulna at right angles to their length, dividing the bones of the sheep in a similar manner. To secure these surfaces immoveably on one another by means of screws and wire is very difficult indeed. I might perhaps have rendered this more easy if I had sawn these bones through at an angle of 45 degrees to their length, but this was found on experiment to offer other obstacles. Should it prove possible to introduce a joint as readily as it is to fasten in a large single bone, it will soon alter the whole of the surgery of the diseases of joints. I am very sorry that I am at present unable to show you such a condition as I had hoped, but trust to succeed shortly. After these preliminary remarks I will now discuss some complications of fracture of the first rib.

In short papers in the 'Transactions of the Pathological Society' for 1884 and 1885, entitled "One Mode of Fracture of the Sternum" and "Mode of Fracture of the First Rib alone," and more fully in a contribution to the 'Guy's Hospital Reports' for 1885, dealing with "The Changes produced by Pressure in the Skeleton of the Trunk and Shoulder Girdle," I explained clearly the mechanics of the shoulder girdle, and showed the manner in which the weight of the arm is transmitted through the clavicle, costal arches, and sternum to the spinal column, together with the way in which the sternum and costal arches can be fractured by an excessive force applied to the outer extremity of the clavicle through the leverage action exerted by it.

At that time the matter had received no attention whatever from anatomists as far as I was able to learn, and the surgeon was entirely unaware of the existence of fracture of the first rib alone, quite apart from possessing any knowledge of the mechanics of its production. Indeed, he went so far as to assert that "fracture of the first rib does not occur, *because it is sheltered by the clavicle.*" I demonstrated experimentally in the dead body that the first rib could be broken at any point in its

length by the application of force to the outer extremity of the clavicle in a definite direction, varying from a directly downward one to one backwards and slightly downwards, and proved, both by instances of this fracture found after death as well as during lifetime, that it is an accident of comparatively common occurrence. It escaped notice because the shoulder girdle had no physiology in the text-books, and the surgeon therefore did not know that the clavicle exerted any leverage action on the costal arches. Curiously enough, the reverse of the teaching of the time is true, for the very reason given, namely, "*the first rib is frequently broken because it is sheltered by the clavicle.*"

I would point out that though the mechanics of the shoulder girdle were fully considered and explained in those papers, and though the knowledge of them is of immense importance to the surgeon, yet the anatomist has not thought it worth while to add anything to the very imperfect, insufficient, and unsatisfactory descriptions of the physiology of the articulations of the shoulder girdle which exist in the text-books on anatomy provided for the purpose of imparting useful and reasonably accurate information to students.

It therefore becomes necessary for me to recapitulate very briefly some of the points described at length in those papers.

The weight of the arm, with that of any additional burden borne by it, is transmitted to the spinal column chiefly through the clavicle, which in the erect position under these circumstances forms a lever of the second order. Its fulcrum corresponds to the sterno-clavicular articulation. The weight of the arm is represented as the power acting at the outer end of the clavicle in a direction vertically downwards, while the weight is transmitted to the costal arch at the point where the clavicle rests upon it, which is about the costochondral junction. The force applied to the end of the clavicle depresses the costal arch on the same side, rotates the manubrium around an antero-posterior axis should a manubrio-gladiolar joint exist, and raises the first costal arch on the opposite side. If a joint is not present in the sternum the entire bone is rotated slightly around an axis passing through the manubrium. If sufficient force is applied suddenly to the outer end of the clavicle, should the lever itself not break or its fulcrum yield from dislocation, the costal arch

fractures at the point of impact. If still greater force is used the second arch may be broken also in the same manner; and if a joint is not present in the sternum, this bone may be fractured about the manubrio-gladiolar junction.

If force be applied simultaneously to both clavicles both first arches may be broken, or the manu-

impact (see Figs. 1 and 2). The tendon of the subclavius muscle and the rhomboid ligament are in consequence displaced to some extent, while a complete capsule is formed about it. In most occupations the load is supported by the hands upon the back or shoulder, consequently the shoulder-joint is retained in a position of forcible

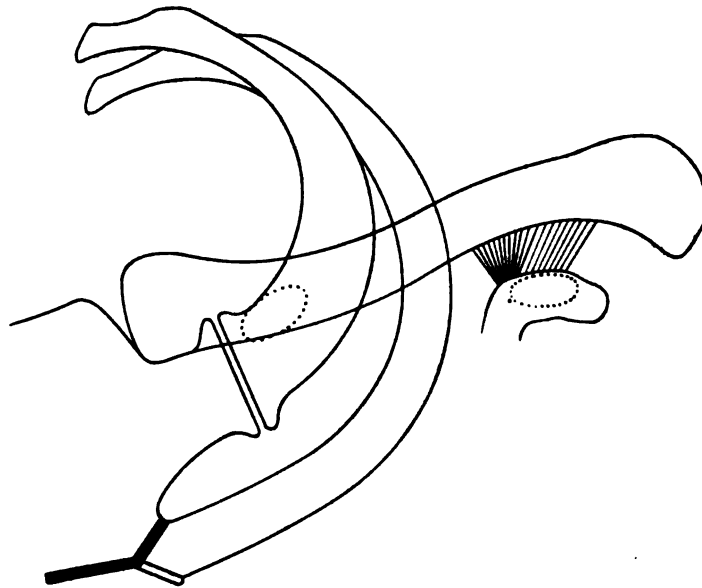


Fig. 1. represents the left first and second costal arches, with the manubrium, clavicle, and coracoid process, of a labourer. The manubrio-gladiolar joint is amphiarthroidal in character, while the joint which has developed in the ossified first costal cartilage is freely arthroidal. The position of the costo-clavicular articulation is indicated by the dotted outline on the first arch. On the upper surface of the coracoid process the facet which articulates with the clavicle forming the coraco-clavicular joint is similarly indicated.

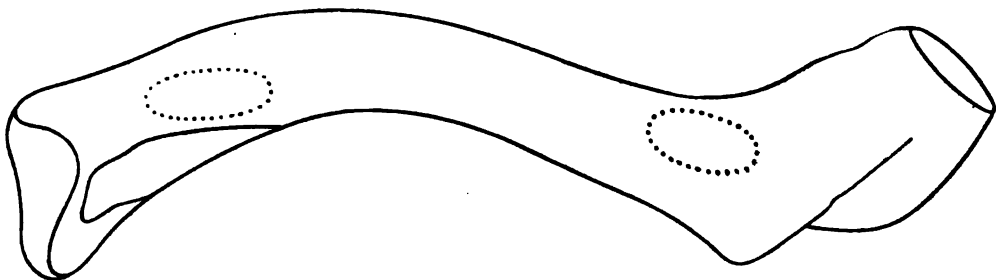


Fig. 2 represents the under surface of the clavicle with the articular facets which correspond with those on the costal arch and coracoid process.

brum may be displaced backwards off the gladiolus at the joint, or if this is absent the bone may be broken in the same position.

If force be applied habitually in this direction, as in the case of the labourer who carries loads upon one or both shoulders, a new joint is developed between the clavicle and costal arch at the point of

and complete flexion. I showed that flexion of the shoulder-joint is limited by the impact of the coracoid process against the under surface of the clavicle, and that when sufficient force is applied abruptly in this manner the coracoid process is fractured by indirect violence, and not by direct violence, as was supposed by surgeons

at that time.* In the case of the labourer the habitual forcible apposition of the clavicle and coracoid process results in the formation of a new joint between these bones, a complete capsule being formed about it. This I have indicated in Figs. 1 and 2.

If the load be carried on the back, as in the case of the coalheaver, the manubrio-gladiolar joint, should it exist, is altered in character, owing partly to the tendency to backward dis-

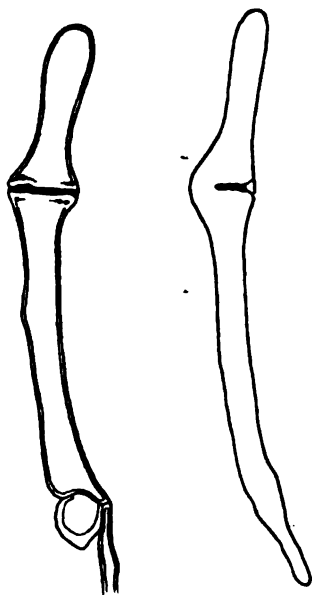


Fig. 3.

Fig. 4.

Figs. 3 and 4 show the changes that take place in the manubrio-gladiolar joint in consequence of the transmission through it of great pressure. These specimens were obtained from the bodies of labourers who had been engaged in heavy portering work at the Docks.

In Fig. 3 it is seen that the opposing surfaces of bone have been increased in area considerably, while the intervening ligamentous tissue has been rendered scant and dense. In Fig. 4 the joint has been almost completely obliterated by being bridged over in front, and by the formation of masses of bone in the ligament posteriorly.

placement of the manubrium off the gladiolus, and partly to the large amount of force transmitted through this joint.

As old age advances the first costal cartilage becomes converted into bone by a process of ossi-

fication which extends into it from the manubrium and first rib along its periphery into its centre. This ossification takes place comparatively early in life in the labourer under the influence of excessive strain, and in proportion to the weight of the load carried. Owing to this progressive rigidity of the arch an amphiarthrodial, and later an arthrodial joint is developed in the substance of the ossifying cartilage to enable it to perform its normal share in the mechanics of the shoulder girdle. This is shown in a well-developed state in Fig. 1.

If sufficient force is applied to the outer end of the clavicle in any direction from vertically downwards to almost directly backwards, it will transmit that force to some point in the length of the costal arch, and will break the bone at the point of impact. Should the patient who has sustained such a fracture continue to use the arm, a large quantity of callus will be thrown out because of the free movements of the fragments on one another, which of necessity result from the force exerted on the arch by the clavicle during the movements of the arm; and if the arch has yielded in its anterior half the constant movement may result in the formation of an ununited fracture. When I was first interested in this matter I found among the few bodies in the dissecting-room at the time three first ribs fractured at different points. Those I preserved, and they are mounted in the museum of Guy's Hospital, No. 1054³⁵. They show a united fracture of the neck of the rib, with some displacement, an ununited fracture of the centre of the rib, and a united fracture of the anterior portion of the rib. A little consideration enables one to realise that displacements can only occur in fracture of the neck of the rib, because occurring inside the articulation of the tuberosity of the rib with the transverse process of the vertebra, the weight exerted through the clavicle raises the outer fragment.

(To be continued.)

The Diagnosis of Sciatica.—According to the 'Philadelphia Polyclinic' for November 12th, the sign of Lasèque, as it is called by the French, is a valuable one in the *diagnosis of sciatica*. If the thigh is flexed upon the pelvis, with the leg fully extended at the knee-joint, considerable pain is produced when sciatica is present, as the sciatic nerve is in this way stretched.

New York Med. Journ., Nov. 26th, 1898.

* "Mode of Fixation of the Scapula suggested by the Movements of that Bone in Extreme Flexion of the Shoulder-joint; its bearing upon Fracture of the Coracoid Process," *Brit. Med. Journ.*, May 19th, 1888.

WITH DR. DONALD HOOD IN THE WARDS OF THE WEST LONDON HOSPITAL,

October 24th, 1898.

GENTLEMEN,—This man came to the out-patient room complaining of great pain in his leg and hip. He was stated to be suffering from sciatica, of which he had had two or three attacks. As he could hardly walk he was admitted as an in-patient. When in the hospital he complained more of a pain over the region of the ilium than along the course of the sciatic nerve. I turned him over on his face and examined him carefully, and was then struck with the fact that the pain, tenderness, and general discomfort were referred to the sacro-iliac joint, and I have no doubt that the pain is referable to some irritation or inflammatory mischief about that joint. I would here say that it is a very common thing to get inflammatory mischief at this joint, and it is nearly always considered to be sciatic. As a matter of experience, I am surprised at the number of cases which come to me complaining of sciatica, but in whom the mischief appears to be connected with the sacro-iliac joint. I have seen it follow strains of various sorts. For instance, the last case I had was that of a man who, while hunting, turned round to open a gate; it slipped through his fingers and he felt a wrench in his back. That patient did not come to me complaining of a pain in his back, but of sciatica. I recommend you to examine carefully the sacro-iliac articulation in all cases of long-standing sciatica. A large proportion of cases of alleged sciatica which have come to me have turned out to be instances of trouble in that joint. I want you to remember how easily that joint becomes inflamed. These cases require a little more definite treatment than ordinary cases of sciatica. A striking case happened to me some three years ago. A young soldier was sent home from Malta invalided from malarial fever; he came to me to be treated for that. He was debilitated and weak, but rapidly recovered from his malaria. A few weeks later he was playing in a cricket match, and lay on the wet grass. His father wrote to me, and stated that his son, although recovered from the fever,

had now got very severe sciatica. I thought he was young to have sciatica so severely, and said I should like to see him again. When he came to my room I was surprised to find tenderness and pain over the sacro-iliac joint. The patient did not improve, and I was obliged to put him into the hands of a surgeon, who cut down upon the joint and removed from it some grumous material, after which recovery was rapid. I believe most cases, if seen early, are easily cured; but I know that it is a popular feeling among medical men that these cases of sacro-iliac trouble are of a very grave nature, and that they are not speedily relieved. [Dr. Hood then demonstrated the seat of pain and tenderness in the patient.] I believe that in a short time this patient will be well again. He will be kept quiet in bed, and well blistered over the joint.

Fictitious Perityphlitis.

This child, while walking, was tripped up by a man, and fell. He complained of pain in the abdomen, and on examining him I found that he had swelling in the appendicular region on the right side. Here the swelling ran up from the brim of the pelvis, encroaching upon the iliac region. I did not think the trouble in this case was due to the appendix, but to cellulitis set up by definite strain. He has now been in a week, and nearly the whole swelling has disappeared; there remains only a small portion of infiltrated tissue.

Thoracic Aneurysm.

This man came with pain in the chest; he also complained of a swelling. On the right of the sternum he was found to have a pulsating tumour, which can be nothing but an aneurysm of the aorta. It is uncommon for such patients to come complaining of a lump. He had no dyspnoea, no cough, and no lung trouble. There is no difference in the arteries on the two sides, no difference in the pupils. Since he has been under treatment the pain has almost gone. He has been kept at rest, with good food, and has been taking iodide of potassium. The tumour has also diminished in area, and I therefore have no doubt that satisfactory clotting is going on, and a cure is to a certain extent taking place. Aneurysms in the chest are not always so easy to diagnose as the one before us.

A patient was admitted under my care suffering

from bronchitis. He was a healthy, strong, vigorous man of thirty years of age, and his symptoms were those of most severe bronchitis. Bronchitis in a healthy-looking man of thirty years is a rare disease. There was no influenza in those days. He was suffering greatly, was cyanosed, had great difficulty in breathing, and his lungs were engorged. I said to my house physician that this seemed to be one of those cases in which venesection might be useful. We accordingly made use of the lancet, and the result was that the man was wonderfully better. I remember saying that many people despised the old-fashioned remedies, but the bleeding has placed this man out of danger. But on my next visit I found the patient in very much the same condition as he was originally; so the lancet was used again, and with the same immediate benefit. But when after the second bleeding the man went back to much the same condition, it was clear enough that we had used the remedy sufficiently, there must be some deeper cause for the symptoms. Eventually we found there was a small sacculated aneurysm pressing on the bronchus. It showed how easily one may be led astray by a deep-seated disease which has not come to the surface, and which is only declaring itself by indirect symptoms.

Duodenal Ulcer.

This man, æt. 28, was admitted from the streets after severe hæmorrhage. I may say at once that it is a case of hæmatemesis. I do not think there was any practical difficulty in making a differential diagnosis between hæmatemesis and hæmoptysis. Our patient was brought in by the police, who saw him vomiting a large amount of blood. He was very depressed and blanched, and had evidently lost a great deal of blood. We found nothing important in the family history, and practically nothing in his own personal history beyond the fact that he had sometimes suffered from epistaxis, but that symptom I think we may put aside as I do not feel it has anything to do with his present condition. One's first impression on having to deal with a very severe case of hæmatemesis, especially in a man of twenty-eight years of age, is that it is more likely to be portal than anything else. If you collect a large number of cases of hæmatemesis you will find that the major portion of them are due to portal congestion—passive congestion due to alcohol. But we were saved from the difficulty of excluding

alcohol in investigating this man's condition, for he is and always has been a total abstainer. With such a history we therefore thought of another though less common cause of hæmatemesis, namely ulceration of the stomach. But our patient had had no dyspeptic symptoms of moment, no gastritis, no pain after food. Therefore it seems probable that this man is suffering from duodenal ulcer. Duodenal ulceration does not give rise to so much pain and gastric disturbance as ulcer of the stomach, but these ulcers are very liable to bleed and to perforate. Duodenal ulcer is rather a rare condition. Its symptoms are often very slight. The patient has been treated with nutrient enemata for several days. He is now able to digest half diet, and has had no bad symptoms since he came in.

Acute Apex Pneumonia in a Child.

This next case is a very interesting one to me, and I hope it will be so to you. A little lad eight years of age was brought to the out-patient room a few days ago, evidently very ill. He was restless, excitable, delirious; at times was screaming out, and intolerant of light. Altogether he manifested in a very marked form what we are in the habit of regarding as cerebral symptoms. When he was brought up to the ward his temperature was found to be 104° ; respiration 60 in the minute. He was very carefully examined, but the only lesion which could be detected was a slight amount of consolidation at the apex of the right lung. Within a few hours of admission there was a little tubular breathing noticed, which only lasted for a short time. My house physician in calling my attention to the clinical facts which he had noticed, questioned whether that small patch of consolidation at the apex of the right lung was sufficient to cause the extreme state of cerebral excitement. Well, speaking from my own experience, I can say that cerebral excitement, rapidity of pulse, and high temperature are very common occurrences in children who are the victims of lobar pneumonia; I would add, especially so in pneumonia of the apex. It is a condition the cause of which is constantly overlooked, inasmuch as that the degree of cerebral excitement and delirium are sufficient in many minds to class the case as one of tubercular meningitis. Yet it is the rarest event in the world for a case of tubercular meningitis to assume such a violent character in the first few hours of

the illness. Therefore, if you find a child breathing quickly, with a high temperature, with so-called cerebral symptoms,—intolerance of light, screaming, and delirium,—go over the chest very carefully, and if you find the slightest symptom of inflammatory mischief about one or other apex, I think you may be reasonably sure that the child is suffering from croupous pneumonia, and that the symptoms, cerebral as well as the others, are due to pneumonia. These storms of cerebral excitement are quickly over, as is exemplified in this case. This child was acutely ill on the 17th, on the 19th it reached its crisis, and now (24th) you see the child practically well. Another interesting point in these cases is the rapidity with which the physical changes take place. This patient had to be watched practically from hour to hour to catch the tubular breathing. I have seen many cases of this sort called tubercular meningitis. I should warn you that enteric fever does very rarely commence with the same character of alarming symptoms.

"Patchy" Nephritis.

Some months ago this child fell in the Thames. A few days ago it was brought up here suffering from bloody urine and some anasarca. We found that the urine was of low specific gravity, was of cherry colour, and contained a small amount of albumen. Therefore I think the child had congestion of the kidney due to the immersion in the river; a damaged spot was left, some fresh trouble brought on acute inflammatory mischief. A rapid recovery is taking place. This is not a case of acute nephritis as one generally understands that term. In acute nephritis only a small portion of the kidney may be affected, or only one kidney may be inflamed; hence we get a marked variety in the gravity of symptoms. I remember a child who was under my care some years ago. The cot was placed by the side of a window where the glass came down level with the bed. There was an exceptionally cold night, the papers calling attention to the extremely low temperature. Forty-eight hours afterwards this child who had been lying close to the glass developed acute lobar pneumonia in the right lung, and also acute nephritis. The little patient had become very thin, and I was able to distinctly feel the swollen right kidney. My impression was that it was

a passive engorgement due to cold, which was also the cause of the pneumonia. Here, although there were pronounced symptoms of acute nephritis, the child scarcely suffered from any results of such local inflammation. I believe that cases of one-sided nephritis generally get well, whereas in double nephritis the patients die, there being a common outlet for the products of the two kidneys; it is very difficult to be certain, from urine testing, whether one or both are diseased.

A Case of Gastric Ulcer.

This woman is aged twenty-six. She was admitted here on October 18th with symptoms referable to the stomach. I called attention in the other ward to the reasons which led me to believe that a patient had duodenal ulcer. I said that such patients were often without those positive symptoms that we meet with in gastric ulceration. Here we have what may be regarded as a typical example of gastric ulcer. A year ago this woman had a good deal of trouble in connection with her food, indigestion, pain after food, a considerable amount of waterbrash,—that is to say, filling of her mouth with a watery fluid,—a good deal of pain in the epigastric area—the pain passing to the back. She was seized with violent hæmorrhage. Here we had the crucial symptom. She went on fairly well from that time, sometimes getting better, sometimes worse. She now applies for relief from her gastric symptoms. I have no doubt from what she tells me that she is suffering from a recurrence of that gastric ulceration, which gave rise to the hæmorrhage and inconvenience fourteen months ago. She has pain over the gastric area, which is relieved by vomiting. She is again suffering intensely from watery brash. She has again experienced discomfort after food. I think in examining such a case as this it is advisable to pay more than ordinary attention to the individual symptoms. My own attention has been directed especially to the symptom hæmorrhage as influencing us in diagnosis of gastric ulcer. I will say at once that I am very strongly of the belief that we are sometimes led astray by the fact that we regard hæmorrhage alone as being a symptom of greater importance than it really is. I believe there is a certain class of girls especially with constipated bowels and general disorder of the dynamics of the circulation, who bleed as readily from the stomach as some

people do from the nose. Therefore, when I see an anæmic girl who, without very marked symptoms of gastric disturbance, has an attack of hæmatemesis, I have for some years dealt with her in a little different way from what I did twenty years ago,—that is to say, I often begin by giving aperient medicine, and I feed such cases sooner than I was accustomed to do. I do not say that if you get a case of bleeding from the stomach you are always to give an aperient and put the patient on solid food, but you will find in many such cases that after the first few days there are no marked gastric symptoms, the patient being free from all pain or discomfort, you will often be justified in feeding such a case more liberally and sooner than in another where the symptoms are more decidedly those of ulceration. In the class of case to which I refer anæmia is a prominent symptom. With every anæmic girl you are likely to get dyspepsia, and you are likely to be too ready to conclude that because such a patient bleeds from the stomach she has necessarily gastric ulceration. Some years ago I was particularly interested in this matter, and a friend of mine compiled a table of all the cases of hæmatemesis which were admitted into Guy's Hospital for twenty years. We were surprised to find that out of all the large number admitted there was not one who died from this particular form of hæmorrhage which I have ventured to call *hæmatemesis puellaris*. The case before us is, however, distinctly the reverse of those I have referred to. It is a case in which we can have very little doubt there is an organic lesion of the stomach. The patient has had pain, discomfort, vomiting, and hæmorrhage, and she has now what is very common in connection with gastric ulcer—a recurrence of all the symptoms. If you take cases of subphrenic abscess, due to the perforation of a gastric ulcer, you will be surprised to find how rare hæmatemesis has been during the entire course of the disease. You will see many cases of stomach ulceration in which the symptoms have been looked upon as simply gastritis, or you will meet with that terrible sequence of events following perforation of an ulcer, in which there has been no premonitory symptom of any form of gastric trouble. We are treating this case with bicarbonate of soda, tincture of opium, and calumba. I like opium better than bismuth, and I am accustomed to give opium in most cases in preference to that drug.

Mitral Stenosis.

The next patient is a woman aged 36. She was admitted six months ago when her life was in great jeopardy. She was cyanosed, breathless, had pain over the chest, slight œdema of the legs, and very rapid action of the heart. There was no bruit to be heard. I think I may say that in this stage of her illness it was impossible for anyone to have given an accurate diagnosis as regards the physical condition of the heart. Within a few days of admission the symptoms improved, her colour became more natural, the œdema left her, the dyspnœa was diminished, and as these symptoms lessened a very marked presystolic bruit appeared, and there was now a sufficient warrant for our diagnosis of mitral stenosis. There is no class of case which shows a greater variety of changes or symptoms, both subjective and objective. They vary immensely in their general symptoms, and the knowledge of this fact should suggest caution in making a diagnosis simply from using the stethoscope. It does not follow because you do not hear a bruit that the cardiac trouble may not be very great. This woman's life, as I have said, was in great jeopardy, principally from passive congestion; as frequently happens in such cases, she caught cold, bronchitis supervened, the lungs were engorged with blood, and the heart rapidly became more and more embarrassed. During the last month or two she has made very good progress, but she is not able to move from her bed. She probably has but a small slit representing her mitral orifice. Knowing this we cannot anticipate improvement sufficient to justify active life. We must try and give tone to the right heart and to the left auricle; we must improve the general nutrition, and avoid any tendency to pulmonary congestion.

Cases of mitral stenosis do not bear digitalis well as a rule. This patient was put upon it, but we very soon had to leave it off because of the slowness of the heart's action. In many of these cases of mitral stenosis you will find the pulse peculiarly slow. Only a few months ago a patient was admitted with the heart beating, it was said, only 40 to the minute. I have no doubt it was beating 100, but it was only observed at the wrist. On using the stethoscope we found that for every beat at the wrist there were three or four at the heart.

Cases of mitral stenosis may last for many years. They are described in our text-books as passing through stages, but there is no sharp line of demarcation dividing stage from stage; you will find authorities differing much in the importance they attach to individual symptoms as affording help to diagnosis. Probably these different opinions depend upon the stage at which the patient may have come under observation. Note that the patient before us on admission had no murmur. Within a few days a pronounced bruit could be heard. This bruit lasted for some four or five months, but has now again quite disappeared. In the meantime the first sound has become altered in character, and is now short and sharp; in fact, you could not distinguish between it and the second. In treating these patients you will be surrounded by dangers—dangers due to embolisms, pulmonary congestions hæmorrhages, bronchitis, and hepatic engorgement.

Bactericidal Properties of the Becquerel Rays.—Becquerel established that phosphorescent bodies, especially the salts of uranium, absorb the most refrangible and the ultra-violet rays, and emit rays that will pass through certain substances opaque to the sunlight. They will even pass through several substances opaque to the Roentgen ray, and can be refracted and polarised. Pacinotti and Porcelli have been studying their bactericidal properties, using for the purpose freshly pulverised metallic uranium, exposed to the sunlight, and then kept at a temperature of 44° to 55° C., without contact with the air. Various germs exposed to the action of the rays thus generated were all killed in the space of three to twenty-four hours, and the microscope showed alterations in their shape, indicating some chemic effect on the protoplasm (staphyl.; strept.; pyocyaneus; proteus; germs of cholera, tuberculosis, diphtheria, and typhus). They also injected 5 cm. of a very virulent bouillon culture of the streptococcus under the skin of the ears of a rabbit, and exposed one ear to the action of the Becquerel rays, which prevented the evolution of any inflammation, while the non-exposed ear ran the usual course of a streptococcus invasion.—*Journ. of the Amer. Med. Assoc.*, Nov. 26th, 1898.

MEETING OF THE SOCIETY OF ANÆSTHETISTS,

At 20 Hanover Square, November 17th, 1898;

The President, Dr. DUDLEY BUXTON,
in the Chair.

(Concluded from p. 126.)

Discussion on Professor Ramsay's Paper on Pure Anæsthetics.

THE PRESIDENT said they were all very much indebted to Professor Ramsay for the very careful way in which he had brought the matter before the Society, and he had no doubt that as a result of the discussion they might elicit even more information than had been yet given.

Mr. WILLETT said they were very much indebted to Professor Ramsay for giving them the benefit of his experiments. Some of the details were known to those members who had read Professor Ramsay's article in the 'Nineteenth Century.' He had read that article, and had been on the point of writing to ask one or two questions about it. He would like to inquire the quantities of slaked lime or mercury which should be put into a 10-ounce bottle of chloroform or ether respectively for the purpose of purifying.

With regard to the product carbonyl chloride, he had noticed it, and he knew some surgeons who recognised it so well that they absolutely refused to operate by gaslight if chloroform was necessary as the anæsthetic. One surgeon in particular, a friend of his, would postpone the operation rather than do it by gaslight with chloroform as the anæsthetic.

He thought that the test which Professor Ramsay had recommended, viz. to smell the substance, was an unfortunate one, because it depended on the individual very much. Personally, in smelling the samples which Professor Ramsay had handed round, he could not detect the difference between the pure and the impure by smelling.

Dr. Newman's tables, he supposed, were interesting, but the anæsthetic, they learnt, was administered in the Scotch way, which Dr. Newman called "the best." He thought they would not all agree with that on this side of the Tweed. The tables which he had constructed, to carry any weight,

required verifying in some way. He would like to point out a little difficulty about A.C.E. mixture experiments. In the way it was usually given a little of the A.C.E. was poured out from time to time, and if it was freshly mixed at the beginning of the operation the fluid would contain the same proportions and become volatilised on the inhaler, on the lint, or on the Skinner's mask, and there would be the same proportions at the beginning of the operation as at the end. In a Junker's inhaler the ether vapour would be carried off quicker than the chloroform, and the chloroform quicker than the alcohol. Therefore the details of the experiments which had been mentioned did not come in practically, although the experiments were valuable in themselves.

Dr. SILK said he would like to add his thanks to Professor Ramsay for his very interesting paper. The whole question as to the purity or impurity of the drugs with which they worked must of necessity be one that came home to them very vividly upon many occasions. There were several points which Professor Ramsay had raised, and many which he had not raised, which were interesting to them all.

First of all, as to the decomposition of chloroform as borne out by Dr. Newman's figures. He was not quite clear about that. It was, of course, rather unfortunate that Dr. Newman was not present, because one could not very well criticise his figures in his absence. But the point which struck him was that Professor Ramsay told them that the chloroform makers, a firm of good repute, claimed that their chloroform, to which they had added a minute quantity of alcohol, had remained pure for a very considerable time; and yet Dr. Newman, in using chloroform from this same firm, got very great differences and discrepancies between the effects of their chloroform and that purified with lime. That was the only criticism he would make on that point in the absence of Dr. Newman.

Then with regard to the question of decomposition of ether, he was not in the same difficulty as Mr. Willett about the quantity of mercury put in, because it did not seem to him that it mattered very much, since the substance produced was quite insoluble, and the mercury fell to the bottom; therefore they might have a pound or half a pound. In the same way he took it that it did not matter how much lime was put into the chloroform for the purpose of purifying it.

Since Professor Ramsay had published his paper in the 'Nineteenth Century' Dr. Silk had himself kept in his ether bottle about half a pound of mercury, and he gave it a stir every now and then. He could vouch for the presence of the black cloud which every one had seen in Professor Ramsay's bottle that evening, but what he was not by any means certain about was as to whether the ether so treated had been in any way improved. He had used the same brands of ether as he always had, but he could not really say that he had had any better effects produced or any worse effects. Of course that was purely negative evidence, and would therefore have to give way before any positive evidence which might be brought forward, such as that of Dr. Newman. He merely gave it as his own personal experience in the matter.

Of the points which were not touched upon by Professor Ramsay, there was one which was of the very greatest importance, and one upon which he would very much like to have the opinions of others of his colleagues,—that is to say, what preparations of anæsthetics ought they to use? For instance, to take chloroform: he thought that the consensus of opinion was in favour of the pure chloroform, and not that prepared from methylated spirit. On the other hand, with regard to ether, he believed it was rather a moot point. There were many who insisted upon using pure ether prepared from pure spirit, and others, again, who said that the ether prepared from methylated spirit, if it answered certain tests—that is to say, with regard to its specific gravity—its neutrality to test-paper, was quite satisfactory. He knew there were many anæsthetists who had not used anything else for years. It was a point of practical importance, because the price of pure ether was about double that of methylated ether. He admitted that he used the latter; he had used pure ether, but he could not say that his results with pure ether were any better than his results with the ether he usually bought. It would be useful if Professor Ramsay could suggest whether it would be more dangerous to use one rather than the other, and if so why?

With regard to the A.C.E. mixture, Mr. Smiles's work on that subject confirmed their own clinical experience, and was of great value because it had put into figures that which they had wanted so

long to know as to the relative evaporating rate of the constituents of the A.C.E. mixture. He did not know, but it did not seem to him that it very much mattered whether the evaporation was carried out from a sponge, or by blowing air through it, or by any other means. Mr. Smiles got 30 per cent. of ether by blowing air through it, and he (Dr. Silk) imagined he would get just the same amount from surface evaporation. He hoped that they would see all these valuable figures and observations in the 'Transactions,' and he was sure they would be welcomed not only by the members of that Society, but also by all who took an interest in anæsthetic questions.

Dr. AUGUSTUS COOK thought the plan, which he had usually adopted, of pouring the A.C.E. mixture on a Rendle's mask containing a sponge, would not be affected by the careful statement which they had heard from Professor Ramsay. Professor Ramsay had stated that the quantity of ether carried off was high at first, and greatly diminished in proportion to the quantity evaporated; but if, as stated by Mr. Willett, they added small quantities from the bottle from time to time by the method he had used, it did not seem to him that the plan would be injuriously affected by Professor Ramsay's statements,—he meant that the quantity would not vary at all. He would also like to ask Professor Ramsay whether carbonyl chloride could be developed from pure chloroform in any other way than by contact of its vapour with gaslight. For instance, if no gas was burning at the time that the pure chloroform was being given, could the carbonyl chloride be developed, for instance, in the lungs? Recently he was operating, and the administrator of the anæsthetic was giving chloroform by means of Junker's inhaler. The surgeon who was assisting him would not go on with the operation, because he said he had to breathe chloroform fumes in leaning over the patient's mouth. These fumes were so irritating that he had to stop in the middle of the operation. It occurred to him as Professor Ramsay was reading his paper that carbonyl chloride was the very substance developed in that particular instance. Did the fact point to the chloroform being impure? The patient, evidently an alcoholic subject, took a long time to go under.

Mr. MCCARDIE said, like Dr. Silk he would like to ask Professor Ramsay whether there was any

difference between ether prepared from methylated spirit and ether prepared from ethylic spirit. As regards impurities, he was told recently by a chemist that methylated spirit always had a certain amount of naphtha added to it. He wondered if any of the impurities came from the naphtha. He always used the methylated ether. He was perfectly satisfied with the substance he had become accustomed to. In the last case or two he had used pure ether, but he had not seen any difference in the results compared with the methylated ether.

He would like to ask the Professor whether the A.C.E. mixture was a purely mechanical mixture, or whether anything akin to chemical combination took place. When mixing the constituents oneself there was an evolution of heat, and that made him ask the question. Messrs. Duncan and Flockhart told him that their last chloroform was of a specific gravity of 1490, and was practically non-decomposable. He would like to ask the lecturer whether he thought chloroform of that specific gravity, containing 1 per cent. of alcohol, was practically non-decomposable,—that is to say, if it were kept in a well-stoppered bottle away from light and moisture.

Mr. CARTER BRAINE related two clinical cases which bore out what Professor Ramsay had told them about chloroform. The other day he had two consecutive cases for examination of the bladder, in which there was supposed to be villous growths. The operator asked him to give chloroform, so as to diminish any chance of bleeding, as he wanted to get a good view of the growth with the cystoscope. He took the bottle of chloroform, which was on the table, and commenced putting the patient under its influence with a Skinner's mask. The patient immediately began coughing, and continued to do so; and to his surprise, although he continued with the administration, the coughing did not cease until he had been administering it for fifteen minutes. Somehow it never struck him that there might be something wrong with the chloroform. The examination was concluded, and the patient left the theatre. The next patient came in, and he started administering the same anæsthetic on the same lines; this man also began to cough, and continued to do so. In this case the operation lasted fifteen minutes, during which time he had given an ounce of chloroform, but the patient coughed the whole time. At last

he thought he would change to ether, to see if it would stop the coughing. He did so, and the coughing almost immediately ceased. He regretted he did not keep a specimen of the chloroform. He was now inclined to believe that the chloroform was impure; it had been in the theatre a fortnight exposed to light. He would like to ask Professor Ramsay how long it would take for chloroform simply exposed in a colourless glass bottle to acquire the acrid smell which he described, and whether he thought that having the bottles coloured, as some manufacturers supplied, had any effect upon this change in the chloroform.

Dr. PROBYN-WILLIAMS remembered reading a report of an operation which took place near Geneva in the summer of last year, when a long abdominal operation had to be done in the middle of the night—he thought it was by the light of candles,—and in that operation both the surgeon and the nurse were coughing during most of the operation, and that afterwards the nurse had bronchitis, which was followed by catarrhal pneumonia, from which she died. He was not quite sure whether Professor Ramsay would lead them to expect that carbonyl chloride would be formed during an operation of some length if perfectly pure chloroform were used in the first place, or whether if it were formed on such a large scale it would make much difference to the quantity if pure chloroform were used, as compared with chloroform which had been left for a week or so exposed to the light.

In regard to Dr. Silk's remark about pure ether and methylated ether, he would like to say that for a good many years in the hospital to which he was attached they had been using nothing but pure ether up to the end of last year. Since then, primarily on account of the expense of pure ether, methylated ether was substituted after a good deal of opposition, and since then nobody had been able to detect any difference in the results obtained.

Mr. E. F. WHITE said he was sure all practical anæsthetists were much obliged to Professor Ramsay for coming to their Society and giving the chemical side of the question of anæsthetics. The question raised by Dr. Silk as to whether pure or methylated chloroform or ether should be used was an extremely important one. He had tried both in the hospital practice of St. Thomas's, and they had

decided in favour of the methylated chloroform and ether. He had also used pure ether and pure chloroform, but he could not find any difference as compared with the "methylated."

He would like to hear of what specific gravity the ether should be used. Years ago they tried for some time various specific gravities, and at last at St. Thomas's they settled down to '720. When they had '730, or below that, their results were not so satisfactory; their patients did not seem to go under so quickly. He would like to hear what specific gravity other members of the Society had been using.

He would ask another question, simply out of pure ignorance. Some years ago some French chemist brought to this country what was called Pictet's chloroform. The residue, after removal of the purified chloroform, was supposed to be excessively fatal, a few drops only being required to kill a rabbit in a very short time, with all the toxic effects of chloroform. Professor Ramsay told them nothing about that product, which came like a meteor, and, indeed, disappeared almost like one. He would like to know whether there was anything in it, or whether it was a fallacy.

Dr. Newman's paper was extremely interesting, and he should be thanked for the extreme care he had taken to bring forward those figures. The anæsthetic was given by the Scotch method. He thought they in London would feel ashamed of themselves if their patients were sick in the number of cases before they "went under" which Dr. Newman brought forward, namely, 30 per cent. That seemed an appalling amount of sickness. He always considered it the fault of the anæsthetist if the patient was sick during anæsthetisation, because it seemed to show that the patient was not properly under the influence of the substance, or he would not be sick.

Mr. E. A. STARLING said he would like to contribute a small and not particularly valuable quota of experience from the provinces with regard to the effect of the use of methylated chloroform and ether. Up till six years ago it was always his custom to use methylated chloroform and also methylated ether in administering anæsthetics. But since then he had used pure chloroform and pure ether, and the effects upon the patient, to his mind, had been that the sickness had been decidedly less under the influence of pure chlc-

roform or pure ether than under the influence of methylated chloroform and ether. Perhaps some other facts might have contributed to that difference in the results. For instance, in former days he always used an Ormsby's inhaler, but latterly he had used a Clover's, and therefore less ether had been employed. But even taking that into account, he might say he had had less sickness following anæsthesia when he used the pure drugs than when he employed the methylated products.

He would like to join issue with those gentlemen who had spoken of the vapour in using the A.C.E. mixture. It had always appeared to him that, the evaporation of ether taking place much more quickly than chloroform or alcohol, there would be the accumulation of chloroform or alcohol as well on the sponge or flannel, and the residue would be richer in chloroform at the end of the operation than at the beginning.

The PRESIDENT said that Professor Ramsay, in the course of his very lucid remarks, had not dealt with a matter which he (the President) thought was of considerable importance in the question under discussion, namely, the mode in which the anæsthetic was prepared. Probably every one present remembered that some years ago Professor Pictet, of Geneva, spoke very strongly in favour of his own method of purifying chloroform. Other methods of preparing chloroform had been introduced more recently, such as the preparation from acetone. It appeared to him that impurities in anæsthetics might enter during the process of manufacture, and be brought to them with the drugs. On the other hand, the drugs might be manufactured perfectly pure, but impurities might arise through decomposition occurring after the stopper had been removed, or, if air had been left in the bottle, even before the stopper of the containing bottle had been removed. But surely it was a very important matter for them to discern, whether any particular mode of preparation would insure them a pure drug at the outset, for they could safeguard themselves by the means which had been suggested by Professor Ramsay, against impurities arising as a result of decomposition; but few or none of them were able to insure an absolutely pure drug unless they could trust the *bona fides* of the man who sold it to them. There could be no question that the quality of the drug

in the market varied enormously. Some years ago he took the trouble to obtain samples from various makers, and on distilling them fractionally he found a considerable difference in the residue. Some of them, although supposed to be pure, gave an excessively impure residue. It must be remembered that when the Pictet process was prominently before the profession, Dr. Dubois Reymond, jun., came over to this country, and gave to the world the views of Pictet, and showed that all impurities of the chloroform remained in the residual liquids after the fractional crystallisation of the chloroform at -80° C. He demonstrated upon some cats that these residual liquids were very lethal, but that no danger arose when the chloroform was purified by crystallisation. However, after preparation a very large quantity of alcohol was added to maintain its purity. He (the President) believed experience now trended in the direction that such an elaborate process of preparation was not essential. He gathered from Professor Ramsay that that was his view also; they might hear later whether it was so or not.

One would also like to know whether the chloroform prepared from acetone—from the grey acetate of calcium—was better than that prepared from the other acetate of calcium, and whether the acetone or chloral chloroforms were superior to those prepared by the ordinary and older method.

Referring to the question of the production of carbonyl chloride in chloroform, he might say that in his own experience carbonyl chloride was a very definite thing that one had to contend against. He had never had a death from it; but independently of the death to which Dr. Probyn-Williams referred, several deaths had been recorded quite recently, though, strangely enough, he believed in none of those cases was it the patient who died, but one of the nurses or one of the doctors, probably showing that the air of the room became so impregnated with the chloroform that it was inhaled by them, whereas the chloroform inhaled by the patient coming directly from the lint, or inhaler, was purer. He remembered a case in which he was giving chloroform on a very foggy day, the result being that the air of the room became so insufferable that the surgeon and himself and an assistant were seized with violent coughing, and they had to throw the windows open; but as this only admitted more

fog, finally the operation was postponed, and finished on a future occasion. It was simply impossible to go on with it. He was bound to say that these difficulties could be avoided by not using the "best method"—according to Dr. Newman. If they gave chloroform in the dosimetric way—small quantities by the inhaler, with proper dilution of the air—there was not the amount of chloroform burnt in the air, and the same degree of irritation and cough was not produced as when the drug was evaporated in unnecessarily large quantities.

With regard to ether, he thought there was one very important question they must ask Professor Ramsay. He had already asked him—and he had answered him,—and he would give the Society the result of his (the President's) experience. The question he put to him was the following. He found that ether inhalers, even after careful washing and scalding in boiling water, gave an unpleasant odour, and he was anxious to know how it could be got rid of. The answer Professor Ramsay was kind enough to give him was that if he shook out the receiver with a little absolute alcohol, probably the compound would be got rid of. The result had proved the accuracy of the advice then given, for by this means one could get the inhaler absolutely sweet, and he therefore presumed it got rid of the oily compound which Professor Ramsay told them might be peroxide of ether.

In conclusion, he begged in the name of the Society to tell Professor Ramsay that they had received the greatest pleasure from his presence with them, and they had derived the greatest profit from hearing him. He had no doubt they would go forth to their daily work fortified with the remarks, feeling that they knew more about the chemistry of their subject than they had done before. The vote of thanks was both retrospective and prospective, because they hoped to hear Professor Ramsay's answers to the questions which had been put to him, and so further elucidate the subject which the paper had already rendered so lucid.

Professor RAMSAY, in reply, said he must ask permission to answer a question which had not been put to him, as to the best method of giving chloroform. On purely general grounds, without practical experience, it appeared to him that the

object of anæsthetists was to give a certain drug in known amount; and unless they could give it with full knowledge of its effect, the proceeding was a crude one. Definite knowledge in this matter must yield better results than trusting to chance or working with unknown quantities as in the "open method."

The quantities of mercury and slaked lime were not mentioned in his paper, because the quantities were so immaterial. Half a thimbleful of slaked lime would suffice for a large quantity of chloroform, because there was only a very small quantity of carbonyl chloride to be got rid of. So also with the mercury; the ethyl peroxide did not form when the ether was constantly being shaken up with mercury, therefore a thimbleful of mercury would be ample, and that might be transferred from one bottle to another, or the residue of one bottle might be transferred into another.

With regard to the test for chloroform, smell *versus* breathing, it was difficult to detect an impurity in chloroform by smell, but he always found a single inhalation showed it. The sense excited was not quite that of smell; it seemed to arise further down, namely, in the bronchi. It was very difficult to characterise these feelings in language, but if they were once experienced they would not be forgotten; the terms harsh and sweet were the best he could employ to convey his meaning. When he was working on the chloroform committee to which he had referred, he was the *corpus vile* of the experiments. He had a habit of going off very quietly and keeping his position to the very last, and he made a signal as to how long he could see a light, or whether he could hear any difference in musical tones. Various experiments were made on him, frequently until he lost consciousness. That got him into the habit of breathing anæsthetics without dislike.

A question was raised regarding the A.C.E. mixture poured on a cloth. The effect depended on how it was done. If the whole dose was poured on the cloth while the anæsthesia was kept up, and if a second dose was not required, the proportions would be those which Mr. Smiles had arrived at. But if the cloth was replenished, the original proportions would not hold good. What he desired to convey was that under no circumstances would pure chloroform be given; the ether would never wholly evaporate away, so that at the

very end there would be a mixture of chloroform and ether to administer.

The decomposition of chloroform was spoken of. Now Dr. Newman, who obtained his chloroform from one of the best firms, apparently had a considerable number of cases of sickness, and surprise was expressed that that should be so. He thought, however, from all the experience he had had, and he had had a great many samples of chloroform under his notice, that any good manufacturer delivered perfectly pure chloroform, that is except for a small proportion of alcohol which was added. But after chloroform had stood for a time there were other substances besides carbonyl chloride formed, which gave the residue. But as it came from the manufacturer the boiling-point was absolutely constant, and that was a safe test of the purity of a substance. If there was a choice between distilling and freezing, distillation as a means of purifying should always be selected. He had a case in point where they required absolutely pure acetic acid. The impurity in acetic acid is water. The water could only be thoroughly removed by distillation. Repeated distillation was, without doubt, the best way to purify.

Allusion had been made to the source of chloroform, such as acetone, alcohol, or methylated spirits. It did not matter in the least. The Germans had made it from chloral, and he thought it was the case that only that made from chloral was used in Germany. But he did not think it mattered.

Then as to the virtue of ether made from pure spirit and ether made from methylated spirit, it was difficult for him to answer with any positiveness. He would think, judging from the fact that the alcohols got more and more poisonous the more complex their formulæ—that is to say, they produce intoxication more quickly—that the methyl ether, the so-called impurity in the ordinary ether made from methylated spirits, would have a less injurious action than the ethyl ether. Methyl ether could be bought, and he would have thought it would make a valuable anæsthetic. He would think, speaking as an outsider, that methyl ether would give rise to fewer complications than ethyl ether. Methylene dichloride had been mentioned by some as an anæsthetic, but he presumed it was not much used now. He remembered examining a sample of

what was alleged to be methylene dichloride, and found no methylene dichloride in it; there were present alcohol and chloroform, and carbon tetrachloride.

He did not think it was in the least likely that carbonyl chloride would be formed in the lungs from chloroform. The process of forming it was a slow one, and there did not seem to be the necessary oxidising power in the lungs; moreover it required the action of light for its production.

As to whether the chloroform should be kept in clear or in coloured bottles, the chances of decomposition were very much less in a dark bottle, because without light chloroform did not decompose so readily as in the presence of light.

There was no combination between the constituents of the A.C.E. mixture; it was simply a mixture. There was some evolution of heat on mixing, but it was connected with some change which was not much understood.

The ether made from methylated spirit did not contain naphtha, for this reason; the Government were in the habit of permitting the methylated spirit used for the purpose of making ether to be sold with nothing but methyl and ethyl alcohols in it. Ordinary methylated spirits did contain naphtha, but University College and other similar institutions were allowed by the Revenue to have it in the form indicated. In the same way the manufacturers of chloroform got a permit from the Revenue Department which enabled them to use methylated spirits without naphtha for making ether.

He was asked what time it took to develop carbonyl chloride in colourless bottles. It was very difficult to answer that question; it depended upon the amount of direct sunlight it was exposed to. Diffused daylight did not cause nearly such a quick formation of the carbonyl chloride as direct sunlight.

Regarding the specific gravity of ether, he would be surprised to learn that that made very much difference. Ether of high specific gravity can only contain alcohol as an impurity, except that it may contain methylated ether. A low specific gravity was preferable to a high one.

In conclusion, he asked the Society to accept his thanks for their patience in listening to him, and for the way in which they had received his short paper.

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SOME POINTS IN THE CLINICAL STUDY OF ENTERIC FEVER.

A Clinical Lecture delivered at the London Hospital, December 1st, 1898,

By A. E. SANSOM, M.D., F.R.C.P.,

Physician and Lecturer on Clinical Medicine at the Hospital.

GENTLEMEN,—I propose to-day to say a few words to you on some points with reference to enteric fever. Of course to lecture on typhoid fever in an hour would be impossible; all I can do in the time will be to take certain points about it, and chiefly those upon which perhaps I have had a little more experience than some others.

First of all it will be useful to think over for practical use *some points with reference to the diagnosis of ordinary cases of typhoid fever*. And in order to do that I will give you an idea of the main features of two cases of ordinary typhoid fever, so that you may know what to look out for in ordinary cases of that disease. After that I want to say something about the cardiac and circulatory phenomena in typhoid fever, which I have had carefully under my observation.

The first case is that of John W—, æt. 19. I am going to imagine that such a case is brought to you in the country presenting certain signs. In the first place you ask as to the previous history. Of course you make an examination of the case as it stands, but one of the great guides that you have is the previous history. This patient was admitted on October 21st, and he is said to have been taken ill five days previously. He says that his illness commenced with headache, followed by pains all over him. The patient took to his bed on Friday the 21st, and was given gruel and brandy. Well, whether that does good or harm depends upon the proportion in which it is given; if there is more brandy than gruel it may do harm; if the brandy is insignificant in quantity it will do no harm. He went to work again on Saturday; I

take it that was the day after he went to bed, "the headache and pains all over being very severe." As he got up the next day I doubt whether the pains, apart from the headache, were very severe. The bowels were confined on the Friday, Saturday, and Sunday; but they were open on Monday after taking pills. Now that is the usual thing; pills are the panacea for all the woes that affect human nature, and to the out-patient of a hospital it does not always matter what pills they are. They do not like single pills, they take two or perhaps three, or perhaps four, because the itinerant vendors of pills that go about to markets and fairs somehow make people believe that the more of their pills they take the longer they will live; but it does not work out that way in practice. I think you can generally safely say that the more pills you take the sooner you will die, as a rule. At any rate, this man took his two pills. The bowels were open afterwards, and opened four times—that is to say, a little too much. Then constipation came on again, and the bowels had to be opened by an enema. He vomited three times, very likely as a result of taking the pills. That is the preliminary story. We will come to hard facts by-and-by.

The next case is that of Alfred R—, æt. 17. His illness began eleven days before he was admitted. It commenced with pain and throbbing on the top of the head—that is, headache, but a more detailed description of it. It was said to have been particularly severe after stooping or walking, and it continued every day until he came to the hospital. He also had pain on the left side of the abdomen; and he had a little dry cough, but there was no expectoration. He felt cold and "shivery," tired and languid towards evening. He had no appetite, but was very thirsty. He had been in bed a week before. And then for four days before he was admitted, namely from Saturday 22nd to 26th, he had diarrhœa. Before the diarrhœa his bowels had been regular.

Those, then, are the early symptoms in an ordinary case of typhoid fever. Now let us go *seriatim* over the points, just as we would if we had to write a paper on them, say with such a title as "The Ordinary On-coming Signs of a Straight-forward case of Typhoid Fever."

There is first (a) gradual onset, absence of initial rigor. Now think of how many acute

febrile diseases are declared by a shivering fit. Such occurs in a number of acute diseases. In children very often there is no rigor, but a sudden convulsion. But whether in childhood or in adult life, a sudden declaration has nothing to do with the ordinary diagnosis of typhoid fever. Things come on in a more gradual way. The point which you have most to think of is initial headache. Therefore we will add to our category—

(b) Headache. Now this headache in the ordinary individual who is out and about—that is to say, the ordinary person who constitutes the out-patient at a hospital—is not much regarded at first. He walks about with it, but he cannot understand why he should have a headache. Then he finds in the next place that he is weaker, he cannot do what he expected to do; he feels "seedy." That may go on for a long time, as long as a week. Then perhaps within a day or two of his headache there may come on—

(c) Diarrhœa. But remember that the diarrhœa can be complicated by pills. The first remedy taken is a pill, and that starts a looseness of the bowels which does not cease in many cases, while in others it does cease. After that the patient loses his appetite. Then there occur—

(d) Muscular weakness, cerebral signs (though these are generally slight), and deafness. The muscular weakness is very marked. In the early stages of typhoid fever the patient cannot understand why he is so weak; it seems an inexplicable thing. If you examine a patient then you will find that his muscles tremble, perhaps there is some subsultus, and they seem as if they had some influence enfeebling them. The patient may also be a "bit off his head,"—that is to say, he may have delirium, but the cerebral signs are not marked. He does not respond readily; he is dull and apathetic; and in some cases, particularly a little later, deafness becomes a marked sign.

Next comes (e) heightened temperature. Of course the man has no idea of his temperature; he feels a bit feverish, but even when he is most feverish he probably does not know much about it. As a matter of fact the temperature in typhoid fever generally rises at the onset a couple of degrees every evening, and falls about one degree every morning, therefore the chart peaks; the temperature is of a remittent type. Sometimes there are variations of that. The usual thing is

a rise in the evening, and a fall in the morning. But you must remember that the people who form our out-patients go on for a long time before they "cave in;" and because they walk about some time after the symptoms have commenced, we call them sometimes *ambulatory* cases of typhoid fever.

The next point is (*f*) cough and signs of bronchitis. In regard to that you of course should look at the man's throat, and in typhoid fever you do not see much in the throat to account for the symptoms. Such an examination will enable you to differentiate from tonsillitis, scarlet fever, and so on. Perhaps all you see in the throat in typhoid fever is a little dryness of the pharynx and a slight congestion, and there is a little toughish mucus about. If you auscultate you often find rhonchi and signs of bronchitis in the early stage.

Suppose that you are called to such a case as this, in which the symptoms are very gradual and the signs very obscure. Perhaps you have taken the temperature, but when you are called to the patient it is perhaps midday, when you may not find much deviation from the normal, because you may not be with him when he does deviate from the normal in that respect. You, of course, then lay him flat down and examine him generally, and particularly you look for—

(*g*) Spots on the abdomen; but the spots on the abdomen do not come out until about the sixth day of the illness. You may take it that the spots begin to come out from the sixth to twelfth day. I will not describe to you the characters of them minutely; you will learn them best by careful examination of cases in the wards. The little raised spots of typhoid fever are liable sometimes to be misunderstood, but you must not expect to see large spots; they are about as big as a pin's head, and it may be only as large as a very small pin's head. They may be very sparse, that is to say a few near to each other on the abdomen; perhaps a few near together on the chest. Sometimes you may see some on the back. Sometimes their colour is that of a pale rose, sometimes of a deeper rose. When you press them with the finger they pale down and disappear, and the colour comes back to them after the finger is removed; that is to say, they are not purpuric extravasations, or anything of that sort. These spots are very inconstant. There are a good many cases where you do not find a spot at all; in other cases they

come out in successive crops, and you may watch them from day to day. The next thing, as you have the abdomen exposed, is to feel for any enlargement of the spleen. Put your hand flat over the splenic area; do not press with the tips of your fingers, for by so doing you may excite the rectus and other muscles to contraction, which will prevent your feeling the organ so well. Put your whole hand on, and make steady pressure while you tell the patient to take a deep breath. Sometimes you may find a little bit of the lower part of the spleen presenting, as it were, under your finger. You say "I will trace that up by percussion," and you do so. Often there is a dull area which you can map out between the apex of the heart on the left side and the axilla. Now if you are asked what are the physical signs indicating the size and position of the normal spleen you will have a ready answer, namely that there are no signs; in the normal condition the spleen is not detectable by physical investigation. When it is enlarged, and you feel something which is doubtfully splenic, if you percuss over it and it is resonant you may say you are mistaken; but if it is dull to percussion, and you can trace it up over a certain area, you can say that is the spleen.

We now come to something extraordinary in the diagnosis of typhoid fever. It is a good thing for you to try every means of diagnosis, and I want you in all cases to try Ehrlich's diazo reaction. Do not let me be understood to say that is of absolute value, but it is of very considerable relative value. In typhoid and in some other diseases there is such a disturbance of metabolism that the urine contains some aromatic product that it does not contain in health, which gives a reaction with aniline solution; this constitutes Ehrlich's reagent. I need not go profoundly into the chemistry of it; first of all because I cannot, and I do not know anybody who can explain the whole of the changes which take place. It is (*a*) a solution of sulphanilic acid and 5 per cent. of hydrochloric acid; next (*b*) you have a solution of nitrite of sodium. You put a drachm of urine into a test-tube, and an equal quantity of the solution *a* and then five drops of *b*, the second solution. You shake together, and the mixture colours, and there is a froth; then you add to it some ammonia, and if you find that it becomes a very brilliant purple colour, and the

froth also is coloured, you say it is a positive reaction; if it does not become coloured, you say it is a negative reaction. If you have a suspected case of typhoid fever, and in the second week you get no reaction with Ehrlich's test, it is very unlikely that the case is one of typhoid fever at all. To arrive at that conclusion may be of very considerable importance. You may get it in measles, but not in German measles, and that is said to be one way of distinguishing the two forms of disease from one another. As I have said, in a doubtful case it is important to have every bit of evidence you can. But the foregoing is not so valuable a test as Widal's, which is a bacteriological test. That is founded on the fact that the bacilli of typhoid are motile, and they can be kept cultivated, so that there is a supply to draw from. Of course it is not easy to use this test, except in a properly equipped bacteriological establishment, but there are places to which you can send suspected blood to have the test applied. It consists in the following:—if you take a drop or two of blood of a suspected typhoid case, and mix it with a culture of typhoid bacilli, the blood of the typhoid case has got something in it which peculiarly acts on those typhoid bacilli, so that it stops their movement and makes them clump together, which they never do normally. If the blood of your patient produces that clumping together the chances that your patient has got typhoid fever are enormous. You may require all these tests to make perfectly sure that your case is one of genuine typhoid fever.

The next points I want to talk about, supposing we have gone over the ordinary diagnosis in a systematic manner, concern the consideration of *the circulatory phenomena met with in typhoid fever*. Of course I include the heart. What are these phenomena? If you were asked what is the effect of typhoid fever upon the circulation and upon the heart, you would say there is a great effect upon the arterial pulse in the sense of enfeeblement, even in the early stages of typhoid fever. The pulse at these stages is often what is called *dicrotic*. First of all the pulse is quickened; most febrile pulses are quickened; in some fevers the pulse seems cylindrical and fairly firm, whereas in typhoid fever it is by no means of that character. First of all you should feel the pulse by very slight pressure; then if you put a little extra pressure

upon it you will soon extinguish the pulse altogether, and that is the case where you roll it under the finger, or put your finger parallel with the artery. You can easily feel the *dicrotic* beat; it is the pulse of extremely low tension. Then sometimes it is irregular as well. Sometimes it is very rapid indeed, so much so that you can hardly count it. The pulse of low tension is a peculiar pulse, and if you could trace it with the *sphygmograph* you would find it has a very marked *dicrotic* wave; sometimes the *dicrotic* wave is as high as the other wave, and sometimes it may be as high and come down lower than the base line, in which case it is called *hyperdicrotic*.

Now let us look at the result upon the heart in typhoid fever. I daresay many of you who have been clinical clerks may have gone over cases of typhoid in considerable number, and have said that there are no signs connected with the heart in typhoid fever. If I were asked whether obvious cardiac signs were usual in the disease, I should say I do not think they are. But let us consider them more closely. First of all I will make a negative statement. I do not believe that *endocarditis* is ever produced by typhoid fever. I have never seen a case of *endocarditis* in which it could be concluded that typhoid fever had produced it. Typhoid fever can occur in a man who already has got *endocarditis*, that is to say, who has got a mitral or other murmur. Again, *septic endocarditis* or *infective endocarditis* may occur, though very rarely, in typhoid fever, but I do not think because of the typhoid fever. I think you may say at once that valvular disease of the heart never occurs from typhoid fever; certainly it is so according to my knowledge and belief. But certain things do occur in the heart, and when you examine a number of cases you will sometimes be somewhat puzzled. In the course of typhoid fever you may hear a very slight murmur at the apex of the heart or near it; you may say, "I did not hear any murmur before, but I hear one now." You may find that the murmur may increase, but when the patient gets well that murmur has gone. Sometimes you find other things besides murmurs. I had a case of a young lady of nineteen, a well-marked case of typhoid fever. On the tenth day there was no cardiac murmur. On the eleventh day there was a very soft murmur with the first sound, localised at the third left costal cartilage.

On the thirteenth day there was a bruit almost as far as the apex of the heart. It had all the characters of a blowing endocardial murmur. In fact, if I had heard it under other circumstances I should not have hesitated to ascribe it to valvular disease; it was well within the mitral area, but its maximum was a little to the right of the apex. On the fifteenth day it was heard as far as the apex, and the maximum loudness was at the third left intercostal cartilage. On the seventeenth day of the fever the bruit was only heard in the last-mentioned situation, namely, the third left costal cartilage, and there was a distinct reduplication of both the first and second sounds of the heart. On the twenty-first day there was reduplication of the first sound only, but the bruit was still audible as before. Convalescence went on most satisfactorily. In a few days all trace of reduplication had ceased; the basic murmur passed away, and slight anæmic murmurs were manifest over the arteries of the neck. On the thirty-eighth day the patient called at my house in perfect convalescence, and presenting no signs of cardiac disturbance whatever. There have been some other cases like that. Now what are the signs due to? As I have said, and as I still say, there is no evidence, to my mind, that there is anything like endocarditis in those cases. What we do know is that the muscle of the heart can and does undergo changes.

There is myocarditis—inflammation of the muscle of the heart. And it is a peculiar form of myocarditis; it is not evident all over the heart, but is noted to be in patches, and may occur in the papillary muscles which control the mitral curtains. The valve does not fit as perfectly as it should fit, and there is therefore produced a murmur which resembles that of mitral regurgitation, and accurately speaking *is* a murmur of mitral regurgitation. I might quote Shakespeare in this connection, and say, "Seems, madam? nay, it *is*." It is mitral regurgitation because the valve is incompetent; it is not because the orifice is dilated, it is simply because the muscles are not strong enough, as they used to be, to keep the valves well together, consequently the flaps do not close so perfectly as they did, and so there is enough regurgitation to make the noise which we hear. And by-and-by the case gets well; there is, then, no more morbid change in the muscle of the heart. We will call the disease, if you like, typhoid myocarditis. It is

not only in the heart muscle that changes occur. They occur also in the rectus abdominis, for instance. Some people call it Zenker's change, and it was described by French observers, notably Hayem. Remember that there is a characteristic enfeeblement in the early stage, and the heart must be very much enfeebled to produce the weak pulse which I have mentioned. So also is the muscular system weak. That this enfeeblement does occur is traceable by morbid anatomy. We find there is a morbid change which may get perfectly well; the voluntary muscles as well as those of the heart may quite recover themselves.

Now how shall you examine this case of typhoid fever now convalescing for circulatory conditions? If I ask you if you can feel anything which will locate the apex of the heart you will say No. And you say you cannot feel the right ventricle. What about the heart condition? Has the heart area increased? Is the heart enlarged or dilated? As far as I can find out, all over the sternum the sounds are absolutely unmodified. There is no evidence of any enlargement of the heart. But is it the exact contrary? Is it unusually small? I answer, Yes, it is. That is what I want you to note carefully as one of the great cardiac signs in the later stages of typhoid fever. To my mind that is one of the crucial signs in typhoid fever. Why is it small? The heart wastes because all the muscles waste. Wasting is a great feature in typhoid—look at this youth's muscles;—besides, we do not give them much food in typhoid which would enable them to put on flesh. I do not mean to say that the heart does not waste disproportionately to the other muscular structures in the body; I think it does. Now there are other diseases in which you get an attenuated heart; you get it in phthisis and in carcinoma very often, and the more rapid the wasting in carcinoma the more rapid the wasting of the heart. I put the decrease in the size of the heart in typhoid fever as a most important heart sign in the disease, and I do not think it is sufficiently recognised. Happily the disease leaves no trace of this reduced heart behind; in the course of weeks or months the tone and size of the heart muscle and other muscles are recovered. Of course I am only speaking of my own personal experience. I have told you that in the course of typhoid fever there is myocarditis in many cases; if we were quite

correct we should say in most cases, because probably that accounts for the extreme enfeeblement of the heart in the early stages of typhoid fever,—that is to say, that the poison of typhoid has such an intense action upon the heart and arteries that it depresses them in an extraordinary way; it inflames the heart tissue, and changes it very much indeed.

You will naturally ask, what effect have these changes upon recovery? What about the significance of them as regards prognosis? Well, I have read a good deal about that, and I have observed a good many cases. First of all I think it is best to take the experience of cases themselves, and then to hear what others say about it afterwards. I have seen these cases where there have been positive evidences, like that one I quoted to you, of implication of the heart muscles; I have seen a large number of cases of intense enfeeblement; I have seen some cases where there has been an almost constant delirium, as well as an exceedingly depressed typhoid state, and where the heart has been so feeble that I thought it impossible for the individual to recover; but the remarkable point is that I have never seen a fatal case yet; that is the fact which puzzles me. I have seen cases where there has been undoubted implication of the heart muscle by the typhoid poison, and yet the patient does not die. Yet I can give you no rules for prognosis. Another sign which I should have alluded to is that sometimes the heart assumes a foetal rhythm; there is a "tick-tack" sound like the beating of a foetal heart, both sounds being of equal length. I need hardly say that this means extreme enfeeblement of the heart's action. Yet I find French observers speak of sudden deaths in typhoid which they ascribed to myocarditis. I am not so sure of it. The sudden death is undoubted because we have their word for it, but I do not see that it is necessarily from myocarditis; I think it is most likely due to some effect on the central nervous system. At any rate my experience does not lead me to assign a gloomy prognostic import to cases of myocarditis. I invite you to observe this point carefully. Typhoid fever is a very strange disease, and the prognosis in a case of that disease is very difficult to give accurately. A great many years ago my old master, Dr. Todd of King's College, to whom I was clinical clerk and afterwards house physician, said one thing which has

always stuck to me, namely, "Never give up a case of typhoid fever, but remember its extreme uncertainty."

As regards never giving up a case of typhoid fever, I will give you an experience. I was called some years ago to see a young lady, the daughter of a clergyman who had been doing exceedingly good work in the parish. I saw this young girl with undoubted typhoid fever; she was delirious, with an enfeebled pulse, fluttering and irregular to a degree. The prognosis seemed extremely bad; we could not be sure she would live an hour, she certainly looked as if she would not. I gave a most unfavourable prognosis. I was called again some days afterwards, and she was worse, but she was alive. The next point was, she had profuse hæmaturia added to her other symptoms, and still she lived, though she looked as if the loss of a teaspoonful of blood would kill her, but it did not. I saw her a short time afterwards, and she was worse than ever. The whole of the integument over the sacrum had sloughed away, and the bone was perfectly bare; there were intestinal hæmorrhages, hæmaturia, and purpuric spots. She made an excellent recovery, and the last time I heard of her was that she was still doing good work in the parish. That sort of case completely floors one as regards prognosis. On the other hand, another case you will think is going on perfectly well, a case of ordinary severity; you may, perhaps, give a favourable prognosis, because you feel that all cause for anxiety has gone, but as a matter of fact there may be something as thin as a bit of tissue-paper between that patient and eternity, and the next you may hear of him is that he is dead. Just let us realise that fact; that the intestinal lesions of typhoid are not betrayed in their intensity by symptoms, and even if they were it would not count, because *one* ulcer excavating down and coming down too low may perforate, and kill the patient; or the ulcer may involve a vessel and produce fatal hæmorrhage. You see the dangers are so subtle. I show you a picture which will give you an idea of what the intestinal condition is, and how the irritative products affect the mesenteric glands. I do not propose to go minutely into the morbid anatomy now, because you may study that for yourselves downstairs.

There is no time for me to go into certain difficulties of diagnosis as I intended.

Dr. Fraser has been good enough to give me the notes of some very extraordinary cases where the signs seemed to be those of typhoid fever, and in which the diagnosis of typhoid fever was made, and where the ulcerations were all in the lower bowel. In typhoid fever the signs are chiefly in the lower part of the ileum. Although typhoid fever may involve the ileo-cæcal valve, it does not come into the colon as a rule. Here are notes of cases which began like typhoid fever, and there are certain other resemblances, but where the colon only was affected. The point arises, are we right in calling them cases of typhoid fever at all? I am inclined to think we are not justified in doing so; I think it is a form, of which I have seen other examples, of acute colitis, differing from dysentery, and differing from a chronic colitis. Acute colitis has certain resemblances to typhoid fever, and certain differences from it. There are sometimes spots, but they are doubtful spots. A great point is that there is diarrhoea, and generally there is pain, but not in all cases. There is hæmorrhage from the bowel, but it is not like the hæmorrhage of typhoid. In typhoid every stool is not tinged with blood, but it is tinged in every stool in acute colitis.

Another kind of case which presents considerable difficulty, but which I have not time to treat of adequately now, is influenza. I have seen cases of influenza and typhoid go together, and I have seen other cases where it has been a matter of extreme difficulty to make a diagnosis between influenza and typhoid. There is a gastric form of influenza, and it is between those cases and cases of typhoid that I have been in the greatest doubt about day after day. If you get a case with high fever, and you find that there are pulmonary signs, not the signs of bronchitis which I spoke of, but patchy broncho-pneumonia, and the illness begins with pains in the limbs and shivering, you may say that is a case of influenza, and so most probably it is. But the doctor will say in a few days, "I do not think it is influenza after all, I think it is typhoid." You say, "It begins with influenza, does it not?" And he answers, "Yes, but there are abdominal signs, and I see a few spots." Well, I have seen spots, and I have been very doubtful about those spots. I have also seen in some cases those spots cohere together in an intercostal space, and be of the nature or appearance of herpes

zoster. Or perhaps the doctor will say it is typhoid because there is hæmorrhage. But hæmorrhage from the bowel can occur in influenza in the form of purpura. Therefore even that is doubtful. Again, the chart of influenza can closely resemble the chart of typhoid fever. Further, diarrhoea occurs in the gastric form of influenza. All I have time to say now is that you must be careful in your diagnosis between typhoid fever and influenza, and to remember that you get influenza complicating typhoid fever. The difficulties are enormous; but recollect what I have said about the ordinary diagnosis of ordinary cases of typhoid fever. You will have to try and ascertain whether there is any extraordinary feature in the case, and then to try and find the reason for it.

A Bullet Embedded in the Heart for Thirty-seven Years.—A correspondent of the 'Baltimore Sun,' writing from Morgantown, W. Va., asserts that a man named William B. Smallridge, who died a few days ago at Glenville, in Gilmer County, carried a bullet in his heart for thirty-seven years. He was a member of Company E, 1st West Virginia Infantry, in the Civil War, and in September, 1861, while marching through Gilmer County, was shot by some one in ambush, the bullet entering Smallridge's chest at the lower point of the scapula on the left side, passing thence directly through the left lung into the left ventricle of the heart. The force of the bullet was so broken that it did not penetrate the inner wall, but the regimental surgeon pronounced the wound fatal, and left Smallridge to die. He did not die, however, but was sent back up the Little Kanawha River in a skiff to his home in Glenville, where he recovered and has since lived. A few weeks ago, while on his death-bed, he asked Dr. G. O. Brown to make an examination of the wound after his death. This Dr. Brown and Dr. O. B. Beer did, and found the bullet embedded in the heart. The man had never suffered from any disturbance of the heart. His death was due to cancer. This report is confirmed by Dr. Beer in a letter in the Cincinnati 'Lancet-Clinic' of November 19th, 1898.

Medical Record, December 3rd.

FRACTURES AND THEIR TREATMENT.

**With reference especially to Fractures of the
First Rib and Injuries of the Hip Joint.**

BY

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Children.

(Concluded from p. 133.)

I NEED hardly point out the inconvenience that must arise from the presence of a large mass of callus beneath the subclavian vessels and brachial plexus. Since surgeons were unaware of the existence of fracture of the first costal arch, if they found a bony tumour growing from the rib in this situation and producing symptoms of compression they probably regarded it as an exostosis, and I fancy I have read such cases described somewhere, but cannot remember the reference at the present moment. I can perhaps best illustrate this condition of the subclavian vessels and brachial plexus by a description of three typical cases which came under my observation.

The first was a middle-aged woman whom I was asked to see some years ago. She was supposed to be suffering from a traumatic aneurysm of the third part of the subclavian artery. The pulsating tumour appeared after a heavy fall on the shoulder sustained some little time before. She had used her arm freely since the injury. At first she complained of pain in the subclavian area, but this slowly subsided. Later she experienced some numbness in the arm and hand. An examination of the part showed that the artery was displaced forwards and upwards by a considerable elastic swelling beneath it, which was continuous with the rib. It was clear that the mass projecting from the surface of the rib was callus about a fracture. Placing the arm completely at rest resulted, after a time, in the progressive disappearance of the lump, the artery returning to its normal position.

The second case was that of a woman, A. B—, aged 37, who was recently under my care in Guy's Hospital. She was said to be suffering from an aneurysm of the left subclavian artery, associated

with considerable pressure changes in the nerves of the arm. The history she gave was that about eight weeks before admission, while exerting herself greatly at work which was very heavy and unaccustomed, she was seized with sudden pain in the neck, in the position of the present swelling, where she felt something give way. She continued to use her arm following her employment. A week after this she suffered from pain in the distribution of the median nerve in the hand. This spread up the front of the forearm and then up to the shoulder. The fingers began to swell, when she soon lost all power over them. An examination showed the presence of a hard mass beneath the subclavian artery and brachial plexus, displacing them forwards. No pulsation could be detected in the vessels of the arm. The arm was blue and cold. The fingers were flexed and rigid, and movements at the wrist, elbow, and shoulder were very limited. She suffered very great pain in the whole upper extremity. On October 12th, 1898, an incision was made along the upper border of the clavicle over the lump, which was exposed after carefully turning the subclavian artery and the posterior cord of the plexus, which was much broken up and compressed, off it. The mass of bone was clearly defined as a formation of callus about what had evidently been a fracture of the rib in the situation of the subclavian groove. It was necessary to divide the clavicle over the tumour in order to expose and remove it completely. This bone was reconstituted afterwards by means of a wire. Pulsation was soon detected in the vessels of the arm, the pain gradually subsided, and the muscles and joints are regaining their normal freedom of movement.

The third case was that of a labouring man who was employed in carrying very heavy loads on his shoulder. He received a blow on the shoulder which produced a sharp pain in the left subclavian region. He continued his laborious occupation, though he felt for a time what he considered to be sharp rheumatic pains in this region. These gradually subsided, but were soon followed by pain which extended down his arm to the hand. Later there was definite loss of power and wasting, which, with the pain, quite prevented him from following his laborious occupation.

An examination of the part revealed the presence of a hard, smooth, bony, and fibrous swelling, surrounding the left first rib just outside the artery,

and pressure on it increased the shooting pain down the arm. Rest of the arm in a sling was followed by some relief, but the pain returned whenever he resumed his occupation. In consequence of this he decided to have the mass of bone removed, and was admitted into Guy's Hospital for this purpose on October 31st of this year.

On cutting down on it I found it to be an ununited fracture of the rib. The opposing articular surfaces were considerably increased in area, and produced a large, prominent lump upon its upper surface. Splayed out over this were the subclavian artery and the cords of the brachial plexus. These were displaced off it, and hooked to one side, when the large bulk of the mass about the upper part of the fracture was removed by means of a gouge forceps. He made an uninterrupted recovery.

I have come across several other cases of fracture of the first rib in which there was clear evidence of compression of the superjacent structures by callus, but in none of them was it necessary to interfere surgically. These, like the first case, were recent fractures, and the callus disappeared with support of the arm and the consequent removal of the pressure of the clavicle from the subjacent costal arch. In cases 2 and 3 it was absolutely necessary to interfere actively, in case 2 because the muscles and joints were becoming fixed, while the pain was hardly endurable, and delaying in interfering would have resulted in permanent damage of the arm; in case 3 because the patient could not afford to rest, and as the operation showed, the fracture was ununited, and without operative measures the lump would exist permanently as a considerable prominence, and continue to render the man incapable of following his laborious occupation.

Before leaving this subject I will give you briefly one or two other interesting instances from among very many of the advantages which the surgeon can derive from the study of the mechanics of the attitudes of activity as shown to perfection in a fixed condition in the skeleton of the labourer. You remember that the development of the costo-clavicular and the coraco-clavicular articulations resulted from the habitual retention of these bones in certain relative positions, while a very great amount of force was being transmitted through them. It is clear that the mechanical gain from the point of view of economy

of expenditure of energy to the individual by the formation of these joints is very considerable. The chief function of a muscle is to retain bones in such a relationship to one another that force can be transmitted through them to the greatest mechanical advantage. The evolution of these new joints and the resulting limitation of the range of movements of these bones on one another, consequent on their development, save the expenditure of a large quantity of muscle energy which would otherwise be required to retain the bones in position. The same applies with equal force to the obliteration of joints from enormous pressure, as, for instance, in the ankylosed spine of the fully developed coalheaver. This explains the facility with which an aged labourer will perform an accustomed act which a young and stronger but untrained man is utterly incapable of doing. The great advantage accruing to the labourer from the fixation of a joint in an extreme position is also illustrated in a most interesting manner by the elbow-joint of the coal-trimmer.* While this man pursues his very laborious occupation the range of movement of the elbow-joints during the constantly repeated sequence is within a limited angle, which varies somewhat in the two sides. In the fully developed coal-trimmer this range, both in the directions of flexion and of extension, is controlled and rendered extreme at its working limits by the formation of dense bone on the floors of the coronoid and olecranon fossæ, and upon the ends of the corresponding bony processes. In this manner both the flexor and extensor muscles of the arm are relieved of the enormous expenditure of energy which would otherwise be required to control the working limits in the powerful and heavily loaded movements of flexion and extension which are being constantly performed by this labourer. On the other hand, the range of movement in a joint may be increased enormously beyond the normal, as in the arthrodial joint between the bodies of the fourth and fifth lumbar vertebræ of this labourer, the lamina and pedicles of the fourth lumbar being separated from the body during its development.

I could further illustrate my point by innumerable instances of labour changes, but I will now confine myself to only one more, namely, to a condition which exists in the hip-joints of all labourers who

* 'Journal of Anatomy and Physiology,' vol. xxi, p. 385.

carry heavy loads in such a manner that the trunk is at the time retained in a position of extreme flexion and slight adduction on the thigh bones. On picking up that very excellent text-book, Gray's Anatomy, I find the following stated deliberately and without reservation of any sort:—"Flexion of the hip-joint is arrested by the soft parts of the thigh and abdomen being brought into contact." Now, gentlemen, this very definite and precise statement is, except possibly in the case of obese people, absolutely without any foundation; in other

depended for his description on an examination of the movements of the head of the femur in the dead body, but has apparently assumed that when in the living body the thigh is flexed till it comes in contact with the abdominal wall this movement takes place only in the hip-joint. You may remember that a similar mistake was made in precisely the same manner when the anatomist described a rotation of the astragalus around a vertical axis, on the foot being fully extended by the flexor muscles.

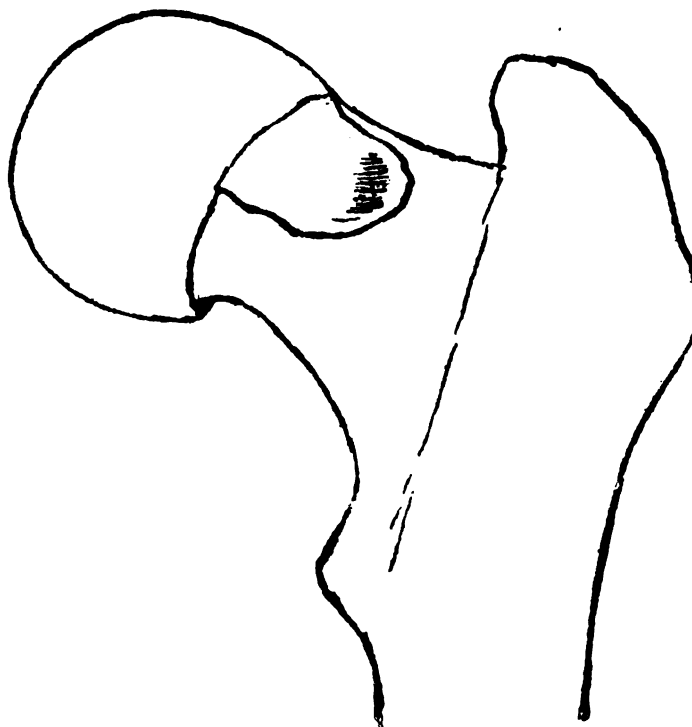


Fig. 5 represents the anterior surface of the femur of a man who has been engaged in carrying heavy loads upon his back. It shows an increase in the area of articular cartilage due to an extension outwards from its upper part on to the neck. Also the depression on the part of the neck which came habitually into contact with the margin of the acetabulum in extensive flexion and slight adduction. The upper surface of the head is on a lower level than normal, and is also flatter. On the under surface of the head an alteration described later is indicated diagrammatically.

words, it is utterly false, as you will see by examining this dissected specimen. This is only one instance out of many of the manner in which the authors of text-books on anatomy copy from generation to generation the carelessly made and incorrect observations of their predecessors on matters of the greatest importance without making any attempt whatever to verify their accuracy. How strong is this habit of mimicry amongst us! Is it merely because it saves trouble? The anatomist has not

If you examine the hip-joint in the dead body you see that the real range of movement between the limits of flexion and extension is small as compared to the apparent range which includes the movements of the lumbar, lumbo-sacral, and sacro-iliac joints.

The movement of extension is controlled by the resistance offered by the almost vertically placed fibres of the anterior ligaments of this joint. Flexion is limited by the impact of the upper and

anterior portion of the neck of the femur against the margin of the acetabulum, and any associated adduction limits the amount of flexion proportionately. A labourer, as the coalheaver, who carries any heavy load habitually on his back, walks with his hip-joints in a position of complete flexion, there being also a slight amount of adduction. The constant impact of the acetabular margin upon the same part of the neck of the femur, and the simultaneous transmission through it of great pressure result in the formation in this

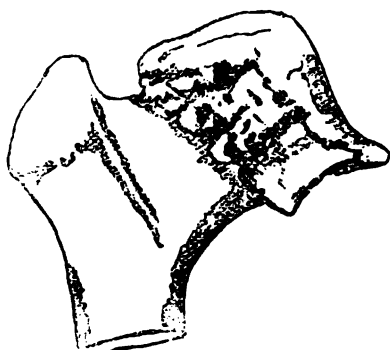


Fig. 6 shows the changes in the femur which result from the sudden transmission of excessive pressure through it in the manner described.

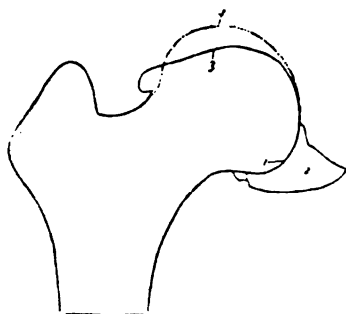


Fig. 7 represents a vertical transverse section through the specimen shown in Fig. 6.

part of the femur of an articular facet, which is covered by a thin layer of cartilage (see Fig. 3). The exact seat of this facet varies with the character of the load, and with the manner in which it is supported on the trunk. There is a corresponding surface in the innominate bone, though it is less defined. The advantage to this labourer of being able to secure the pelvis on the femur is clearly very great, since it removes the necessity of the muscles balancing these bones on one another while the heavy load is being carried, which would

otherwise be required. If the floor of the acetabulum be removed, and the ligamentum teres be exposed, you see that it is most tense in this position of flexion and slight adduction of the hip-joint. In extreme flexion the femur, and indeed the whole leg, performs the function of a lever of the first order, the fulcrum corresponding to the point of impact of the neck of the femur against the acetabular margin as represented by the facet described, and the short arm to the interval between it and the extremity of the head of this bone.

The head of the bone is retained in position



Fig. 8.

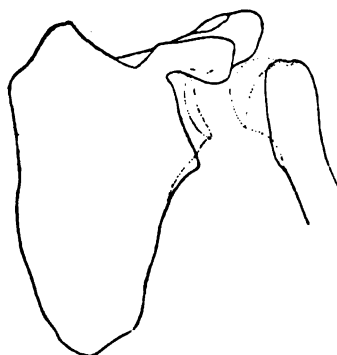


Fig. 9.

Figs. 8 and 9 show the changes which take place in the radio-humeral and shoulder joints respectively in "traumatic arthritis," in consequence of the severe bruising of opposing articular surfaces in subjects past middle age.

partly by atmospheric pressure, partly by the resistance offered by the ligamentum teres, and partly by that afforded by the various muscles and fasciæ which surround the upper posterior and lower aspects of the joint.

In the coalheaver and other labourers who carry in a similar manner, the ligamentum teres is enormously strong, presumably on account of the great strain to which it is habitually exposed. There is, I think, no doubt that the function of this ligament is to control extreme flexion, and the

slight adduction which is associated with it in locomotion in this position in the manner I have just described. That other views are held about it the following quotation shows:*

"The security of the joint may be also provided for by the two bones being directly united through the ligamentum teres; but it is doubtful whether this so-called ligament can have much influence upon the mechanism of the joint."

How can we apply the knowledge that we have obtained by the study of the fixation of the position of extreme flexion of the hip-joint in the body of the labourer? By it we learn that the immense leverage action exerted on a single occasion by the lower extremity in a position of extreme flexion and some adduction can produce a rupture of the ligamentum teres, and the displacement of the head of the femur out of the acetabulum,—in other words, the first stage in all dislocations of the hip-joint is effected in this manner. If the force be exerted gradually and over a long period of time, as in the uterus, the head of the femur is forced out of the acetabulum, and the ligamentum teres gradually yields and elongates, becoming also thicker at the same time because of the strain to which it is exposed. The head of the femur escaping from the acetabular cavity forms so-called "congenital dislocation of the hip-joint." During locomotion the hypertrophied round ligament assists very materially in transmitting the weight of the trunk to the extremity of the femur. If in the case of the feeble old subject or in the growing child force be applied suddenly through the long arm of the lever in the same extreme position of this joint, the ligamentum teres and the atmospheric pressure are more resistant than the lever itself, which yields in the situation of the fulcrum, a fracture of the neck resulting. This in the young subject corresponds to the junction of the neck of the bone with the epiphysal line, and is spoken of as a "separation of the epiphysis," and in those advanced in years the term "intra-capsular fracture of the neck" is usually applied to it.

Returning to the hip-joint of the labourer, the habitual transmission of an enormous pressure through it while retained in a position of extreme flexion and very slight adduction results also in a

removal of the upper surface of the convexity of the head of the femur which is thus rendered less convex than before, while the opposing articular surfaces through which the force is transmitted are increased in area to a corresponding extent. This is increased still further by a formation of bone and articular cartilage in the immediately adjacent portion of the neck, the head altering in form considerably from the normal. If, as in some labourers, the load carried is still greater, the articular cartilage over this area is removed after a time, and highly polished, dense, eburnated surfaces of bone replace the original articular covering. In either case the upper surface of the head of the femur comes to occupy a lower level as compared to the trochanter. As there is associated with the removal of the upper part of the head a compensatory deposit of bone on its under surface, the angle formed by the head and neck with the shaft in the fully developed labourer is decidedly smaller than it is in the subject who has not followed a laborious existence.

Mechanical changes on precisely similar lines take place very rapidly when these opposing surfaces are exposed to enormous pressure on a single occasion. In such a case the articular cartilage and subjacent bone are removed over this area, no attempt being made to restore the covering of articular cartilage, and abundance of bone is deposited below. I am of opinion that it was the alteration in the form of the bone which results from labour that induced the earlier observers to imagine that the neck of the femur yielded in old age. I may remind you that the pathologist was also led to regard the change in the head produced traumatically in the manner I have last described as being brought about by "interstitial absorption of the neck of the femur." Figs. 4 and 5 represent this condition of "mechanical or traumatic arthritis," as I have called it, in the head of the femur, and Figs. 6 and 7 precisely similar changes in the radio-humeral and shoulder joints. I think that I was able to satisfy myself from a careful examination of the bodies of infirm and aged people who had not followed a laborious existence that, owing to their transmitting their normal pressure through joints retained habitually in a position of considerable flexion with some adduction, a change similar to that to which I have called your attention in the labourer ensues in the

* 'Gray's Anatomy,' p. 361, line 24.

very feeble and ill-nourished bones. That it takes place in other joints in the body I have shown beyond dispute. For further description and illustrations of these changes I would refer you to a paper "On Senile Changes" in the 'Transactions of the Pathological Society' for 1886. In considering the variation in the angle of the femur in old age I am leaving out of consideration the yielding of the neck or of the other bones that results in the condition of imperfect nutrition, called senile mollities ossium.

I ought perhaps to apologise for inflicting on you these details, which may at first sight appear to be solely of anatomical interest; but I feel sure that a little consideration will convince you that it is only by the careful observation and recognition of the general laws that govern these changes that we can hope to learn anything of the functions and mechanics of bones and joints, and to base our surgical procedures on some sound scientific foundation, which is at present singularly conspicuous by its absence.

Indications for Laparotomy in Contusions of the Abdomen.—Speaking on a paper on "Surgical Intervention in Abdominal Contusions," presented by M. Potherat to the Société de Chirurgie, M. Hartmann ('Journal des Praticiens,' October 29th) said that after contusions and wounds of the abdomen the contraction of the muscles of the abdominal wall indicated certainly a visceral lesion, and consequently demanded laparotomy. This symptom had no doubt been noted by other observers, but the necessary operative indications had not been drawn from it. Out of ten cases of serious contusion it had been present seven times; and acting upon this indication, surgical intervention had been made, with the result that in each case grave visceral lesions had been found. M. Delorme, however, attached no importance to this symptom. He had observed it many times in cases of grave abdominal contusion occasioned by the kick of a horse, without operating, and his patients had recovered. He, like his colleagues, recognised the necessity of emergency laparotomy in these cases, but only when there existed a pathognomonic group of visceral lesions pointing to intestinal complication.

N. Y. Med. Journ.

THREE CASES OF HERNIA PRESENTING UNUSUAL CONDITIONS.

BY

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CASE 1.—Right Inguinal Hernia complicated with Right Irreducible Femoral Hernia: contents of Femoral Sac the Vermiform Appendix.

Edith H—, æt. 21, was admitted under my care at Westminster Hospital in March, 1897, suffering from reducible right inguinal hernia and irreducible right femoral hernia.

The history of the case was as follows:—For some years the patient had been the subject of right reducible inguinal hernia, and had worn a truss for the condition; at times the hernia had come down, been painful, and reduced with difficulty.

A few days before admission into the hospital the rupture had come down suddenly, and the patient was unable to reduce it. Great pain, nausea, and constipation followed. Dr. Durno, of Loughborough Junction, was called in, and found the hernia strangulated. He reduced the rupture by taxis, the intestine going back with a gurgle. He had some difficulty in reducing the gut. After reduction he discovered a lump in the right femoral region, the nature of which he was not quite certain of, though he took it to be in all probability an irreducible femoral hernia.

The next day the patient was much better, and in the course of about a week was sent up to me for radical cure.

On March 19th I cut down on the site of the femoral hernia, which was irreducible, and appeared to contain omentum.

On opening the sac some adherent omentum was found, and on carefully unravelling it a process about two inches long was discovered, which process was intestinal, and taken by me to be the vermiform appendix. On further examination I was able to make out the connection of the process with the cæcum. The vermiform appendix seemed healthy in appearance, and after separating it from one or two adhesions which bound it to the

omentum, it was carefully cleansed and returned to the abdomen. The omentum and sac were then ligatured separately and excised. I finally dissected up a flap of pectineus muscle and fascia, and stitched it to the edges of the saphenous opening and Poupart's ligament, the opening of the femoral canal being thus closed. By extension of the skin incision upwards I was able to deal with the inguinal hernia; a well-formed and thickened sac was found, but it was empty. I excised the sac, closed the canal with kangaroo tendon sutures, and the skin incision with carbolic silk and horsehair sutures. Seven days later the dressings were removed, and the wound found to have healed by first intention.

The interesting points about this case were—

1. The presence in the first instance of two irreducible herniæ on the same side, with symptoms of strangulation.

Dr. Durno recognised the strangulation, and dealt with it successfully; he also diagnosed the presence of a second swelling in the femoral region, which he took to be an irreducible femoral epiplocele. In such a case as this, symptoms of strangulation being present, it might have been difficult to determine which of the two herniæ was the seat of constriction. The history, however, made this point clear: the patient had suffered from reducible inguinal hernia for some years; suddenly the rupture came down and became irreducible. This, together with the amount of tension and pain about the neck of the inguinal hernia, constituted strong points in favour of its being the seat of strangulation in contradistinction to the femoral hernia.

If after the reduction by taxis, as was practised by Dr. Durno, symptoms of strangulation had persisted, it would obviously have been necessary to cut down and explore the femoral region.

2. The presence of the vermiform appendix in the sac of the femoral hernia.

The appendix vermiformis has been found occasionally as the sole occupant of an inguinal or femoral sac on the right side. It may become considerably enlarged, adherent to the sac, and inflamed. In my case it was adherent by adventitious bands to the omentum, but the process being healthy in appearance I returned it to the abdominal cavity. If it had been diseased the wiser course would have been to remove it.

CASE 2.—*Radical Cure of Inguinal Hernia: Undeveloped Testis with well-marked Vas Deferens.*

John C—, æt. 24, was admitted under me in September, 1897, with a well-marked reducible inguinal hernia of the left side. The left half of the scrotum was undeveloped, and the testis absent from the scrotum and inguinal canal of that side.

The patient complained of testicular pain when the bottom of the hernial sac was compressed, but no body approaching the feel of a testis could be made out by myself.

The hernia was cut down upon in the usual way, the sac exposed and carefully opened. It contained a small quantity of healthy omentum, which I returned. Careful search was then made for any "vestige" of testis, but none was found. The sac was then separated in the usual way, and whilst freeing the sac I came across a well-developed vas deferens, spermatic artery, and pampiniform plexus of veins, all of which structures were traceable to the bottom of the hernial sac. No testis, however, could be made out. The whole of the sac, cord, &c., was ligatured as high up as possible with kangaroo tendon, and excised. The inguinal canal was then firmly closed with kangaroo tendon, and the skin wound closed with carbolic silk sutures. The further progress of the case was quite uneventful.

The interest of this case seems to me to have been the condition of the spermatic cord and the total absence of the testis.

The patient when examined and the hernial swelling squeezed, curiously enough, stated that he had slight testicular sensation, and it was principally on account of attacks of apparently testicular pain on that side that he desired operation.

Yet though a well-formed cord was found, there was no testis to be seen or felt. A microscopic examination of the tissues at the bottom of the sac was made, but no testicular elements were discoverable. The section made showed merely fibrous tissue and a fair amount of blood-vessels. The vas deferens, up to within an inch of its termination, was patent. There was no epididymis. According to Henry Morris, either the testicle as a whole, together with the epididymis and vas deferens, may be absent, or the testis proper may be absent, the epididymis and vas deferens being present, or the two latter may be absent and the

testis proper be present. He explains these congenital deficiencies of part only of the testicular apparatus by the fact that the Wolffian body and its duct give rise to the vasa efferentia, epididymis, and vas deferens, whilst the testis is developed separately from the genital mass.

In my own case I think it possible that there had been a more or less ill-developed and retained testis in the canal, and that it had eventually degenerated into a very small mass of fibrous tissue.

This degeneration included that of the vasa efferentia and epididymis.

CASE 3.—*Strangulated Interstitial Hernia: Myxomatous Condition of Testis and Cord.*

George R—, aged 28, was admitted into the Westminster Hospital October 18th, 1897, with a swelling in the right inguino-scrotal region. The history was that the patient had had a reducible right inguinal hernia for some years; about six hours before admission into the Westminster Hospital the rupture suddenly came down, and the patient was unable to reduce it; he had a considerable amount of acute local pain and a feeling of nausea, but no other general symptoms.

The house surgeon attempted gentle taxis, but failing to reduce the hernia sent for me.

I found on examination a large swelling occupying the inguinal region of the right side, and extending down into the right scrotum. The inguinal swelling extended further out towards the anterior superior spine of the ilium than usual, and was resonant on percussion. The scrotal swelling was dull on percussion, and felt doughy. There was no impulse on coughing. The testis appeared enlarged, but in its normal position below. I took the case to be a strangulated entero-epiplocele, and decided to operate at once.

The man having been anæsthetised, I cut down on the inguinal canal and exposed the sac of the hernia. This I carefully opened. I then found that the sac not only extended down into the upper part of the scrotum, but also up between the internal and external oblique muscles of the abdomen (interstitial hernia), the contents of the double sac consisting of small intestine, a loop of which had become nipped and congested at the internal abdominal ring.

I divided the stricture, reduced the gut, and then

separated both the interstitial and scrotal portions of the sac, ligatured with carbolised silk, and removed them. I then found that the swelling which occupied the rest of the scrotum consisted of a curious jelly-like tumour of the testis and cord. I cut into this tumour, and finding it to be solid, and that the testis was involved in the growth, decided that the best procedure was to remove the testis and cord, which I did in the usual way. The operation was completed by doing the ordinary radical cure.

The patient made an uninterrupted recovery.

The interesting points about this case were—

1. *The variety of sac found.*—In this particular case of mine the condition found was the commonest of the three varieties of interstitial hernia described in the text-books, though any of these forms is rare. The peculiarity of this kind of sac is that besides the usual sac of a hernia there is an additional diverticulum which extends between the layers of the abdominal parietes, the two sacs having a common opening into the abdominal cavity. One of three conditions is found: (1) The additional sac is between the fascia lining the abdomen and the parietal peritoneum (very rare). (2) Between the internal and external oblique muscles (the commonest form). (3) Between the external oblique muscle and the skin.

The second and third varieties are called intra-parietal herniæ, and Mr. Macready has collected 163 cases from the records of the Truss Society. Out of the 163 cases 129 were males and 34 females; in both sexes it is twice as common on the right side as on the left.

In the majority of cases that have been described a well-marked ventro-inguinal swelling, the greater part of which consists of the additional sac, has been seen.

In the male it is usual to find some abnormality of the testis; of the 129 cases published by Mr Macready some abnormality of the testis existed in 73·4 per cent., and in 67·1 per cent. the testis was completely retained, or had only partially descended.

According to Mr. Treves, the mode of production of the interstitial hernia may be—(a) owing to some congenital abnormality in addition to that usually evident in the arrested testis and open processus vaginalis; (b) owing to a narrow ex-

ternal ring, the bowel extending in the direction of least resistance; (c) the production of the adventitious sac from persistent and ill-directed attempts at reduction. In my case there was a full-sized testis in the scrotum and no narrowing of the external ring. The external oblique muscle did not appear to be in any way abnormal or atrophied.

(2) *The presence of a tumour of the testis and cord.*—The result of the presence of this myxomatous-looking tumour and of the inguinal swelling was to make the whole condition appear that of a large inguino-scrotal hernia, and it was not until I had cut down and thoroughly examined the parts that I was able to make out the condition of affairs present.

At first I thought the condition of testis and cord might be due to general œdematous infiltration from pressure, but on cutting into the tissue it was quite solid, and infiltrated the sac and testis.

It is also interesting to note that there were but few of the active symptoms of strangulation present, though there was a loop of intestine fairly tightly nipped at the common opening of the two sacs.

This is of course not uncommon, but it emphasises the fact that the earlier one operates in cases where the symptoms of strangulation are only slight, the better for the patient.

THE MODE OF SPREADING OF BUBONIC PEST.

P. L. SIMOND summarises a careful research concerning the mode of spreading of bubonic pest in the following important conclusions:—The study shows that the pest is spread by two principal factors—the rat and man. Man is the ordinary agent of transport for long distances, which could not be attained by the rat; the rat is the carrier from house to house.

When pestiferous rats enter a new district there is usually some delay before the disease appears in epidemic form; the arrival of infected human patients into a new territory is not always followed by epidemics of pest. In order that epidemics may be established there must be a circle of favorable circumstances, among which the preliminary

transmission of the virus to rats seems to be the most important. The period of incubation represents the time necessary for the development of the disease among rats.

The gravity of a human epidemic stands in close relation with that of the epidemic among rats. The progress of the disease throughout the city follows the route adopted by the migrating rat. Even when the disease is no longer associated with a great mortality among the rats it may continue to exist in a benign form, and the so-called sporadic human cases, which manifest themselves after the subsidence of an epidemic, may be attributed to this benign form in the rodent.

A second epidemic of the disease usually manifests itself about twelve months after the appearance of the first, from which it is separated by a longer or shorter period of calm. The reason of this periodicity in the epidemics is not determined. It is undoubtedly connected with the re-establishment of the disease in epidemic form among the rats, and depends in part upon the repopulation of the city involved by these animals. Careful experiments have shown that there is but little danger in the spread of the disease from infected organs or food.

In a certain proportion of cases the point of entry of the microbe is marked by local reaction, which is generally situated in a region where the skin is thin and delicate.

The notion that the transmission of the disease depends upon larger parasites is in accordance with the observations made on rats; a few hours after a rat dies from the disease the fleas abandon the cadaver in order to attach themselves to other animals and to human beings. The points here referred to are strengthened (1) by the presence of the specific micro-organism in the contents of the intestines of the fleas which have absorbed infected blood; (2) by certain facts in connection with the transmission of the disease from the rat to man and from man to rat. In the last instance it is possible also that other parasites of the same class as the flea may act as carriers. (3) By the possibility of the transmission of the pest to a healthy rat by cohabitation with a rat covered with pestiferous fleas, whereas cohabitation with a rat sick of the disease, but deprived of fleas, is constantly harmless.

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THE INDICATIONS FOR OPERATION IN CASES OF ADENOID VEGETATIONS OF THE NASOPHARYNX.*

BY

JAMES B. BALL, M.D.,

Physician to the Throat Department, West London Hospital.

THE question whether a child affected with adenoid vegetations should be operated upon or not is one which a medical man is often called on to decide at the present day. The regular medical attendant on the family has generally to decide the question. In certain cases he may refer the point to some one in a more special line of practice, but in the majority of cases it is the family physician who has the decision in his hands. It therefore seemed to me that the indications for the operation might be a fit subject for discussion, especially as one has heard of late the most opposite opinions expressed on this subject. Statements have been made in some quarters which seem to mean that practically no case of adenoids need be operated on; and, on the other hand, the opinion has been freely expressed by some specialists that every case of adenoids without exception should be immediately operated on. I do not intend especially to direct my observations against either of these extreme views. They both appear to me to be equally absurd. Many of us were in practice long before the operation came into general use; and when we consider how very common some degree of the affection is, it is perfectly clear that many cases, if not the great majority, were none the worse for being left alone. On the other hand, no one who has witnessed the enormous benefit derived from the operation in certain cases can doubt the advisability of operating in such cases.

I assume, therefore, that we all agree that some

* Read at the Chelsea Clinical Society, Nov. 15th, 1898.

cases of adenoids should be operated upon, and that some do not need operation; and what I propose to do is to briefly inquire into the conditions under which we should feel bound to say that a particular patient, admittedly suffering from adenoids, ought to be operated upon, and when we should feel bound to say that a case does not require operation. I shall keep my remarks within the limits of the subject proposed, and I shall not enter into any detailed description of the disease, or into the diagnosis or methods of operating. I think in approaching the subject I cannot do better than in the first place briefly recapitulate the main troubles which have been traced to the disease, at the same time suggesting how far each of them may contribute indications for operation. I shall then refer to some general considerations bearing on the question of operation, and finally I shall enumerate the symptoms and the rules which influence me in coming to a decision in favour of operation.

There is no need for me to enter into any explanation of the nature of the disease. It is now well understood by all that it consists in hypertrophy of the collection of adenoid tissue in the vault of the naso-pharynx, known as the pharyngeal tonsil, or Luschka's tonsil. This tonsil is always distinctly developed in childhood, and it undergoes atrophy as adolescence approaches, and even the hypertrophied structure or adenoids usually diminishes or disappears about the same period, although there are many exceptions to this rule. It will be easily understood that the limits between a normal and an enlarged pharyngeal tonsil, between a physiological and a pathological condition, are not easily defined, the more so as our means of examining this tonsil and estimating its size are not so easy and direct as in the case of the faucial tonsils. I am not, however, dealing with these borderland cases, in which there may be a doubt whether adenoids are present or not, but with cases in which there is admittedly some enlargement of the pharyngeal tonsil, and with the question of operation in these cases.

The respiratory trouble may be mentioned first. The most direct result is interference with nasal respiration. Mouth-breathing in various degrees may take the place of normal nasal respiration. The mouth-breathing may be intermittent or constant; it may be present both waking and sleeping, or chiefly in sleep. Many of these mouth-breath-

ing children breathe noisily at all times, but especially when eating or drinking. Infants may be quite unable to suck. In sleep the breathing is especially laboured. Noisy breathing and snoring are frequent symptoms. Sleep is often very disturbed in these children; they toss about, moan, talk, and are often subject to night terrors. Sometimes in young children the breathing is arrested altogether for some moments, and they start up, partly suffocated and struggling for breath. One cannot doubt that many of these children, owing to their nasal obstruction, are labouring under imperfect oxygenation of blood. Chest deformity sometimes develops from the impeded respiration in young children. I may also remind you of the deformity of the upper jaw, so often associated with chronic nasal obstruction in early life, although the various explanations of the way in which it arises do not seem to me satisfactory. The deleterious effects of chronic nasal obstruction, especially in children, are generally admitted; and it must be remembered that persistent mouth-breathing in childhood will from various causes continue into later life, even after the adenoid growths have completely disappeared. It is obvious, therefore, that chronic nasal obstruction may constitute an indication for removal of adenoid growth.

Many children with adenoids are very subject to bronchial catarrh, and undoubtedly operation will often cure this tendency. Asthmatic symptoms occurring in a subject with adenoids are also benefited by removal of the growths. Cough, independent of any bronchial complication, is not an infrequent symptom. It is a short, dry, hacking or barking cough, sometimes constant during waking hours, sometimes noticed chiefly when the child lies down at night, or waking the child up after an hour or two. This cough will often disappear suddenly of itself or under general treatment, change of air, &c.; but it may be very persistent, and be in itself an indication for operation. Frequently recurring colds in the head, often very prolonged, followed or not by cough and bronchial symptoms, are often the result of adenoids. So is a chronic nasal catarrh with a profuse mucoid or muco-purulent secretion, or a chronic purulent rhinitis. These symptoms, if obstinate to other treatment, may indicate an operation in an otherwise slight case. An occasional symptom, even in a slight case, is a habit of constantly making a

snuffing or snorting noise. One can well understand, when this has been going on a long time, that an operation might be sought to cure it, and a cure may be safely promised. Paroxysmal sneezing and hay fever symptoms are often benefited by removal of adenoids.

The next group of symptoms to which I shall allude is of great importance, viz. ear troubles. They are common and undoubted results of the disease, and occur in a greater or less degree in a good proportion of cases. Deafness, chronic or frequently recurring, repeated attacks of earache, otorrhœa—all these are among the symptoms most strikingly benefited by removal of adenoid vegetations. It is also certain that quite a moderate amount of adenoid growths, unattended with any other marked symptoms, may cause these ear troubles.

It may be convenient here to allude to another symptom which is sometimes associated with adenoids—namely, dulness of intellect. The child is backward for its years, and rather stupid at its lessons. This is a point on which a good deal of nonsense has been talked. To hear some people, one might suppose that this condition did not occur independently of adenoids. Now there is no denying that these mouth-breathing children, especially if the characteristic facial aspect of this condition is developed, do look stupid; and if a child happens to be stupid, it is only natural to conclude that the condition which gives an air of stupidity is the cause of the stupidity. But this is not necessarily so. It is also easy to satisfy one's self by inquiry that these children are generally not so dull as they look, and many of them are clever, forward children. Of course, if they are deaf they look still more stupid, and owing to their deafness they are probably backward in their studies. I am not inclined to deny that marked long-continued nasal obstruction does sometimes make a child, or indeed an adult, dull, but in these cases there are sufficient indications for removal of the adenoids without laying too much stress on the mental condition, or promising too much in this respect from operation. Sometimes in bad cases decided improvement in this respect follows, and of course if deafness has been present the child's progress will be greatly improved. This is as far as I can go. To look for adenoids because a child, presenting no other symptoms of the disease, is

rather stupid, and to remove a slightly hypertrophied pharyngeal tonsil on the promise that the child will improve mentally, is in my opinion simple quackery.

Another set of troubles, which need not detain us long, are those connected with voice and speech, the stuffy dead voice, the inability to sound certain consonants, and backwardness in learning to articulate clearly. These troubles when they result from adenoids occur in cases otherwise well marked, with other indications for operation. There is evidence that stammering is common in children with adenoids. In certain cases of stammering, in which I have removed adenoids, no improvement that I could trace to the operation followed, but I do not wish to speak too dogmatically on this point.

Chronic and persistent enlargement of the glands of the neck seems to be a generally accepted reason for examining the condition of the nasopharynx, and removing an otherwise apparently harmless mass of adenoids, so I need say no more on this point.

The general health and nutrition of the patient have next to be considered. The general condition undoubtedly often suffers in marked cases, from various causes, though by no means in all. The general condition may turn the scale towards operation in an otherwise doubtful case. That is all. There is no evidence that a moderate amount of adenoids giving rise to no other troublesome symptoms does injure the general health, and for a doctor to promise benefit from removal of a trivial mass of these growths is a thing which he has no right to do, although it is done every day.

There are other occasional symptoms and effects of adenoids besides those I have alluded to, and still more that have been rightly or wrongly, often wrongly, I believe, attributed to them. I need not allude to these. I have suggested the chief symptoms upon which our decision as to operation will be based. I shall now make a few general remarks bearing on this subject.

In the first place I think the duration and persistency of the symptoms should always be considered. Undoubtedly the pharyngeal tonsil swells up temporarily, without apparent cause, or after a cold, or one of the infectious maladies, but in the course of a few weeks or months under general treatment, change of air, &c., the symptoms sub-

side. Do not let us therefore rush into operations. The possibility, too, of other treatment, such as cod-liver oil and sea air, benefiting a patient should not be forgotten. I ought here, perhaps, to allude to the practice of systematically teaching children who are sufficiently old for the purpose to practise breathing through the nose. It has been stated that this may do harm. It can do no harm whatever in the class of cases for which alone I should recommend it, that is in cases of moderate nasal obstruction. It must be remembered that what we mean clinically by chronic nasal obstruction is a degree of impediment to nasal breathing, sufficient to develop the mouth-breathing habit in greater or less degree. Now, many children with a moderate amount of adenoids can breathe very comfortably through the nose, when their attention is called to it, and in these cases I am convinced the practice does good. In really bad and old-standing cases, which require operation, I think it is loss of time, and often dangerous loss of time; but when the symptoms are moderate, or recent, it often does good, and in some cases it is desirable even after operation.

With regard to the age of the patient, I would say that once it is decided on other grounds to operate, the sooner the better, no matter how young the patient is, but in some doubtful cases the age itself may become a factor in coming to a decision. Thus in a patient with moderate symptoms who is approaching the age at which spontaneous diminution of the growth may be expected to take place, the age might influence us in deciding against operation, whereas in a young child of three or four with moderate symptoms of some duration, and showing no sign of improvement, the age would be rather an element in deciding in favour of operation.

This would be the place to enumerate the many benefits which are conferred on suitable subjects by the operation, but I think they are sufficiently well known to all of you. Perhaps they have at times been overdrawn, and this is to be regretted, for of the beneficial results of the operation in the cases in which it is indicated there can be no doubt whatever.

I ought, perhaps, to refer to the question of recurrence, or so-called recurrence. Of course in a young child with a tonsil rapidly tending to overgrowth, a small amount left may so increase as

to cause a return of the symptoms. This is, however, so rare after careful removal that its occasional occurrence cannot be adduced as an argument against the operation. Nor can the cases in which the symptoms remain unrelieved, owing to other causes (deflected septum, hypertrophied turbinals, &c.) having been overlooked, be adduced as an argument against removal of adenoids. These causes should not be overlooked by a careful observer.

Finally, I may be asked what is the objection to the operation. Why not be on the safe side, and follow the rule laid down for themselves by certain people, viz. to operate on every case of adenoids without exception? At first sight this way seems plausible enough. The operation is not dangerous, it does no harm, and it may prevent some unforeseen harm, or do some unexpected good. It is not a dangerous operation; still there have been accidents, and there is a certain mortality. There have been deaths on the table from the anæsthetic: there have been deaths from hæmorrhage, very few, but still some; acute suppurative otitis media has been not infrequently set up, in one case I know of so severe as to require the opening of both mastoid antra; and there have been deaths from septic meningitis. Of course fatal accidents are so rare that their occurrence need be no indication against operating in a case requiring operation, but they cannot be forgotten when the wholesale performance of the operation is recommended. Then there is the question of expense. Operations are always more or less expensive to our patients, and this particular operation is, I suppose, no exception. This is perhaps a small matter, and certainly not to be considered in a case where the operation is indicated by the health and well-being of the patient. And then, again, it is an *operation*, and the majority of people do not care for operations on themselves or their children if they can possibly be avoided. This is only natural, and applies quite as much to doctors and their families as it does to the rest of the world.

Before laying down some general rules which guide one in advising operation or otherwise in adenoid cases, I may say that not only the cases, but circumstances vary so much that no rules can cover every case. Thus with some patients there is such an excessive desire to avoid an operation,

even at some risk, that this may naturally turn the scale in a doubtful case, and then you come across others who seem to be curiously keen to have their children operated on. Sometimes this is due to the exaggerated ideas that have got about on the serious effects of adenoids; but whatever it is due to, it may, of course, influence a decision in an otherwise doubtful case. Then you have a case put this way—How is such and such a symptom to be got rid of? You may think the symptom trivial, and one that will pass off, but the patients will not wait, and beg for an operation. These and other circumstances step in sometimes to influence our decision.

I will now conclude by enumerating several symptoms and conditions, any one of which by itself may, in my opinion, constitute an indication for operation. Of course a combination of two or more affords a still stronger indication.

If there is habitual mouth-breathing in a child, which has been going on for a considerable period, and shows no sign of improvement, I should operate. I should include also those children, especially young children, who, though not habitual mouth-breathers in the day, have noisy laboured breathing or suffocative attacks at night. In such a case the appearance of a falling in of the lower part of the chest in a young child would considerably strengthen the indications.

If the child is deaf, or subject to attacks of deafness or earache, or has a chronic otorrhœa, I should recommend operation in all cases, and with a good prospect of cure; but in older children and young adults with long-standing deafness, though I still think we should operate, our prognosis must be guarded.

Repeated attacks of bronchitis constitute an important indication; so does the presence of asthmatic symptoms. Distinct benefit may fairly be expected in these cases. A constant persistent cough without bronchial symptoms, not yielding to ordinary treatment, is often cured by removal of adenoids.

Repeated colds in the head of a severe and prolonged character, or a chronic nasal catarrh, or purulent rhinitis not yielding to simple treatment, may be an indication for operation. Paroxysmal sneezing and hay fever symptoms are also indications for removal of adenoids.

There are finally a few maladies, such as noc-

turnal enuresis, chorea, and epilepsy, where the operation may sometimes be done, although none of the foregoing indications are present, rather with a view to remove all possible sources of reflex irritation than with any distinct promise of direct benefit to the malady in question.

Treatment of Gout.—Ransom regards the use of drugs, other than tonics and digestives, in cases of gouty diathesis which show no actual symptoms of the disease as of doubtful value. The writer's treatment consist of six tumblerfuls of water daily, three of which may be taken hot; small doses of calomel followed by sulphate of sodium, or, instead of the calomel, a pill containing colchicum, colocynth, and calomel; and the taking of medicated baths. Regular bathing is regarded as almost the most important item, and next to it regular exercise, out of doors if possible. All forms of meat are allowed at least once a day, or twice if desired, but no vegetable which grows underground, and a restricted amount of sweets.

In the treatment of chronic gout, in the intervals between acute attacks, the use of mineral baths, with massage and exercise, are regarded as of the greatest value. If there be some local subacute affection present, the exhibition of calomel, followed on the day after by iodide of potassium and colchicum wine, may be given, and continued for several weeks. An ointment containing iodine and iodide of potassium applied to the joints is often of service, or, if there is much pain, ichthyol ointment from 30 to 50 per cent. in strength may be used instead. In acute gout treatment should commence with a full dose of calomel, followed by doses of 15 drops of colchicum wine, which may be increased or diminished according to the absence or presence of toxic symptoms. If the pain is very severe, opium in the form of Dover's powder is indicated.—*Monthly Cyclopædia*.

ACCORDING to Nottage, there is a certain variety of cold for which gelsemium is as near specific as any drug can be. The patient feels cold chills running up the back. A clear watery fluid runs from his nose. Now give the gelsemium, and next morning the cold is gone.

New York Med. Times.

THE DIAGNOSIS OF AN ABDOMINAL TUMOUR.

A Clinical Lecture delivered at the Westminster Hospital,
October 25th, 1898,

By **CHARLES STONHAM, F.R.C.S.,**

Surgeon, Lecturer on Surgery, and Teacher of Operative
Surgery, Westminster Hospital.

GENTLEMEN,—To-day I propose to make some remarks on the general diagnosis of abdominal tumours. Quite recently we have had various abdominal tumours in the wards—tumours of the liver, of the spleen, of the kidney, and of the uterus and ovary. It is an extremely important subject, and I propose to point out to you the methods usually employed in diagnosis.

First, with regard to the history. In getting the history of supposed abdominal mischief it is important to find out how long the patient has suffered, and what are the symptoms of which he or she complains. You should go systematically through the symptoms that may be connected with abnormalities of function of any of the organs, noting negative as well as positive facts. In the case of a woman you should obtain the general history of her menstrual life, of her labours, and as to whether the latter were normal or abnormal, and in the latter case the nature of her illness. In both sexes you will naturally inquire after the condition of the bowels—whether they act freely and normally, or whether there has been chronic constipation or diarrhoea, or constipation alternating with diarrhoea. You should inquire as to the function of digestion; as to whether the patient has ever had any symptoms that point to intra-abdominal inflammation, and if so, how long such symptoms lasted, and whether the attack has been repeated. The latter point is important in connection with the possibility of adhesions occurring in connection with a tumour, or the possible origin of such tumour from previous inflammation, *e. g.* abdominal abscess. Before you actually examine the patient you should, if time permits, have the bowels well opened by a purge, followed by an enema on the morning of the examination; the bladder should be emptied by means of the catheter. During the examination your patient

should be in the supine position, with the legs drawn up, so as to relax the abdominal muscles, and the head should be slightly flexed; it is advisable to have the mouth open, to ensure more complete relaxation. You will find that it is better to talk to the patient during the examination, so that her attention is distracted from what is going on, and she is the less likely to resent palpation. On the other hand, if the patient is watching you, the probability is that she is straining her head to see what is going on, and as she lies supine, this naturally increases the tension of the abdominal muscles, which to a certain extent interferes with a thorough examination. If you find any rigidity of the abdominal walls it is well to tell the patient to take a very deep breath, to be succeeded by deep expiration. As the air escapes the abdominal wall goes back towards the spine, and gives you a much better opportunity of judging whether a tumour is present or not.

When you have thoroughly examined the abdomen, with the patient in the supine position, as I have described, you should shift your patient from side to side to see if posture makes any difference in the symmetry of the abdomen or in the area of dulness which you may find, or whether any tumour which you have discovered by palpation has shifted its position. Having done that, it is well, if any doubt remains, to make the patient stand up, and see if that makes any alteration in the contour of the belly. And lastly, you should make the patient go into the knee-elbow position, which is sometimes very useful.

The general methods of examining the abdomen are similar to those of examining the chest. But whereas in diseases of the chest percussion and auscultation are the two most useful methods of diagnosis, percussion and palpation are chiefly relied on in diseases of the abdomen; auscultation is not of very great importance. Inspection is valuable, but as a rule nothing like so valuable as palpation.

Auscultation is useful to detect borborygmi or the passage of air or fluid fæces through or up to an obstruction. It is also employed in eliminating or confirming the diagnosis of pregnancy. After the fourth or fifth month you can hear the foetal heart-sounds, which, of course, are positive proof of pregnancy. Another use of auscultation is in the case of abdominal aneurisms; but at the same

time you must not rely too much upon the presence of a bruit as being diagnostic of aneurism, because you will hear one in thin patients if the end of the stethoscope is pressed back against the abdominal aorta.

Inspection of the abdomen. The first thing you should observe is whether the size of the abdomen bears a due proportion to the rest of the body. If you find a large abdomen in an individual with small thighs and a small chest, it is evidence, at any rate to some extent, that that abdomen has something inside it which ought not to be there. You should further notice whether the abdomen is symmetrical in shape, and if not, whether the bulging is in the flank or in the middle line above or below the umbilicus. Increased bulging in the flanks points to a tumour either of the kidney itself, in connection with it, or to the presence of ascites; whereas if you observe relative flattening in the flank, with a bulging forwards in or towards the middle line of the abdomen, the appearance suggests rather a tumour in connection with the uterus, ovaries, or mesentery; above the umbilicus a tumour of the liver, stomach, spleen, or pancreas is to be suspected.

Another point in regard to inspection is the condition of the umbilicus. You find sometimes that when the abdomen is filled with a huge tumour, the umbilicus instead of being represented by a depression is more or less a prominence, or is at least flattened out. You should note whether the veins of the abdomen are dilated and tortuous; if so, the condition points to the probability of the existence of some malignant disease in the abdomen. Extensive venous dilatation may be caused by a tumour which is compressing the vena cava inferior. Inspection ought also to afford information as to how much a patient is using the abdomen for respiratory purposes; if it is filled with a huge ovarian cyst or other tumour, you will find that thoracic respiration is very much increased, whereas the abdominal muscles hardly move at all.

Palpation.—Palpation of the abdomen certainly requires care and some skill. In the first place you must be sure that your hands are warm, and it is said—although I rather doubt it—that it is better to warm them in hot water, because it increases the sensitiveness of the peripheral nerves; whether this be so or not, hot water is the most convenient means. In palpation of the abdomen

you should not use the tips of the fingers, but the whole of the flat of your hand. Another thing of importance is that none of your movements must be rough, and none must be rapid; simply put your hand on the abdomen in the place in which you wish to begin examining, and gently depress the abdominal wall, or gradually palpate the whole area. If you find that there is any local resistance, this may be merely that the recti are contracting, because the patient is ticklish, but it may be dependent upon pain. While you are palpating the abdomen, keep your eye fixed upon the patient's face, and you will then be able to tell at once whether you are hurting. Having examined the abdomen superficially first, it is then necessary to palpate deeply. If the patient resists, you must do as I told you just now—make her take a deep inspiration followed by a full expiration. As she does so your hand will go deeply at once. If you find increased resistance in any part, you must very carefully examine that region with the view of detecting a tumour. Palpation, if properly conducted, will enable you to find a tumour in the abdomen if it is of any size; and even quite a small one provided the patient be thin. But if the patient is stout and has much fat in the abdominal wall, and presumably a large quantity in the omentum and retro-peritoneal tissue, you may miss a tumour of considerable size, or a mass of fat in the abdomen may simulate a tumour.

Supposing that by palpation you fail to discover anything, and that the symptoms are mainly referred to the abdomino-pelvic region, the proper course to pursue is bimanual examination, with one finger in the vagina or rectum. In unmarried women you should only examine in the rectum; you ought not to examine *per vaginam* except under exceptional circumstances. Bimanual examination will generally reveal the presence of a tumour which is either entirely within the pelvis, or has risen from it into the abdomen. It will also show the condition of the uterus and its appendages. In cases where palpation is very difficult, either because of the fatness of the patient, of pain, or of rigidity of the abdominal wall, it is wiser to examine your patient on several occasions to see what you can make out from time to time. If you still remain unsatisfied you should give an anæsthetic, but do not resort to this expedient unless you are obliged.

If by palpation you discover a tumour in the abdomen, you must next assure yourself as to its exact situation and limits. This will give you a hint as to its origin, because you may take this as true, that a tumour arising in any organ of the body first of all occupies the position which that organ normally occupies, and although it may spread to neighbouring regions, yet the great bulk of it still remains in the primary position. A kidney tumour, for instance, although it may extend more or less forwards or towards the pelvis, the bulk of it remains in the position normally occupied by the kidney. So it is with cases of ovarian cysts (unless possessing a long pedicle), and tumours in connection with other organs.

Another thing which you must try to discover is whether the tumour be fluid or whether it be solid. Now it is not by any means always easy to be certain on this point. You try to obtain fluctuation by placing the palm of one hand against the tumour, and then lightly tapping it on the other side. But you must remember that some solid tumours are so soft, especially when they are rapidly growing, that they appear to be distinctly fluctuating. On the other hand, some fluid tumours are so tense that you cannot elicit fluctuation, and they appear solid. You may be also unable to get fluctuation in a tumour which is multilocular, or because it contains colloid jelly-like material, which does not convey the impression of fluid to you through the abdominal wall.

You must also ascertain whether your tumour is tender or whether it is painless. If it is tender you should try to find out whether it is equally tender all over or whether pain is elicited only when pressure is applied to one particular part. Tenderness round a tumour may be either deep or superficial; it may be slight or it may be severe; it may be felt by the patient spontaneously, or it may be only elicited by pressure; lastly, pain may have been always present, or it may be that it is a new element in the case altogether. Take an ovarian cyst for example. A patient may go about with an ovarian cyst for a long time without complaining of the least pain, but suddenly from acute peritonitis or twisting of the pedicle she develops a high temperature and great pain on even the gentlest manipulation.

Lastly, you must ascertain the degree of mobility or fixity of the tumour. Freely movable tumours

are usually connected with the mesentery or with some part of the intestine, or with the uterine appendages. Some tumours of the ovary, such as heavy dermoids with long pedicles, are very freely movable; you can move them over a wide area. Another freely movable tumour is a floating kidney, which you can sometimes even push over the brim of the pelvis. Tumours of the stomach are sometimes movable, and they may by their weight drag the stomach down so that the pylorus descends into the iliac region.

Percussion.—For percussion of the abdomen you should have the patient in exactly the same positions as those assumed in palpation and inspection. You must be careful that your percussion is both superficial and deep. By superficial percussion you may get the usual resonance all over the abdomen, and may quite fail to detect dulness due to the presence of a tumour deeply situated. If you percuss deeply to begin with, it is possible you may miss a small tumour which is situated superficially. For detecting a superficial tumour only light percussion is necessary. If you find that the dulness remains in the same place in whatever position you put your patient, it of course points to the fact that such dulness is due to some immovable cause, such as you might get from an adherent tumour or a patch of inflammation. On the other hand, you may find that as you move your patient the dulness which was evident in one area is replaced by resonance, while dulness appears in some other part. Such change of position may be due to free fluid in the abdomen, or to a tumour which is easily movable. In cases of free fluid in the abdomen, if the fluid is not very large in amount you will find dulness in the flanks only; the front of the abdomen more or less retains its natural contour, and is resonant, perhaps hyper-resonant. If the abdomen contains a very large quantity of fluid you will find the dulness extends higher than the flank, and near to the umbilicus. In the case of solid tumours growing in the middle line of the abdomen, such as ovarian cysts or large fibroids in the uterus, there will be dulness in the central line and tympanites all round the dull area. This surrounding tympanitic band is sometimes spoken of as Tate's corona. Tympanites, it is important to note, may occur in connection with abscesses owing to the presence of gas in the sac. When the pus in an abdominal

abscess putrefies, the gases involved in the process of putrefaction tend to distend the sac, and give it a definite tympanitic note.

There are two other methods of diagnosis; one is by tapping. I think one need only mention this mode to condemn it. Tapping is always dangerous, and can hardly ever be required if due care is taken in the investigation of a case by the methods I have given you. When the practice of treating hydatids of the liver was by aspiration it was not by any means uncommon for patients to die of acute peritonitis, due to the escape of the fluid into the peritoneal cavity. Some of you may have seen a case I had last summer, in which I opened the abdomen with the view of removing a tumour, and accidentally damaged a vein lying on its surface. The hæmorrhage from it was terrific. Now supposing that instead of being exposed as it was a needle had been stuck into it for the purpose of diagnosis, the probability is that the patient would have bled to death.

Supposing that by the method of examination that I indicated you still cannot arrive at a definite conclusion as to what the abdominal condition may be, as to whether a definite tumour is connected with a certain organ or not, and as to its nature, it only remains for you to do a cœliotomy. In these days of antiseptic surgery the operation of cœliotomy is perhaps done more frequently than it ought to be—that is to say, it is done for diagnostic purposes too freely. I think one may fairly say that there are few cases indeed in which an exploratory operation is needful to clear up the diagnosis except in cases of obstruction. In these cœliotomy can hardly be said to be exploratory. It is absolutely essential in order to reach the cause of obstruction, although the precise nature of it, whether it be a band or something else, may be doubtful.

Now there are certain conditions which may lead you to make a diagnosis of abdominal tumour when none such is present, or at any rate when no pathological tumour is present. The first of these is pregnancy. When a woman presents an abdominal tumour situated in the mesial line below the umbilicus, the first thing for you to do is to exclude pregnancy. You must bear in mind that in the great bulk of instances this is easy, but in some it is difficult. Some women wish to be pregnant, others have every reason for not wishing to be so;

and therefore the history given by the patients must be guardedly accepted. Also remember another thing particularly—until you have distinctly excluded pregnancy you are not on any account whatever to pass a sound into the uterus with a view of measuring it in a case of supposed tumour. I need not go through the signs of pregnancy; they are sufficiently well known. There is a condition sometimes known as pseudocyesis or spurious pregnancy. This condition has deceived many able men, and will yet do so. It is generally met with in somewhat elderly women who marry late in life and are very anxious to become pregnant. They will present in a modified form many if not all of the signs of pregnancy; there is a definite abdominal tumour, and sometimes even suppression of menses and swelling and tenderness of the breasts. The tumour is due to flatulence or to peculiar local contraction of the abdominal muscles.

So-called phantom tumour is sometimes formed by the accumulation of flatus in the intestine, and sometimes by the contraction of the rectus abdominis muscle. But if ordinary care is taken in examining a woman you will generally avoid making any mistake. Phantom tumour may always be eliminated by the administration of an anæsthetic. They are very rare in men, and pseudocyesis is still rarer.

Another thing which you must always eliminate is a distended bladder. The mere fact that a patient tells you he or she has just passed water and emptied the bladder is not sufficient; it does not show by any means that the bladder is empty; it merely means that the desire to micturate has been relieved. And you may find in such cases chronic retention of urine with an enormously distended bladder, which the patient knows nothing about. Therefore always empty the bladder yourself, and so avoid all uncertainty.

Fæcal accumulation may also lead you to make a mistaken diagnosis. Here, again, the history must not be relied upon too much. A patient may tell you that the bowels have been acting properly, or he may tell you he has had diarrhœa or diarrhœa alternating with constipation. Now alternating constipation and diarrhœa points very strongly to fæcal accumulation. The diarrhœa is dependent upon the formation of ulcers as a result of irritation by scybalous masses; there is a certain amount of enteritis, with ulceration, which

gives rise to a free flow of mucus into the intestine, which, tinged with fæces, is freely evacuated. A fæcal accumulation may be met with in any part of the abdomen, because, although it is true that the colon should maintain a certain definite position in the abdomen, yet at the same time it is liable to many anatomical deviations, and may be dragged out of position by the weight of the mass within it.

The point with regard to such a tumour is not so much the position it occupies as its feel. It is nearly always movable, and if the patient is not very fat, and you can get well down on the tumour, you will find it is almost invariably malleable; it has a doughy, semi-elastic feeling, which is more or less characteristic. The colon, except in the loins, is a comparatively superficial structure, and when it is distended with fæces it becomes still more so. A mass of fæcal impaction may be dependent upon chronic mechanical obstruction, the fæces not being able to pass through the narrowed part of the colon.

I have already mentioned that a very fat patient may present signs similar to those of tumour in the abdomen, and yet be perfectly free of it. There are not a few cases on record in which the abdomen has been laid open in the expectation of finding a definite tumour, but nothing whatever has been found except masses of fat in the omentum or mesentery at the place where the tumour was supposed to be. It is also important to make perfectly certain that any tumour which you may feel is in the abdomen itself, and not in its wall. If there is the slightest doubt on this point, a very good plan which will help you, though I do not say it will be conclusive, is to examine the patient while in the knee-elbow position. If you can make out the tumour as plainly in this position as in the supine, it is probably in or adherent to the abdominal wall; but if the physical signs of the tumour are less evident in the knee-elbow position, it shows to a certain extent that the abdominal wall has fallen away from it. Even supposing you have made up your mind that the tumour is in the abdominal wall, you have also to make certain that it is not a tumour which is extending from the abdomen into the wall, such as an abscess.

The diagnosis of tumour having been definitely settled, its origin and pathological nature must be determined by the history of the case and by the

presence of symptoms referable to the different viscera; tumours of the mesentery, omentum, retro-peritoneal tissue, abdominal parietes and ovary, and abscesses in connection with the psoas do not induce visceral symptoms unless they are sufficiently large or so situated as to induce pressure symptoms.

Treatment of Amenorrhœa.—Leuf states that amenorrhœa may be caused by an underdevelopment of the uterus, or its atrophy, or due to some neurotic influence. Cessations due to wasting diseases are conservative, the flow simply stopping because nature cannot permit the loss of blood in view of the other strain upon the system. An atrophied uterus requires the same treatment as an underdeveloped one, though it is less likely to respond to stimulation if it has existed for a long time, for retrograde changes are generally not as easily reversed as natural development is stimulated. The flow is most readily stimulated by the bipolar application of the long, fine-wire, secondary faradic coil two or three times a week for from two to six months, continuing for ten or fifteen minutes at each sitting. It has also been accomplished by the passage of a similar current through the pelvis from the sacrum to the hypogastrium, the anode being behind. When the flow is simply scanty, the galvanic current is better, the cathode being in the uterine cavity and the anode upon the abdomen, with a current-strength of from 50 to 75 milliampères. The leading fact should not be forgotten that the cathode increases circulation and nutrition, and that the anode has the opposite effect.

W. E. Fothergill claims that senecio will not cause abortion, or in any way influence the course of pregnancy. The practical utility of a drug which will cure functional amenorrhœa, but will not interfere with pregnancy, is, of course, obvious.

Monthly Cyclopædia.

Naphthionic Acid as a Preventive of Iodism.—Captain ('*La Médecine Moderne*,' June 4th, 1898) recapitulates the essential known facts about iodism. He says that it is due wholly to the elimination of iodine by the various mucosæ of the body. The decomposition of the iodine salts which precedes this elimination is due to the presence of nitrites in the blood. By using naphthionic acid this decomposition is prevented.

Philadelphia Polyclinic.

CHAPTERS FROM THE TEACHING OF DR. G. V. POORE.

No. XIII.

GENTLEMEN,—There are sundry salts which, given in large quantities, are poisonous, and I shall not detain you very long with them. One of them is nitre—*potassium nitrate*. I take it sodium nitrate would behave in much the same way. Sodium nitrate is extensively used at the present time in agriculture, and therefore it is easily got, and may be taken accidentally. I do not find any record of poisoning with it, however. When a big dose of salt such as nitre is taken, you get symptoms of gastric irritation accompanied by very great collapse, and it is said that with nitre there has been recorded perforation of the stomach. Another salt which sometimes has caused gastric irritation is *potassium sulphate*; and it is said that in France potassium sulphate has been given in large quantities with the idea of procuring abortion. It does not seem to have been used in this country for that purpose. It acts as an irritant, and I do not think it need detain us.

I need hardly do more than mention the fact that any common purgative drug if given in excessive quantity may cause violent purging and collapse. For instance, Epsom salts or Glauber's salts given in excessive quantity will have that effect; and alum is one of those bodies which has caused the same thing. There is another body I want to refer to, which is used in the arts a great deal, namely *barium*, and the salts of barium are said to be very poisonous. For instance, there is recorded a case of death with vomiting, purging, and collapse in seventeen hours after taking 100 grains of barium chloride. An assertion has also been made that very small quantities of barium chloride, even a grain or so, will produce violent irritation. I think, however, that that is a statement which rather wants confirming. It is said that salts of barium have a specific action upon the heart, and that they act upon the heart not unlike digitalis, and that they bring the heart to a standstill in systole. Barium is used, amongst other things, for sizing cotton warps; that is, I believe, for giving a white appearance

and a heavy weight to bodies which have no legitimate right to either of those qualities. There is recorded a case of a man who died in fourteen hours from eating a piece of barium nitrate as big as a bean. But I warn you that some of these out-of-the-way cases must be received with caution, and we must always have an open mind on the question of how "ripe" so to say the victim was for death before he took the material which immediately preceded the catastrophe. Now the salts of barium are amongst the things for which you must know the tests. I will put a little barium nitrate in each of these tubes. We have a clear solution, and if I evaporate a drop on platinum foil over the spirit-lamp I get a fixed residue. There is but little change upon the blue litmus paper, nor upon the red. Therefore I have practically a neutral solution which gives a white solid residue, but no blackening. By adding a drop of hydrochloric acid I get no precipitate nor any effervescence; whatever I have got there, it is not a carbonate. Then I go through with my sulphuretted hydrogen and ammonium sulphide until I come to the addition of ammonia and carbonate of ammonia. You see that I get a dense precipitate immediately. Therefore we have one of the alkaline earths, and the only alkaline earth with which we are concerned is barium. I find that it is soluble in acetic acid. I now add to my original solution some sulphuric acid. We get a dense precipitate which is insoluble in nitric acid. That is test enough I take it, but we will put a drop of barium chloride upon the platinum foil and hold it in the flame. If the room were darkened you would perhaps see, as I do, a green colour, but flame tests are not of much value.

Iodine has caused death with all the symptoms of irritation and purging. Iodine has this peculiarity, that if the man vomits and there be any starchy food in the vomit you get a blue coloration. Death has been caused by drinking the tincture of iodine or the liquor iodi, and there have been cases of death from injecting ovarian cysts with the tincture, and one case after injecting a spina bifida with the strong tincture. In one case death occurred thirty-six hours afterwards, in two cases on the tenth day without warning. In chronic cases of iodine poisoning we get a condition of iodism, with which you are

familiar. When patients take large doses of iodide of potassium they water at the eyes, there is running of the nose, and sometimes there is salivation. Not infrequently there are exanthemata, which are papular or pustular. You may remember one of my patients who was taking iodide of potassium had a group of pustules across the centre of his face which looked very much like an eruption of smallpox. It is said that when iodine is taken for a long time you are apt to get atrophy of the mammae and testicles, but it has not fallen to my lot to see either condition from the use of the drug. The treatment is by means of the stomach-pump, and you must counteract the collapse by giving stimulants. It is said that starchy food is in some sense an antidote. If a patient is taking iodine it is eliminated by the saliva, and many of you must have seen me in the wards, more than once, demonstrate the presence of iodine in a man's saliva by a very simple manoeuvre. Let the patient spit into a white porcelain vessel of any kind, mix that saliva with a little starch—there is always some arrowroot or something starchy in the ward—then pass the electric current through it and electrolise the mixed fluid. The iodine is then liberated at the positive pole, and the starch is coloured blue. We have been able to demonstrate that fact in a patient nearly a week after he had ceased to take the drug. In trying an experiment upon myself once I found I could detect iodine in the saliva thirty-six hours after the administration of two grains; therefore the test is a very delicate one indeed. One caution is that you must use unpolarizable electrodes; if you use ordinary copper wire you will get green staining, and if you use iron wire you will get red colorations which interfere with the reaction. Platinum wire is the best to use. Or you can use gold, hooking the wire round a couple of sovereigns, or, better still, round two breast pins.

Bromine is another element which I do not think need detain us. It is said that a man has killed himself by drinking one ounce of pure bromine. Strong bromine water coagulates albumen and destroys the skin. It is said that workmen who have been splashed with strong bromine water have suffered in this way. Again, the symptoms of bromism are seen in those who are taking bromide of potassium—such, for instance, as epileptics;—and bromine, like iodine, causes eruptions

on the skin, and an eruption quite indistinguishable from ordinary acne on the face. An epileptic, therefore, often has to take the choice between acne and fits, and there is not much doubt as to which he would choose. The other symptoms of bromism are said to be dizziness, somnolence, and depression. The acuteness of intellect is somewhat blunted, and there is diminished irritability and diminished sensibility, especially about the pharynx and palate. It is said that people who take bromine become very aphasic, and that after a time they suffer from definite cerebral degeneration. There, again, we have a complication, the complication being that almost everybody who takes bromide continuously is an epileptic.

Leaving now these materials of small importance—that is to say, the salts of the alkalis and the alkaline earths, iodine, bromine, and chlorine—we come to a poison which is more important, and which well merits our attention, namely, *phosphorus*. Of course, phosphorus is a modern poison. Phosphorus was discovered at the end of the seventeenth century—1678—by a German alchemist named Brandt. It was nearly another century before it was rediscovered or obtained in a different fashion by Scheele, a Swedish chemist. But for a long time, even after Scheele's discovery, phosphorus remained a chemical curiosity rather than anything else, and phosphorus did not become to any extent an article of commerce until the invention of the lucifer match, when we said good-bye to the tinder-box. With the invention of the lucifer match phosphorus became a common article of commerce. The lucifer match was first made in Vienna in the thirties. So rare was phosphorus as a poison that it is mentioned by Orfila only in connection with a few experiments on animals. Then I may note that phosphorus as a poison was not included in the first edition of Guy's 'Forensic Medicine,' published in 1854. We now know that phosphorus is a very great cause of fatty degeneration, especially of the liver; and again I may mention it as an interesting fact that in Sir Samuel Wilks' book on 'Morbid Anatomy'—as good a book of its kind as was ever published,—the first edition of which appeared in 1859, the fact that phosphorus produces fatty degeneration is not mentioned. You will see the bearing of all this presently. We come to the year 1860 before the

fact that phosphorus produces fatty degeneration is what I may call common knowledge. Medicine is so wide a subject, and there are so many workers in it, that it progresses from one day to another; and one feels, in consequence of the enormous army of workers, that one's ignorance of the last thing which has been done is something which it is almost hopeless to try to grapple with. If, therefore, I say that the connection of phosphorus with fatty degeneration was not known in 1860, I may be contradicted, and some one may say, "So-and-so pointed it out thirty years earlier." But if a man of Wilks' observation and experience, working regularly in Guy's Hospital, does not mention it, I think I am right in saying it was not then a matter of common knowledge.

Phosphorus is obtainable in two forms—there is the common yellow phosphorus and amorphous phosphorus. Common yellow phosphorus has certain properties, namely, that of fuming in the air, which amorphous phosphorus has not; and, as far as we know, the poisonous properties of phosphorus are restricted to yellow phosphorus. The phosphorus can be swallowed in many ways—in the form of a stick, and it is said that stick phosphorus swallowed in bulk is not so dangerous as phosphorus in a state of fine comminution. I may remind you that people have taken large doses of mercury to overcome obstruction of the bowels without being poisoned, whereas had they taken one hundredth or one thousandth of that dose in the form of grey powder or blue pill, they would have suffered severely from mercurial symptoms. Phosphorus has been taken in the form of match heads (made with yellow phosphorus), and there have been many cases of death from children sucking match heads. Sometimes match heads have been pounded into a paste, and taken in that way deliberately. Phosphorus is one of those poisons which is easily obtained. Here I have a bottle of so-called phosphorus paste, which is used for killing blackbeetles and other animals of that kind. The instructions say, "Spread the paste rather thick on small slices of bread, and place them overnight in or near their haunts"—it does not say whose haunts;—"in the morning remove all the remains, and renew with fresh paste the following night till the place is clear. Dogs and cats will not eat it. To prevent accident, be cautious to place the paste or any vermin found

dead from its effects beyond the reach of children and domestic animals."

That is a very important matter, and I shall allude to the fact because it is conceivable, although I do not know that it has ever been proved, that you may have second-hand phosphorus poisoning. For instance, some of you may be able to tell me whether chickens will eat blackbeetles. If they were to eat phosphorus-killed blackbeetles, they might become tainted with phosphorus themselves, and a human being eating the chicken might become tainted with phosphorus. That is a very important possibility, and it is one of those things which have to be determined. The fact whether a person has had phosphorus or not is sometimes difficult to determine, and you will do well to cross-examine, and to look to all of the out of the way channels by which the person may have got it. Phosphorus is soluble in oil and in ether, and some of the ethereal solutions of phosphorus have produced poisoning. As we all know, phosphorus fumes in the air, and in the dark is seen to be luminous, the luminosity being due to its rapid oxidation. Therefore people have said that phosphorus "burns;" and writers on phosphorus poisoning have rather dwelt upon the initial symptoms of irritation. But it is not very clear whether the symptoms of early irritation are very strongly marked, at all events whether they are always so marked. People have taken a poisonous dose of phosphorus without suffering from symptoms of severe gastric irritation. If you get the idea that a man who swallows a bit of phosphorus has got something like a stove lighted in his stomach, you would be carried away with an idea of an amount of irritation which is not always certain. Irritation is, however, present in some of the cases, and you get the phosphorus odour of the breath, and the vomit may be bloody and it may be luminous if the vomiting be in the dark. Or the vomit may be mixed with colouring matter, because the phosphorus pastes are mixed with colouring matter. After the initial symptoms of phosphorus poisoning have passed off there is a lull, and the patient is thought to be well. You may be called to somebody who has taken phosphorus, and you may find the symptoms of gastric irritation; you may go to-morrow and find the symptoms less severe. Next day you may find the patient pretty well. Now

be not deceived. That lull in phosphorus poisoning is altogether fallacious; it does not at all mean that the poison is out of the body. The next symptoms which present themselves are very startling, consisting first of all of jaundice, within four or five days or a week of taking the phosphorus. The jaundice gradually deepens, and it is accompanied by all the usual symptoms of jaundice. In the early days there is pallor of the stools and high colour of the urine, but the coloration of the urine usually subsides before the jaundice. Then with the jaundice there is sometimes irritation of the skin and urticaria, and there is often great pain over the liver. The only two cases of phosphorus poisoning I have seen had one symptom in common, namely, that they both cried out in agony at any accidental jog of the bed. Anybody accidentally knocking against the rail of the bed caused the patients very great pain, and made them cry out. Probably the pain was caused by imparting motion to the swollen liver, and in these cases the liver is usually enlarged. With the enlargement of the liver there are other symptoms, and one of the symptoms which is common is hæmorrhage. You may get hæmorrhage from various points; you may get petechiæ under the skin, you may get blood in the vomit, or you may find blood in the urine; and very often there has been bloody discharge from the vagina in females. Again, there is great weakness, the prostration is almost absolute; the first sound of the heart gets exceedingly weak, the pulse is fluttering and variable, and the tendency to hæmorrhage increases. It is said that a leech-bite in a case of phosphorus poisoning has been known to bleed to a dangerous extent. The intellect remains clear almost up to the last, and death is very often ushered in by coma and convulsions. The urine is scanty, often bloody and albuminous, the urea is diminished, and it is a question as to what extent you find leucin and tyrosin in the urine. It is said that you find paralactic acid if the patient lives some time. It is undoubtedly the fact that the liver may diminish in size, or it may remain enlarged.

Now with regard to death. Death has occurred rapidly when children have taken phosphorus, probably a relatively large dose, and that which has killed them has been the immediate collapse. It is said that they have died in nine hours, but it

is not the rule; they usually die in a week or ten days.

With regard to the quantity which will kill, that is a very important matter. It is said that a child has died after sucking two matches; while, on the other hand, it is said that the French juggler "Jabert, the fire king," could swallow sixteen grains; that is of course a very big dose. He took it in a lump and swallowed it, so that he might have luminous breath and so astonish his audience. Parrots, it is said, can take phosphorus with impunity.

Post mortem the most noticeable fact is the jaundice. In the only post-mortem I have done on a case of phosphorus poisoning the body certainly smelt peculiar; that is all I can say, for it is an odour I cannot describe. Another medical man of large experience assisted me, and his assistant and our late deadhouse porter Sainsbury, who had had enormous experience of post-mortem work. We all agreed that the odour was peculiar. Another important fact which is noticeable in these cases is the fatty degeneration. On opening the body the first thing which strikes one is the look of the liver. In the case I am particularly referring to it looked like a mass of baby's fæces; it was smooth but small, and bright yellow in colour. The next point is the fatty degeneration of the heart. The heart in this case was obviously yellow in colour, and fatty degeneration had advanced to a great extent. There is also fatty degeneration of the muscles, which are pale, and in the kidneys the epithelium is undergoing change; if the patient has lived long enough the kidneys change colour. In fact, if I may say so, phosphorus is the genius of fatty degeneration. You also find ecchymoses internally just as you do externally, and I take it that in all probability these ecchymoses are due to fatty degeneration of the vessels. In the case to which I have alluded there was no hæmorrhage anywhere except in the uterus, and that was not excessive. The gastric mucous membrane may be swollen, ulcerated, or ecchymotic; but in the case I saw there was but little amiss with the stomach. Neither, in that particular case, could we determine the presence of any luminosity; but it should be remembered that the patient lived eight or nine days. Caspar records a case in which a body within twenty-four hours of death was found luminous all over, and he says there was a lumi-

nous vapour streaming from the vagina. Therefore you will see there is no rule in these matters; you may get great luminosity, or you may have no luminosity. Whether there is or is not luminosity probably depends on the quantity taken, and the time which has elapsed since the poison was taken more than anything else.

DIABETES AND BICHLORIDE OF MERCURY.

By ABRAHAM MAYER, M.D.

I OFFER for consideration a preliminary report of clinical observations I have made in the reduction of sugar in the urine of patients suffering from diabetes mellitus, and an abeyance of all of its concomitant symptoms by the administration of a drug well known to all of us.

The results are so marvellous that were they obtained in only a single case you would perhaps hesitate to believe in the efficacy of the remedy; but as they are the same in all of the eleven cases experimented upon, as I will show, there can be no denying that *post hoc propter hoc* in the application of this remedy is a positive fact.

I have not completed all the experimental work, and have not elaborated all the scientific facts as to the physiological action of the remedy in this disease, as such takes and entails a great deal of time and an enormous amount of experimental research, which up to the present I have not had. My observations date back over the past six months, and during this short time the clinical results have been so positive that I feel justified in reporting them to the profession. For this reason I ask an apology for the incomplete scientific researches brought out in this paper, and offer it to you, as my title reads, only as "a preliminary report of the successful reduction of sugar in the urine, and the subsidence of the pathological symptoms in diabetes mellitus." These observations are all clinical. The time has been too limited for the assurance of a positive cure, and I can only add that if the results continue as they have been so far in all of my cases, we have a remedy in our hands that has exceeded all other therapeutic measures.

To go into the physiology and pathology of sugar excretion is beyond the scope of this paper. You know there are theories upon theories as to

the production of glycosuria, and to give a description of each would entail volumes.

As the most careful examinations of the brain, pancreas, liver, and other organs in a great many cases fail to reveal any pathological change to which importance can be attached, and also no history of any supposed exciting cause as being an ætiological factor; and there being such a diversity of opinion as to the causative factors in the production of this disease, and not one explaining all the pathological phenomena connected with it—my experience and observations led me to reason that a bacterium or its ptomaine may be the chief factor in its production.

Indeed, only recently attention has been called to the contagiousness of diabetes mellitus by Ledieu, Naunyn, and others.

Diabetes mellitus, then, according to my theory, is the result of the pernicious influence of a specific bacterium or its ptomaine in the organism, acting centrally on certain brain centres or peripherally on the glycogenic reservoirs.

The nervous system exercises a controlling influence on the normal production of sugar in the body. If from any cause—such as traumatism, neoplasm, continued worry or mental strain, poisons, &c.—this controlling influence be impaired or abolished, the bacteria and their ptomaines exert their deleterious influence.

My theory supposes the invasion of the organism by these bacteria, or their presence in it. In perfect health they are prevented from exercising their injurious influence by the normal resistance of the economy, or perhaps by some ferment physiologically produced by metabolic changes in certain organs of the body acting as an antitoxin against these bacteria.

If this presumption be correct, then the ingestion of a suitable bactericide in sufficient quantity to counteract the effect of this bacterium should manifestly show an amelioration of the pathological symptoms of diabetes mellitus.

After repeated experimentation I am able to report that the bichloride of mercury in suitable doses fulfils this indication.

My method of treating these cases is as follows:—A history of the case is obtained, and the patient weighed. The quantity of urine passed in twenty-four hours is collected, measured, the specific gravity taken, quantity of sugar estimated, and

other abnormal characteristics are noted. If the urine fails to react to Gerhard's test, the patient is then put on an antidiabetic diet for two weeks, during which time he takes the bichloride. I begin with a dose of one twelfth of a grain three times a day, and in two or three days increase it to one tenth or one eighth of a grain. In a week's time my patient is taking one sixth of a grain three times a day, directly after meals, well diluted with water. This is the maximum dose. None of the symptoms of mercury poisoning showed themselves in any of my cases. A rather brisk catharsis takes place the first few days, especially if the patient previously suffered from constipation; and this diarrhoea I regard as favourable. At the end of two or three weeks my patients have shown a marked reduction of sugar and a decided improvement in their general condition. As soon as I have obtained this result the dose of mercury is diminished until about one fourth of a grain in divided doses is taken daily. Or I may still further reduce the dose. (The diet now is not so severely restricted.) A little bread and a tart apple are permitted.

The urine is examined daily or every other day for the first two or three weeks, that voided in the evening being tested.

As the percentage of sugar diminishes the dose of bichloride is decreased accordingly, and the diet is not so strongly restricted. From time to time the drug is suspended. Even though the sugar has entirely disappeared, I continue to give the bichloride, but in much smaller dose—about one sixteenth of a grain three times a day.

Diabetics with syphilitic history are not included in this report; but I wish to mention that diabetic patients with a history of lues showed a more marked and rapid improvement under the mercurial treatment.

Medical Record, December 10th, 1898.

CHOREA OF PREGNANCY.

ACCORDING to Delage, chorea of pregnant women, which is nearly identical in its clinical form to ordinary chorea, is by far more frequent in women having had this nervous affection in childhood, and this fact is consequently an important predisposition for the disease.

As to the influence of one or several pregnancies,

we may conclude from reported cases that if chorea is more frequently met with in the primipara, multiparae are not exempt, although they may not have been afflicted with it during former pregnancies.

It makes its appearance more frequently during the third month of gestation, less often during the fourth, fifth, and sixth months. When once the disease has occurred, it only disappears, in the majority of cases, after delivery has taken place. During the puerperium the inco-ordinated movements diminish in intensity, and a cure, which may take place during the third or fourth day *post partum*, is established, usually in from two to four weeks after labour. Occasionally, however, a chorea may diminish during gestation; the movements persist up to the time of labour to a slight degree: but in some instances they increase in intensity during labour, and at last disappear completely during the *post partum*. Such a course of events is more usually observed when a treatment has been ordered. Occasionally a complete cure may take place during pregnancy.

The most important point to consider is the prognosis of this complication of pregnancy as regards both mother and child. If statistics are only to be taken into consideration, the prognosis would appear most serious, because the mortality of 186 cases collected by Delage was 25 per cent. But it must be recalled that the most serious cases are the ones usually reported in medical literature, while slight cases pass by without being recorded.

On the other hand, a number of these patients die from other conditions than chorea, so that it is in reality quite a difficult thing to form an idea as to its real gravity. All that can be said, as Tarnier has pointed out, is that the prognosis should be reserved, and the family should be warned as to the possibility of serious events occurring. The same difficulty exists in considering the prognosis of the child.

Treatment with chloral and bromide of potassium has given good results, but they should be exhibited in large doses, as much as from six to eight grammes of chloral and quite as much, it not more, of the bromide, according to Pinard. In serious cases, artificial abortion may become necessary.—*Annals of Gynecology and Pediatrics*, November, 1898.

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THE DISEASES AND PRIMARY TUMOURS OF THE THYMUS GLAND.

BY

H. D. ROLLESTON, M.A., M.D.Cantab., F.R.C.P.,

Physician and Lecturer on Pathology at St. George's Hospital; Senior Physician to Out-patients, Victoria Hospital for Children.

THE thymus and the spleen form a distinct type of the ductless glands; they are largely composed of lymphoid tissue, and, though they may be affected by disease originating elsewhere, are not, like the thyroid and supra-renal bodies, which may be considered as belonging to the type of the epithelial ductless glands, the primary cause of diseases like myxœdema, exophthalmic goitre, or Addison's disease.

There is, however, a difference between the thymus and the spleen in that the anatomical position of the thymus, in close contact with the trachea, heart, and great vessels, renders any increase in its size of importance from the mechanical pressure to which the neighbouring structures are subjected, while in the case of the spleen very great enlargement is readily compensated for in the capacious peritoneal cavity.

It may be interesting, therefore, to consider systematically the various morbid conditions and associations with disease to which the thymus gland is liable.

Preliminary.—The thymus is found in all vertebrate animals; entire absence of it in the newly born is extremely rare. It is a temporary lymphatic gland, reaching its maximum development at two years of age; it then remains in a more or less stationary condition until the eighth year or even later. It rapidly atrophies at the time of puberty, though it may, as will be seen later, persist for varying periods into adult life. In the process of involution the lymphoid tissue disappears and undergoes fatty infiltration, until eventually the place of

the organ is taken by a mass of fat and connective tissue lying over and adherent to the pericardium. The process of fatty infiltration can often be seen microscopically, groups of fat-cells separating the lobules and cells of the organ.

The size and involution of the thymus are subject to very considerable variations, and are far from being regular or conforming to any rigid time limit.

It has been called the lymphatic gland of infancy, and might be thought to have some analogies with the hibernating gland of animals. Its duration of activity in the human subject also partly corresponds with the repose of intra-uterine and early independent life.

With regard to the *physiology* of the thymus there is little to say beyond classing it with lymphoid tissue generally; it is, like other lymphatic structures, apparently an important centre for leucocytosis, and to this its enlargement in certain chronic and possibly toxic conditions, such as cretinism, exophthalmic goitre, &c., might perhaps be assigned. As bearing on this it may be mentioned that the administration of thymus gland medicinally has sometimes appeared to have a good effect in the ordinary goitre* and exophthalmic goitre.†

The extract of the gland has, like that of other tissues, been found to lead to intra-vascular clotting; it also lowers the blood-pressure. In connection with this last physiological effect it has been suggested that some cases of sudden death in children, where the thymus is found to be much enlarged, are due to the entry into the circulation of an excess of this substance, manufactured normally by the thymus.‡ This method of sudden death, if it occurs, is analogous to that sometimes seen after operations on the thyroid gland in exophthalmic goitre; in the latter case it is thought that owing to manipulation an excessive amount of thyroid secretion is forced into the circulation.

The physiological importance of the thymus gland in early life has been investigated by removing it from animals, and noting the results. In rabbits no change occurs in the blood, blood-

forming tissues, or in the weight of the animal. In dogs its removal is followed by anæmia, leucocytosis, weakness, increased appetite, and impaired growth, but the animals are able to live without it. Though the thymus resembles the spleen, it does not undergo any compensatory hypertrophy in splenectomised dogs,* as one might perhaps have expected. After removal of the thyroid the thymus is often found to be enlarged, but this does not prevent the characteristic symptoms developing.†

In frogs, however, complete removal of the thymus is described as bringing about death from toxæmia.

Persistence.—The thymus not uncommonly is found well formed and normal in anatomical and histological structure long after the time at which it should have completely atrophied. It may be found accidentally when death has occurred from some definite and sufficient cause, so that no question arises as to whether the thymus had anything to do with the fatal result. One of the best examples that I have met with was in a boy aged sixteen years, who died from fracture of the skull, the result of a bicycle accident. Claude‡ has recorded persistence of the thymus in a woman at the advanced age of ninety-four years.

In some cases the persistent thymus either preserves the size of a well-developed gland or is somewhat smaller; in the latter case its involution has been delayed, but is nevertheless commencing. In other cases the persistent thymus is much larger than it is at its normal maximum; there is then either hypertrophy or hyperplasia in addition to persistence.

Persistence and hypertrophy of the thymus are often difficult to separate from each other; the size of the thymus is subject to considerable variations, and the rate and degree of its involution, as already stated, is far from uniform in different individuals. The enlargement may be due to hypertrophy, *i. e.* to an orderly and regular increase of the constituents of the normal gland, or to hyperplasia, in which there is an overgrowth of the lymphoid tissue, so that the normal histological appearances are lost.

* Mikulicz, 'Berlin, klin. Woch.', April 22nd, 1895.

† D. Owen, 'British Medical Journal', 1896, vol. ii, p. 1017. Cunningham, 'New York Medical Record', January 15th, 1895.

‡ Svehl, 'Wien. med. Blatt', 46-52, 1896. Abstract in 'British Medical Journal', 1897, vol. i, epitome No. 437.

* Laudenbach, 'Archiv de Physiolog.', 1897, p. 410.

† Cadeuc et Quenard, C.R. Soc. Biol., Paris, 1894, p. 308.

‡ Claude, Bull. Anat. Soc. Paris, 1897, p. 219.

In order to distinguish with certainty between these two conditions—persistence and hypertrophy of the thymus, and persistence with hyperplasia—microscopic examination is necessary.

In hypertrophy the normal arrangement and lobulation are fairly well preserved; it is possible that a diminution in the number of Hassall's concentric corpuscles may occur, and be due to phagocytosis. They may also vary in size without any ascertained cause, and show degenerative changes. Thus, in a man aged twenty-six, Jacobi found the persistent thymus containing calcified concentric corpuscles.

Persistence of the thymus in certain diseases.—

This persistence of the thymus may, if diseases such as exophthalmic goitre, Addison's disease, myxœdema, or acromegaly supervene, enable the gland to undergo an hypertrophy. Possibly it assists in the production of an antagonistic leucocytosis. It is difficult to go further, and to believe that the thymus has any special internal secretion, or if it has, that it is in any way different from that which hypothetically might be provided by the lymphatic glands and lymphoid tissues of the body generally. This view is perhaps chiefly suggested by the clinical observation that thymus gland has seemed to do good in some cases of Graves' disease; it might be thought that an internal secretion of the thymus antagonises that of the thyroid, but in this connection it should be remembered that while the thymus gland is practically always enlarged in exophthalmic goitre the symptoms are not thereby relieved. So that at best it is only an attempt at compensation that has failed; on the other hand, Dr. Hector Mackenzie* has come to the conclusion from the examination of the published cases, and from personal observation, that thymus feeding does not really modify the course of Graves' disease.

Marie† has described the thymus as becoming revived or rejuvenated in certain conditions of the thyroid and other vascular glands; for example, in such different diseases as myxœdema and exophthalmic goitre. But it is difficult to explain this as other than an accidental persistence if we accept the view that these two diseases are due to the

opposite conditions of athyroidism and hyperthyroidism—an absence on the one hand, and an excess of thyroid secretion on the other.

In cretinism or congenital myxœdema the thymus is, according to Marie, almost constantly persistent. In these various conditions, however, the revival or persistence with hypertrophy are unable to cure or prevent the symptoms of the disease. It is noteworthy that a persistent and enlarged thymus has been found in cases of epilepsy, and in a peculiar diathetic condition called the status thymicus or lymphaticus, in which the other lymphoid tissues of the body show hyperplasia.

Histological condition of the thymus when persistent in various diseases.—In cases of exophthalmic goitre,* and in one case of acromegaly observed by myself, the concentric corpuscles of Hassall in the persistent thymus were found to be larger than natural. In the latter disease they might be said to share in the giant growth.

In a case of Graves' disease Soupault† described a further stage, viz. an adenomatous growth of the epithelial elements of the thymus gland. In a case of hæmophilia, and in one of purpura, Acland‡ noted the presence of what were probably epithelial proliferations of the concentric corpuscles of Hassall.

Enlarged thymus and sudden death.—A subject of considerable practical interest is the relation of an enlarged thymus gland to sudden death. In many cases of sudden death occurring in adults who have previously, as far as is ascertainable, been in fair health, there is very little found at the autopsy to account for death, which is often due to cardiac failure or syncope; this is a fact within the experience of every pathologist. In some similar cases occurring in earlier life a large thymus is the only gross change found, and is therefore thought to be the cause of death. It is possible, however, that the presence of a large thymus is sometimes a mere coincidence. On the other hand, there are various ways in which an enlarged thymus gland has been thought to precipitate death. Death has been referred to sudden engorgement of the thymus mechanically interfering

* Mackenzie, 'American Journal of the Medical Sciences,' February, 1897.

† P. Marie, Bull. et Mém. Soc. Méd. des Hôp. Paris, 1893, p. 136.

* Edmunds and Mackenzie, 'Path. Trans.,' vol. xlviii.

† Soupault, Bull. Anat. Soc., 1897, p. 592.

‡ Acland, 'Trans. Path. Soc.,' vol. xxxvi, p. 491.

with respiration by compressing the trachea against the spine. This mechanical effect is much more likely to be effective if the head and neck are extended on the trunk, as may occur when the head is allowed to fall backwards over a pillow. Under such circumstances the enlarged thymus is fixed by the manubrium sterni in front, and the lordosis of the spine compresses the trachea between the bodies of the vertebræ behind and the thymus gland in front. This compression is then likely to occur at the junction of the cervical and thoracic portions of the trachea. It is said that in rickety children with muscular debility the head may fall back, and that the feeble child may not be able to flex it; if there is a large thymus present the trachea may be so narrowed that death results.

This explanation for "thymic asthma" probably accounts for some cases, but thymic asthma and laryngismus stridulus have been somewhat mixed up. Some cases regarded as thymic asthma have been found to be associated with thymus glands of normal or small size, and are therefore probably examples of that rare event, death from laryngismus stridulus. Some authorities, in fact, consider that most, if not all, of the cases of so-called thymic asthma are cases of laryngismus stridulus in which as a coincidence the thymus has been enlarged. There is at any rate absolutely no evidence that laryngismus stridulus is solely due to an enlarged or hypertrophied thymus.

Probably many cases regarded as thymic asthma are really due to laryngismus stridulus, while there seems to be no doubt that the enlarged thymus can, if the head be retracted, compress the trachea, so that the view that the enlarged thymus can mechanically obstruct respiration must not be given up.

It has also been suggested that the enlarged thymus may produce and throw out into the circulation a large amount of the substance which endows the watery extract of the gland with the power of lowering the blood-pressure, and that fatal syncope is thus produced. This would account for cases where death has occurred from cardiac failure and not from asphyxia; but it is difficult to see why the toxic effect, in a lesser degree, should not have been manifested more frequently in the same individual, and why it should only be exerted in a single attack so

intense as to destroy life. Though an ingenious suggestion, and one in touch with the spirit of the present time, which explains everything by a toxin, it cannot be said that it is in any way proved.

A large thymus may press upon the heart and vessels, and so embarrass their action as to lead to cardiac failure. Another mechanical effect of an enlarged thymus that is quite conceivable is pressure on the vagi; some sudden movement might easily so stimulate the vagi as to stop the heart in diastole. This was perhaps the explanation of death in a boy aged six years, who died of cardiac failure lasting twenty minutes, and whose thymus was in a condition of marked hyperplasia, the thymus together with the trachea weighing no less than eleven ounces. During life he was under the care of Dr. H. A. des Vœux, who recognised the existence of a tumour in the anterior mediastinum.

Another and important explanation of the association between the occurrence of sudden death and the presence of an enlarged thymus gland is that the enlargement of the gland is part of the general condition called lymphatismus, status lymphaticus, or status thymicus. The subjects of this diathesis have a pale transparent skin, and are well covered with fat; the tonsils, lymphoid tissue at the base of the tongue, the lymphatic glands, thymus, and spleen are large; there are other congenital changes—for example, in the eyes, and hypoplasia, or insufficient development of the aorta.

These children, like rickety infants, are liable to laryngismus stridulus and tetany, and, according to Escherich,* are in a condition of latent irritability of the nervous system, and are therefore especially likely to be the subjects of convulsions, and to die from cardiac failure without any very sufficient cause. Thus deaths during or after emotional disturbance, anæsthetics, small operations, and other trivial causes may be explained. Osler† speaks of one such case proving fatal during the administration of anæsthetics for adenoid vegetations; this raises the important question, To what extent are adenoids a manifestation of lymphatism, a condition in which death may unexpectedly supervene and upset our calculations? The tragic death of Professor R. Langerhans' son in

* Escherich, 'Berlin. klin. Woch.', 1896.

† Osler, 'Practice of Medicine,' ed. 3, p. 827.

1896, immediately after his father had given him a hypodermic injection of Behring's antidiphtheritic serum in order to protect him against infection from a case that had arisen in the house, is explained by Escherich as being due to lymphatism. More than forty cases of sudden death occurring in lymphatism have been recorded.

To sum up—(1) An enlarged thymus gland may be found as a coincidence in cases of sudden death; for example, in laryngismus.

(2) It may mechanically cause compression of the trachea, especially when the head and neck are extended, and so give rise to asphyxia.

(3) It may mechanically compress the heart, or possibly the vagi, and so lead to cardiac failure.

(4) An enlarged thymus may be part of the status lymphaticus, which is a condition where death may follow apparently inadequate causes.

Hyperplasia of the thymus gland.—Hyperplasia of the thymus gland may give rise to a tumour of considerable size; thus, in a case already referred to, the thymus and trachea of a boy weighed eleven ounces. Hyperplasia must be distinguished from primary sarcoma of the thymus by its microscopic appearances, the absence of any infiltration of adjacent parts, and the absence of any secondary growths.

It is not improbable that some cases recorded as lympho-sarcoma or sarcoma in the anterior mediastinum, without any evidence of metastases or infiltration of neighbouring parts, are in reality examples of hyperplasia of the thymus, especially when the tumour is described as retaining the normal outlines of the organ.

Microscopically the normal arrangement of the thymus is lost and the whole field obscured by small round cells, resembling the lymphocytes of a normal thymus. Hassall's corpuscles do not share in the hyperplasia, and are only seen in the remains of the normal thymus.

Acute inflammation starting in the thymus gland must be extremely rare. The gland must, however, necessarily suffer by extension in cases of pericarditis, pleurisy, and Ludwig's angina. Suppuration in the thymus has been seen in association with double pleurisy, but it is a curiosity. The neighbouring lymphatic glands may be attacked with acute or chronic adenitis in a variety of conditions, but this is to be distinguished from inflammation of the thymus itself. It is an interesting

question whether the rare disease, indurative mediastino-pericarditis, may not sometimes start in the thymus gland; I have seen one case in which the fibrosis was so excessive in the position of the thymus that clinically the case was thought to be a mediastinal growth.

Durante* has recorded extreme fibrosis of the thymus in a child aged twenty-seven days; its weight was only 1.5 grammes.

Hæmorrhages may occur into the thymus in common with other parts of the body in toxæmic conditions, in asphyxia, backward pressure in mitral disease, and sometimes in the newly-born as the result of difficult birth. The extravasations are commoner under the capsule than into the substance of the thymus. Durante has described two cases—one still-born, the other three days old—in which the thymus was the only organ into which hæmorrhage had occurred.

Tubercle very rarely affects the thymus; in generalised tuberculosis it may be affected like the rest of the body. One case of primary tubercle of the gland is on record.

In diphtheria commencing necrotic changes were observed by Jacobi† in two out of nine cases examined.

Syphilis.—In congenital syphilis small abscesses were formerly described, but it has since been thought that these were either due to post-mortem softening, or to physiological accumulations of lymphocytes. Jacobi refers to four cases, and lays stress on the marked fibrosis, one of his cases contained a small gumma. Gummata of any size have not been met with in the thymus, but it should be remembered that a periosteal node may develop on the posterior surface of the sternum, and so project into the anterior mediastinum that pressure symptoms suggesting mediastinal tumour may result.

Leukæmia.—Thymus may be enlarged in cases of lymphatic leukæmia so as to give rise during life to post-sternal dulness. In a case observed by Muir (Allbutt's 'System of Medicine,' vol. v, p. 643), in a woman aged twenty-five, the spleen and the thymus were both much enlarged, while the lymphatic glands were hardly affected.

Lymphadenomatous growths may occur in the

* Durante, Soc. de Biolog., Paris, March 14th, 1896.

† Jacobi, 'Trans. Assoc. American Physicians,' vol. iii, p. 299.

position of the thymus ; in the cases I have seen it appeared that it was probably due rather to the lymphatic glands of the anterior mediastinum being involved than to lymphadenoma of the thymus itself. But lymphadenoma is said by Letulle* to be the commonest new growth arising primarily in the thymus, and other authors record cases. Possibly the term has been used to designate cases of hyperplasia. If the growth in the thymus is associated with lymphadenoma elsewhere, the condition is rightly called lymphadenoma, but nothing is gained by speaking of enlargement and hyperplasia affecting the thymus alone as local lymphadenoma.

Primary tumours arising in the thymus gland.—

The thymus though not a foetal relic, inasmuch as it reaches its acme of development after birth, is yet on very much the same footing as those structures that atrophy after birth. Morbid lesions may occur in these foetal relics, and probably from the interest attaching to Cohnheim's theory of tumour formation from foetal inclusion, much attention has been paid to tumours arising in foetal structures and relics. To some extent this has been overdone, for the ductus arteriosus and ductus venosus are practically never the site of growths, while the vermiform appendix is very rarely the site of a primary new growth. The thymus gland, however, certainly supports the contention by being comparatively often the starting-point of a new growth. The fact that, according to Wilson Fox, mediastinal new growths are met with in the anterior mediastinum nearly twice as frequently as in the posterior mediastinum, may in part be due to growths originating in the thymus.

Before describing the forms of tumours arising primarily in the thymus it will simplify matters to glance at the development of the organ. In the first instance, the thymus and tonsil are both epithelial outgrowths from the alimentary canal, which become surrounded by leucocytes ; this secondary lymphoid infiltration becomes the important part, while the epithelial elements undergo atrophy. The thymus is at first a paired epithelial tube derived from the third, fourth, and partly from the second bronchial clefts ; it then undergoes ramification, and gives rise to condensation of the connec-

tive tissues around,* which then become infiltrated with leucocytes as soon as they are found in the body ; thus the more permanent part of the thymus is formed. The epithelial elements become changed into the concentric bodies known as Hassall's corpuscles.

It has been suggested that the thymus is a protective organ, providing phagocytes for the branchial arches in fishes, in which it is a permanent and well-developed organ. This view is of interest in connection with the early atrophy of the organ in man, an event which is otherwise rather difficult to explain. But if it be specially related to the very transitory existence of branchial clefts its disappearance in man is intelligible.

Innocent tumours, apart from hyperplasia of the gland already referred to, practically never occur in the thymus. It is quite conceivable that the epithelial cells, which normally degenerate into Hassall's concentric corpuscles, might proliferate and form an adenoma, but a simple adenoma has hardly ever been met with. Soupault† has described a unique case in a girl aged eighteen years, who died with exophthalmic goitre ; the thymus contained cysts lined with columnar epithelium. This he regarded as a developmental error. I have described a case in which a compound tumour of the thymus contained adenomatous growth resembling Lieberkühn's crypts, but sarcomatous tissue and cartilage were also present. Dermoid cysts occasionally occur in the anterior mediastinum. Hare, in his monograph on mediastinal disease (1889), refers to seven cases ; but they are not derived from the thymus gland, though they may displace it, and so occupy its anatomical position. Cysts derived from branchial clefts are not the dermoid cysts, inasmuch as they do not contain hair. Histologically these cysts, for which Shattock‡ proposes the title mucosal to distinguish them from true dermoid cysts, do not contain the stratum granulosum. Now a cyst lined by epithelium in the thymus would be derived from the thymus diverticulum, and as this comes off from the fore-gut, such a cyst, if it occurred, would come under the heading of a mucosal cyst, and would be homologous to a cyst formed from a branchial

* Gulland, 'Laboratory Reports,' Royal College of Physicians, Edinburgh, vol. iii, p. 172.

† Soupault, 'Bull. Soc. Anat.,' Paris, July, 1897.

‡ Shattock, 'Trans. Path. Soc.,' vol. xlviii, p. 254.

* Letulle, 'Archiv general die Medicine,' vol. clxvi, p. 641, 1890.

cleft. Hence a true dermoid cyst cannot develop from the elements of the thymus.

Practically the tumours of the thymus gland are structurally malignant, and may be considered under the usual heads of sarcoma and carcinoma. Before proceeding to describe them, however, it is interesting to note (1) that the sarcomatous tumours may be complex, and contain other forms of connective tissue, such as fat, fibrous tissue, smooth muscle, and exceptionally cartilage; (2) that epithelial proliferation may co-exist with sarcomatous growth, and (3) that in some instances something like a combination of sarcoma and carcinoma may be present in the same growth. In their complexity some thymus tumours resemble parotid growths, and it is interesting that they are in some way analogous in their embryonic origins, branchial clefts for parotid tumours, and the thymus diverticulum from the pharynx. In the irregular and somewhat atypical character sometimes displayed by the growths of the thymus a resemblance to neoplasms in the supra-renal and pituitary bodies, also ductless glands, may be traced.

Sarcoma starts on the fibrous framework or in the lymphoid tissue of the organ, while carcinoma arises from the remains of the thymus diverticulum from the fore-gut.

Sarcomata are commoner as primary tumours of the thymus than carcinomatous growths. Very various forms are met with. Small round-celled growths, the cells of which resemble the small lymphocytes normally found in the thymus, may occur. These, as shown by infiltration of adjacent parts and by secondary growths, are undoubtedly malignant. I have seen these growths to be so freely traversed by blood channels, not blood-vessels, since the boundaries of the vascular spaces were composed of the naked cells of the growth, that an alveolar arrangement resulted. The blood may break into the growth and form hæmorrhagic areas. It is this kind of tumour that may in very rare cases pulsate, and so make the diagnosis from aneurysm more difficult than it is already.

The round-celled growth may contain a varying amount of interstitial fibrous tissue. When the cells are small, and the fibrous tissue so delicate that it resembles the reticulum of a lymphatic tissue, the growth is called a lympho-sarcoma. It is to tumours of this histological structure that the

term lympho-sarcoma is strictly applicable, and not to growths thought to arise in lymphatic glands, which should be called lymphatic gland sarcoma.

The amount of fibrous tissue, on the other hand, in some cases of round-celled sarcoma arising in the thymus gland may be large, and the tumour then merits the title of fibro-sarcoma, or rather fibrifying sarcoma. The round cells may be of a medium or comparative large size.

Spindle-celled sarcoma may also occur, and may present a considerable amount of fibrous tissue, again justifying the term fibrifying sarcoma. A growth composed of variously and irregularly shaped cells, combined with considerable hæmorrhagic extravasation, may be met with, and perhaps is best described as an irregular-celled sarcoma; but the size and irregular shape of some of these cells recalls Dansac's * description of "voluminous cells" in thymus tumours, which he regards as derived from the epithelial elements of the thymus diverticulum.

Carcinoma arising in the thymus is characterised by its atypical structure, and in addition by the various forms that may be met with.

The cells composing the growth are generally somewhat large, and may resemble those seen in the Malpighian layer or stratum mucosum of the skin. Letulle mentions mucoid degeneration occurring in the cells, and in one specimen I examined the cells showed very marked vacuolation, and presented an appearance exactly like that which at the time (1893) was regarded by some writers as being evidence of the much-discussed intra-cellular parasites of cancer. In other cases of carcinoma epithelial pearls may be present, thus plainly indicating that the growth, like the normal concentric bodies of Hassall, is derived from the primitive thymus diverticulum from the fore-gut. The combination of polyhedral cells and squamous cells in the same tumour has also been observed. Sometimes the cells are large, and resemble the cells seen in adenomata of the supra-renal body or liver; these cells are not improbably a transition from a squamous to a spheroidal-celled type, and may correspond with the "voluminous" cells regarded by Dansac as derived from the epithelial elements in the normal thymus. Carcinoma of a squamous-celled type, and also of a transitional type between squamous and

* Dansac, 'Bull. Soc. Anat. Paris,' 1893, p. 199.

spheroidal-celled carcinoma, thus occur; and, in addition, a form in which the cells are spheroidal and small, somewhat resembling those seen in sarcoma, has been described. About the carcinomatous nature of the latter some doubt appears to me to be permissible. Thymus tumours may vary greatly in different areas, so that what appears to be carcinoma in one region is associated with sarcomatous tissue in another part of the same growth. It is possible to tumours of this character that the rather vague and non-committal term of "endothelioma" has been given. Growths of this kind suggest the possibility of combined sarcomatous and carcinomatous growths occurring in the thymus, just as they occasionally do in the kidney and elsewhere.

Secondary tumours are sometimes seen in the position of the thymus gland.

The *clinical aspects* of tumours arising in the thymus gland are those of growths in the anterior mediastinum; the growth tends to spread downwards over the anterior surface of the pericardium and upwards around the great vessels. It may invade the pericardium and give rise to pericarditis, with effusion sometimes of a hæmorrhagic character. Its most important manifestations are those due to pressure on the superior vena cava and innominate veins; the thin-walled veins are readily compressed and may be invaded by growth, which may thus extend towards and even pass into the right auricle of the heart. The veins of the head and neck become dilated, and cyanosis and œdema of the upper part of the body comes on. In some cases the development of a collateral circulation to some extent compensates for the obstruction. It is noteworthy that the aorta is seldom infiltrated or compressed by the growth, though the thinner walled pulmonary artery may be somewhat narrowed. Pleural effusion may occur; this, together with signs of venous obstruction, absence of accentuation of the second sound over the aorta, and of pulsation, are points in favour of a growth as against an aortic aneurysm in the anterior mediastinum.

In the CLINICAL JOURNAL, Vol. XII, p. 508, an abstract is credited to the *St. Louis Medical and Surgical Journal* which that journal credited to *La Semaine Médicale*; the original publication, on "The Use of Alcohol and Strychnine in the Treatment of Apparent Death in the New-born," was published by Dr. Bedford Brown in the *Therapeutic Gazette*.

A DEMONSTRATION OF DERMATOLOGICAL CASES AT CHARING CROSS HOSPITAL,

October 27th, 1898,

By JAMES GALLOWAY, M.A., M.D., F.R.C.P.,

Physician, Skin Department, Joint Lecturer on Practical Medicine, and Medical Tutor at the Hospital.

GENTLEMEN,—I have just returned after a period of absence, and therefore have not collected many cases for you to see, so I have taken advantage of the occasion to arrange a demonstration of these various fungi which have been talked about so much recently in connection with ringworm, so that you might have the opportunity of seeing them on a day when we have not so many cases to occupy our attention.

Erythema induratum.—There are one or two cases which I would like to show you, and this is one of the most important of them. Here is a woman (of thirty-five years of age) who suffers from lesions such as you see on her side; she has had them for three years now, and they have been steadily getting worse. Let me describe them to you. They occur on all parts of the body, and there are to be seen faint scars of them on her legs, about her ankles, on her arms, and on the shoulders and buttocks; indeed, they have practically occurred all over the body, without any very distinct choice of position. They commence simply as erythematous areas, and these areas soon begin to show a very considerable amount of œdema, the swelling sometimes being greater even than you observe at the present time. The œdema is followed by an increase of redness, and after lasting for three or four or sometimes six weeks, the most intensely red and œdematous part breaks down a little in the centre, it simply necroses without any obvious suppuration. There is no formation of abscess or discharge of pus except by accidental contamination. After this event the œdema seems partly to escape and partly to be absorbed, and the whole lesion gradually vanishes, leaving only one or two comparatively superficial scars. In this characteristic lesion the œdema is so great that there is an

exudation of serum from the surface. You can see the oozing now, and I must ask you to take my word for it that there will probably be no supuration in spite of its very "angry" appearance. Just as it reaches this stage, one or two of the inflamed looking spots simply show a certain amount of superficial necrosis, and all that will be left will be a very superficial scar. I show you several of these scars. I also want you to notice that there are numerous scars in the neck, no doubt produced by the suppuration of tuberculous lymphatic glands in youth. In addition please observe that the right elbow shows fibrous ankylosis, the result of old—probably tuberculous—arthritis.

I will now give you the diagnosis which I have made, and will then proceed to state my reasons for forming the opinion. I believe we have here, in a somewhat exaggerated form, an example of what has been called erythema induratum—by some it would have been called erythema induratum scrofulosorum, because it was supposed to have some relation to tubercular disease. It is also known as Bazin's disease, a name synonymous with erythema induratum. On looking at it superficially, I think the impression one is most likely to form is that it is probably a gumma on the point of caseating or necrosing. But when you consider that this process has been going on for three years, that there is no history of nor other evidence of syphilis, that there occurs only severe chronic œdema, that these lesions never caseate, that there is no formation of pus, that there is no central slough, but that in six or eight weeks the whole of this disappears, leaving only a little central scarring, you will see that at any rate the diagnosis of gumma is not a very easy one to arrive at under the circumstances. On the other hand, those of you who have watched cases of erythema induratum will be able to appreciate how very closely such a lesion as this corresponds with those more usual cases of the disease which have been described, where the extremities only have been affected. Let me mention the difficulties of considering the lesion as tubercular. These cases have been examined times without number for the tubercle bacillus, but always with failure. I believe one or two cases of disease of this type have been reported as showing tubercular structure, *i.e.* possessing "giant-cells" on microscopic examina-

tion, but it must be recollected that the "new formations" of all granulomata simulate each other to some extent, and the presence of giant-cells turns out not to be quite enough to allow of a definite diagnosis of tuberculosis. Dr. Audry, of Toulouse,* who has devoted much attention to this form of disease, has recently written an account of the histology of the disease, and founded on it a critical account of its ætiology. There was none of the structure of tuberculosis in his case at all, no giant-cells or other formation which we regard as histologically characteristic of tuberculosis. Inoculation experiments have been made by Dr. Audry and others on animals, but in no single case has the inoculation set up tuberculosis. The actual lesion of the disease is simply an intense œdema of the cutis with degeneration of the connective-tissue cells and widening of the lymph spaces. The amount of infiltration is almost *nil*, as there is scarcely any inflammation, attraction of leucocytes, or proliferation of mesoblastic cells. The hypothesis which is made as to the cause is that these cases of erythema induratum are due to some peculiar poison which circulates in the blood, causing a vaso-motor paralysis with constant distension of small vessels at some weak portion of the skin; that is to say, it is a disease somewhat analogous to Raynaud's disease, it belongs to that type of diseases—the group characterised by what has been called the "chilblain circulation." The actual origin of these poisons is also a matter of very great doubt. One rather important point seems to stand out in the consideration of these cases, and that is that many of the patients do undoubtedly suffer from tuberculosis or are liable to tuberculosis. They often show signs of old tuberculosis, as this patient does; therefore it has been considered by some that the substances which cause this very extraordinary vaso-motor paralysis, ending in partial necrosis of tissue, are toxins gathered up by the blood from old tubercular lesions, possibly in the mediastinum or in some situation where they are not readily recognised. A certain number of physicians hold to this view, and it is one which seems to have commended itself to Professor Boeck (Christiania) and others who were present at the British Medical Association meeting in Edinburgh, but that it

* Audry, 'Ann. de Dermat. et de Syph.' t. ix, p. 209, March, 1888.

does not account for all the cases must be obvious to those who have seen a sufficient number of examples of the disease. Probably the disease is nearly in line with other examples of chronic toxic indurative erythemata, such as the well-known erythema nodosum.

A difficulty in this particular case in the diagnosis—I do not think it is a difficulty of any magnitude—is that the lesion is not in the extremities, whereas in perhaps the majority of cases which have been described the lesion was in the extremities. I really do not see why a disease which occurs in the extremities should not occur in the body also. I believe, then, that this is a rare case of erythema induratum affecting the skin of the trunk.

In conclusion, I may say that the case, under the idea that it was syphilitic, was treated two or three years ago with iodide of potassium and mercury, with the result of making her very weak and depressing her very much, but having no effect whatever on the lesions.

I show you the next case, one of *purpuric erythema*, so that you may compare it with the one you have just seen. There is very distinct evidence of alteration in her digestive processes, and as a result of that we see these erythematous patches going on to purpura, especially in the lower extremities; she has it to a certain extent in the arms also. There is nothing else very important about the case; I wish to draw your attention to it simply by way of comparison. It shows the result on the skin of the absorption of abnormal digestive products—a true toxæmia.

Also, by way of comparison with the first case, I wish to show you a patient who has been demonstrated at these lectures before, who shows in a very mild way a similar condition to our first patient. If you look at the hands you will find that they have been scarred over, and in certain parts there is little normal skin remaining. What happens is, that in little areas there occurs first of all erythema, then œdema, and the central portions of these œdematous and erythematous areas slough, but there is no suppuration, simply necrosis from want of proper nutrition. She also has distinct signs of tuberculosis, there are tubercular glands in her neck. At one time I thought she was going to develop pulmonary tuberculosis, but fortunately that has not developed and apparently

is cured. Probably there will be a recrudescence of this erythematous lesion as the winter comes on. Cases such as this patient have attracted much attention of late.

Now just one word as to the treatment of erythema induratum. The treatment one has to adopt at the present time, as we know so little of its exact causation, is simply palliative. We must use every means in our power to strengthen the tissues of the body by general treatment. I have shown that the treatment by iodide of potassium and mercury only does harm by reducing the patient's strength, because there is no trace of syphilis as a cause. Good food, fresh air, and judicious exercise are the best remedies. Treatment by scraping, or destroying the lesions by any operative interference, is very bad, it only results in the condition spreading further, and healing is rendered very difficult because the tissues are so weak. One sometimes feels tempted to scrape them, but in the cases where that has been tried the condition has got worse, and much disfigurement has resulted. The parts should be kept clean, so as to prevent the introduction of pus-forming organisms and the occurrence of suppuration.

The next patient is a young girl aged about ten, who has been attending for some time. When she first came she was very ill and very pale, and I would have kept her in the hospital, but she insisted on going home, and went to bed. It is a case of *Hæmophilia*. The slightest injury produces bruises, and the marks left by the exercise of a small amount of force are altogether out of proportion to the severity of the injury. It is not uncommon to find bruises on her body as large as the palm of one's hand. I remember seeing a bruise occupy the whole of her forehead, but it could not be explained. On some occasions there has been serious bleeding. She has a loose tooth, and that has been bleeding for a week.

Ringworm.—The subject with which I wish specially to deal to-day is that of ringworm. A good deal has been written about it of late, and the subject is rather a complicated one on account of the discoveries which have been made from the pathological and mycological side. I happen to have a series of preparations which illustrate the new work on the subject, and will probably help you to grasp the significance of the new discoveries.

First of all I have a case of ringworm which I wish to show you. He came up to me last outpatient day, and I asked him to let the disease remain without treatment, so that you might see the condition of affairs. Here on the chin is a circinate erythematous area on the skin, and scattered over this patch are numerous pustules. Coming through some of these pustules a hair can be seen. Hairs can also be seen without any pustules. If you look more carefully, you will find these hairs are very little contorted or split up, and as it happens in this particular case, nothing like a sheath surrounding a hair can be seen. Nevertheless, from the position of it, his age—20 years,—and the actual pustular lesions, we can come to the conclusion that he is not suffering from the ordinary variety of ringworm, but from one of the fungi which is "at home" in the lower animals, the *Trichophyton ectothrix*, and probably infection has taken place not from a human being, but from some domestic animal. An actual hair from this patient is under one of the microscopes for you to see. Let me run over as shortly and as clearly as I can the recent work which has been done in connection with the ringworm fungus. I admit that to a person who has not been working at it, it is difficult to understand readily, and yet it is so important for you to recognise these cases from the point of view of diagnosis and prognosis, as well as treatment, that I think you cannot avoid keeping up a pretty thorough knowledge of the subject.

Up till quite recently, as you know, we were quite satisfied if we found a fungus of any sort, and made our diagnosis of ringworm. *Trichophyton tonsurans* was credited with causing all the cases of ringworm that ever occurred. But more than forty years ago certain physicians, with sharper eyes than others, began to notice that there were different clinical features in cases which were called ringworm, and the work which was done at that time, especially in France, was really the foundation for the work which has been re-done in the last six or eight years, but which had been completely neglected and lost sight of. *Trichophyton tonsurans* was thought to be the cause of all ringworms by most people; but others, among whom were some very distinguished physicians, refused to believe that ringworm was caused by a fungus at all. The whole matter has been

re-investigated quite recently, especially by such well-known observers as Sabourand in Paris, and by Colcott Fox, Blaxall, Leslie Roberts, and Adamson in this country, and by others in America, Germany, and Italy.

We know now that the great mass of cases of ringworm in this country are brought about by a very distinct and separate fungus from the rest; that is to say, something like 80 or 90 per cent. of cases of ringworm in this country are produced by this fungus, which I show you under the microscopes in various stages of development. It is so distinct from the other fungi that most pathologists and certainly most botanists consider it a separate species, and perhaps belonging to a different family. For this fungus has been revived the name which was given to it fifty years ago, namely, *Microsporum Audouini*. This fungus may be said to be the cause of all the cases of ringworm in children under twelve or fourteen years of age. Now let me mention the characteristics by which you will be able to recognise this fungus. Clinically it produces the rounded scaly patches on the scalp which you see in this child, in which the hairs are twisted and contorted in all directions, and in which also, if you look carefully, you will find a peculiar little scale or collar surrounding the root of the hair just as it leaves the follicle. But remember that the clinical characters of this form of ringworm are so interfered with by treatment by the patients themselves, that it is somewhat rare to find it in this natural condition. Patients wash it and cover it with iodine, ink, and other supposed remedies, so that when we see it, it is very difficult to make out its exact clinical appearance, and we are driven to a microscopical examination to find out the variety, for much depends on its recognition in the matter of diagnosis and treatment, and especially of prognosis. The manner in which the fungus invades the individual is as follows: first of all the fungus grows on the skin, where it produces a certain amount of irritation and desquamation of epidermis, and causes a greyish white scale, which is characteristic of the disease. Finally it reaches the mouth of the hair follicle, and then proceeds to grow down the hair follicle. It does not grow right to the end. You will recollect that a discussion has been taking place about the fungus growing down to the foot of the hair follicle, and then going up

the hair. That is not the way in which it grows. The fungus disturbs the scales of the hair-cuticle and perforating between them grows into the medulla of the hair itself. For a certain time it seems to content itself with growing in the medulla in the form of filaments with very irregular dissepiments; and there, probably on account of the air spaces in which the fungus grows, it forms rather larger threads and "spores" than one sees finally. As it begins to be crowded, and probably the nourishment becomes less, it divides into smaller sections or so-called spores. These spores produce smaller filaments also, and finally the whole of the cuticle and the hair is thrown off, and the hair is surrounded with a bark-like cortex of fungus, as you see in this illustration. It scarcely ever invades the root, or if it does, only very slightly. The peculiar fringe-like termination which it has near the root of the hair is quite characteristic. But the most characteristic point of all is the peculiar bark-like covering; it is that which produces the collar or scale so characteristic of the disease. You will find this collar just as the hair leaves the follicle. The hair continues to grow a little, and as it grows it pushes up the sheath of the fungus, which is the so-called collar which surrounds the hairs. If you look at these diagrams you will be able to see the state of affairs.

I have here a culture of this fungus, and you will see that the characteristics of it are as follows: that it produces a peculiar white cotton-wool like growth, and this growth tends to radiate from the centre of inoculation; at the centre a small elevation is usually produced. This method of growth is very characteristic.

I wish to say that these beautiful drawings and cultures are lent to me by Dr. Colcott Fox for our use this afternoon. I will not go further into the details of the growth of this fungus except to say that the botanical characters are very distinct, the methods of fructification are completely different from other forms of ringworm fungi, and the general impression is that it is not only a separate species, but that it probably belongs to a different family altogether of mould fungi.

There are one or two important characteristics about it clinically. It is what may be called a human ringworm; that is to say, it is especially parasitic on the human body, usually on the scalp, but also on the trunk, and it is very much more

difficult to kill than the majority of others, and it grows much more easily than the other kinds. The proportion of the cases of ringworm produced by this fungus varies in different countries. In Paris it does not seem to account for quite so many of the cases as in England. Sabourand gives 80 per cent., Adamson gives 85 per cent. for this country, Mr. Pernet in a recent investigation gives a still higher proportion; therefore we may say that between 80 and 90 per cent. of the ringworms in Western Europe are produced by this microsporon. But it is very different in Italy and Germany, for there other fungi are predominant. Therefore in this country and in France there still remains 15 per cent. or 20 per cent. of the cases of ringworm to be accounted for, and the question arises, do we know anything about the ringworm fungi which cause the balance of cases? Fortunately of late a good deal of knowledge about them has been obtained, and I will describe to you very generally what that knowledge is.

These fungi are called specially the Trichophyta, and two main groups are distinguished according to the position of the fungus in relation to the hair, viz. the *T. endothrix* and the *T. ectothrix*.

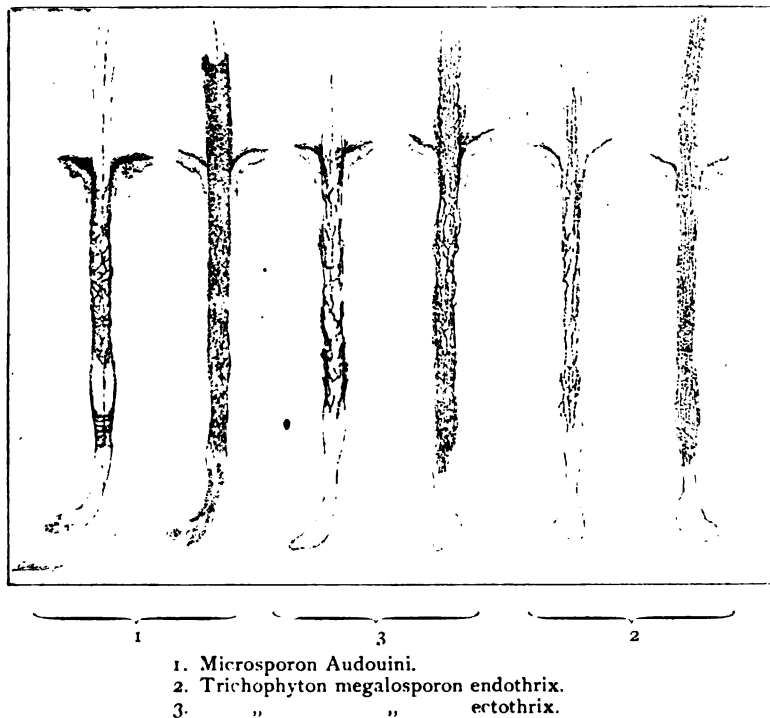
Clinically the *Endothrix* fungus produces a totally different form of ringworm from the type I have just mentioned. It produces those peculiar varieties of ringworm which simulate alopecia areata, in which there is not much scaliness, and in which smooth patches are caused early in the disease with little black stumpy hairs scattered over them. It also produces a ringworm with numerous small points of infection in the scalp,—little groups of three or four hairs affected. It also sometimes, but not very commonly, produces tinea circinata.

Now let me describe how this fungus grows. Infection takes place from some source. It begins to grow on the scalp first of all, but it does not grow very vigorously there. It comes in time to the hair follicle, and proceeds to grow down into it, winding round the hair, perforating its cuticle, and so gets into the medulla. There this fungus produces great chains of growth, the filament being separated into regular blocks. In the microsporon the joints of the mycelium can scarcely be seen. This fungus then fills up the whole inside of the hair, and that is why it is called the endothrix. Curiously enough it does not continue to grow on the outside of the hair, but the inside

of the hair is filled with mycelium. It is different, therefore, not only in the size of the spores, which are generally larger, but especially in its mode of growth. Instead of producing the collar or sheath which is characteristic of the microsporon, it produces a sac full of spores. You will notice the culture is very different also. Instead of producing a growth with radiations from the centre, it produces this peculiar *crateriform* culture, which, when fully developed, shows a great mass of fungus with a central depression. The specimen I

ducted, probably because there are several species grouped under this particular name ectothrix. Cases of the variety of ringworm produced by this fungus probably originate from some animal suffering from ringworm of the ectothrix type. In the human subject these cases show some little variations, and that has caused the confusion to be even greater. Let me describe the usual results of infection. Infection takes place in the same way as in the other two kinds. The first thing that takes place in the growth is the production

DIAGRAM ILLUSTRATING THE THREE CHIEF RINGWORM FUNGI.



have here is a very beautiful example; it almost looks as if it were artificially made.

The next group of fungi, which are called, for shortness *Ectothrix fungi*, are very much more closely related to the ones I have just described than to the microsporon. It receives also the name of trichophyton. I must say, however, that up to the present the study of this group has been less prolific of definite results than with regard to the others. They are very much more difficult to study, and there seems to be great confusion in the results of investigations which have been con-

of a sheath of fungus between the follicle and the hair. Later on it is true that it grows down towards the softer parts of hair and perforates as the others do. It then begins to fill up the hair, though perhaps not so fully as in the case of the endothrix. In some cases the ectothrix growth seems to occur in irregular masses outside the hair, the inside of the hair being but little infected. Thus, though the name ectothrix is given to this form, it does not mean that it does not grow inside the hair at all, but it is so named in contra-distinction to the form in which the outside of the

hair is not affected. Notice how regular are the chains which this fungus forms; perhaps the ectothrix chains are the most beautiful of any we are in the habit of seeing. The particular case we see here is one of ectothrix invasion, probably from a dog. Of course there is the possibility that an ectothrix invasion may spread from one human being to another, but the probability is that it has originated from a domestic animal in the first instance. Therefore, when we see a case of this sort, the domestic animals in the house should be suspected as the source of infection. There was a very interesting case described by Dr. Fox recently, in which a ringworm infection of this type took place in a family, and the source of the trouble could not be traced.* It eventually transpired that a cat was buried in the garden which had suffered from skin disease. The cat was exhumed, was proved to have had ringworm, and a child had caught it from the animal.

Clinically this form of fungus is characterised by its tendency to produce the markedly circinate varieties of the disease, especially in adults, by its affecting the trunk, extremities, and beard rather than the scalp, and especially by its liability to produce suppurating lesions, such as the well-known agminate folliculitis of French authors.

In its culture the main point of differentiation, as far as the naked eye is concerned, is that it is apt to form a great mound in the centre of the growth instead of producing a crateriform lesion. The main distinctions appear on microscopical examination; its fructification and manner of growth are very distinct.

Now, before I ask you to look at the microscopical specimens, I will say a word about the *diagnosis and treatment* of these affections. Remember that the large amount of ringworm in London and in this country generally, arises from the cases of ringworm which have been under treatment and which have been sent away half cured. Every one who reads the medical papers comes across statements of new cure for ringworms. Sometimes the cure is salt and water, sometimes some other remarkable remedy, and the enthusiastic practitioner declares that in four or five weeks cases of ringworm are completely cured by the remedy. Now there is nothing

easier than curing a surface infection of even the most inveterate of these ringworms, even the *microsporon Audouini*. Almost anything will do that,—perchloride of mercury, carbolic acid, or any antiseptic,—and after a period of careful treatment the skin looks very nice indeed, and as if the case were cured. These are the cases which are sent away. But can you imagine that these ringworms which have grown deep into the scalp can be affected by these applications? They are not cured. These cases all recur when sent back to school, let us say, and thus we have epidemics of ringworm. Every one who is in the habit of seeing ringworm cases knows that the large schools are very commonly infected; when cases are discovered and apply for treatment, many of them are sent away half cured. At this hospital we see large numbers of ringworm cases, and the history I have given is constantly repeated to me. Therefore it is very incumbent upon us all to be sure that our ringworm cases are cured; and we can be certain of this only after treatment and careful microscopical examination. No case of ringworm should be sent back to school without *repeated* microscopical examination of the hairs, in order to satisfy the practitioner that the case is indeed cured. The patch which has been affected should be carefully looked at, and any suspicious hair removed and examined. It is a comparatively easy matter to do so, and a microscopical examination without any staining is sufficient to make the diagnosis. The specimens I shall show you are of course stained so that you may see the arrangement of the fungus easily.

You will naturally ask what is the way in which I treat these cases? You are aware that the methods of treatment are very numerous indeed. In the case of children, unless there are only one or two points of infection, the first thing we do is to have the scalp shaved. If there are only one or two points, and you are prepared to take a great deal of trouble, you can perhaps manage without shaving off all the hair. It may be a pity that the head must be shaved, especially in the case of girls, but it cannot be helped. We next give the patient directions that every night regularly, or sometimes twice a day, the head must be carefully washed with hot water and soap. After that is done I generally use as the first method of treatment the compound carbolic acid ointment of the

* 'Brit. Journ. of Dermat.,' July, 1898.

London hospitals. A new formula* has been suggested by our dispenser at the Great Northern Hospital, Mr. Skinner, which is very much better than any of those I have used up to the present time, and better than any I know of in any London hospital pharmacopœia. It consists of—

Olive oil, pure phenol, white wax,

āā 1 oz.

Nitrate of mercury ointment ... 2 „

Sublimed sulphur ... ½ „

This ointment keeps its colour very much better than the others, and it is altogether a very much nicer preparation. Mr. Skinner advises that the phenol should be dissolved in the olive oil, and then the white wax added, heat being continued till liquefaction. When it is cool the mercurial ointment and sulphur are added, using a vulcanite spatula. The ointment keeps well for three months. This rapidly gets rid of the surface ringworm. When the scalp is shaved you can see the deep points of infection by ringworm very well. Now what are you going to do with the deep infection? The only way to get rid of that quickly is by producing a sufficient amount of inflammation of the scalp to loosen the hairs, so that they either fall out of themselves or are easy to depilate. If I can see the patients regularly myself, which is not always easy, I use a modification of Aldersmith's well-known croton oil treatment,—a solution of croton oil, 10, 15, or 20 minims to the ounce of paraffin as an ointment. I give directions for this to be used until a certain amount of œdema of the scalp is produced. Then when the hairs are sufficiently loose they must be pulled out one by one. Of course if you use the carbolic acid ointment or other remedies you may cure your case by a similar process, but it will probably take six or nine months. But you must yourselves undertake the croton oil treatment. The risk of cases going wrong is too great if this treatment is undertaken or left in the hands of uninstructed persons. A case of *Microsporon Audouini* infection came here two years ago, and as the mother was an intelligent woman I thought that after I got rid of the surface infection in the ordinary way I would give her directions to use the croton oil ointment, which contained 10 to 15 minims to the ounce. I did it myself for the first

two or three times, and then asked the mother to continue it. We cured one or two spots, and everything seemed to be getting on nicely. But he was a very troublesome young man, with plenty of energy. He found that if he made a tremendous disturbance at night he could get his mother to abstain from rubbing in the ointment. One evening he was kicking and screaming when his father came in. The father was also an energetic person, and thought he would stop this game, so he got the boy between his knees and rubbed the scalp well with the ointment. The result was one of the most extreme cases of suppurating folliculitis of the scalp I have ever seen. Of course I was not prepared for this sudden and vigorous application of my remedy. A very furious kerion was produced, and I was alarmed at one time that more deep-seated suppuration might ensue. The result, however, was that his ringworm was very much more quickly cured than it would have been under ordinary circumstances; and when I saw the boy last the hair had grown well over the area of disease, which could be detected only with difficulty. The point of the matter is that if you order strong applications you must oversee their application yourselves. Chrysarobin is also a very excellent remedy, but if it is used strong enough to produce œdema it is necessary to carry out the treatment, and epilation carefully, and to personally superintend its application. Formalin is also an excellent remedy, but its use must be similarly safeguarded.

Symphysiotomy.—Edward P. Adams ('Cinc. Lancet-Clinic') reports a case terminating in perfect immobility. The patient was kept in bed seven weeks; the rubber adhesive bandage was not removed until the end of the sixth week, when it was replaced by a new one. At the end of the tenth week there was slight motion visible in walking, but the gait was normal. In three months there was no motion whatsoever. He attributes the success of the perfect union to the long time the patient was kept in bed without removal of the rubber adhesive bandage.

* 'Brit. Journ. of Dermat.,' Oct., 1898, p. 358.

DIAGNOSIS OF MAMMARY ABSCESS.

It may be thought that the ordinary diagnostic symptoms of acute mammary inflammation and abscess are so evident, that mistake would be out of the question. On some exceptional occasions a diagnostic difficulty arises in these cases which is of very great import, namely, the confusion between abscess and acutely inflamed, rapidly growing carcinoma or sarcoma. This is especially likely to occur if the growth be breaking down in the centre, or if it be of the nature of an inflamed carcinomatous cyst. Precisely the same diagnostic difficulties are found in suppuration in connection with carcinomatous glands of the neck, or suppurating epitheliomatous cysts in this locality. If an incision is made in this type of case, blood and broken-down debris escape, but seldom any pus. Fungation follows, with its attendant drain of bleeding and discharge, and the termination of such a case, where the diagnosis of abscess has been confidently given, may be very disastrous to the reputation of the surgeon who has committed the error.

The symptoms and diagnosis of chronic mammary abscess have long been notorious for their uncertainty. There are probably few surgical disorders which have been associated with more deplorable errors. The reader will do well to bear in mind—(1) That fluctuation may be quite absent in deeply-seated chronic abscess, and local heat or general fever may be wanting. (2) That the leading signs of such deceptive accumulations of pus are mainly as follows:—(a) There is often a soreness or abrasion of the nipple to be found on careful inspection. (b) Firm pressure with the pulp of the index finger upon the centre of the swelling usually gives a sensation of yielding or elasticity; when the finger is removed slight œdema may be observed. Pain of a throbbing nature is not uncommon. (c) Exploratory incision is the only certain diagnostic means of discovering a chronic abscess of the breast when deeply seated.

The preceding remarks are taken from Mr. Marmaduke Sheild's latest addition to medical literature—his recently issued volume entitled '*A Clinical Treatise on Diseases of the Breast.*'*

* '*A Clinical Treatise on Diseases of the Breast,*' by A. Marmaduke Sheild, M.B.Cantab., F.R.C.S.Eng., Senior Assistant Surgeon and Lecturer on Practical Surgery to St. George's Hospital, late Assistant Surgeon, Aural

This book is an excellently written treatise on the very difficult subject of the diseases of the breast, and forms the most comprehensive, up-to-date, and authoritative publication on the subject that the medical profession has at its disposal. The work can be confidently referred to for practical help and guidance in all matters connected with diseases of the breast, from treatment of a cracked nipple to the latest opinion on the question of the after dangers and complications of removal of the breast.

We have received from Messrs. Wright and Co., of Bristol, a specimen of a new "case paper," suggested by Drs. Couch and Lancaster. The special feature of interest in this design is found in a very useful series of diagrams, which most certainly offer not only "some advantages," as is modestly claimed, but also supply a very ready means of noting clinical facts which might otherwise be left unrecorded in the course of a medical examination.

The Effects of Mercury on the Kidneys.—

Basing his deductions upon sixteen personal experiments upon animals regarding the action of mercury upon the kidneys, and upon a very careful critical study of the clinical facts published in the literature, J. J. Karvonen ('*Journal of Cutaneous and Genito-Urinary Diseases,*' November, 1898) gives a complete picture of the action of mercury on the renal filter, considering critically every symptom produced by it clinically and histologically. Some points upon which the author insists when he deals with the therapy of kidney diseases due to mercury are worth mentioning. Opium must not be used in diarrhœa due to mercury. The bowels are to be kept open by means of oil, castor oil, and especially by washing the intestines. Atropine is sometimes of use, as the action of the heart and diuresis are increased. He lays special stress on the two facts that mercury must not be administered before the condition of the kidneys is looked into, and the gingivitis is not always the first symptom of mercurial poisoning, as the irritation of the kidneys may sometimes reach a high degree while the gums are entirely healthy-looking.

Medical Age, December 10th, 1898.

Surgeon, and Lecturer on Operative Surgery to Charing Cross Hospital, &c. (London: Macmillan and Co., 1898. 8vo, pp. 510. Price, 15s. net.)

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* Specially reported for The Clinical Journal. Revised by the Author.

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NOTICE.

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A CLINICAL LECTURE

Delivered at Charing Cross Hospital, December 1st, 1898,

BY

F. C. WALLIS, B.A., M.B., B.C., F.R.C.S.,

Assistant Surgeon and Lecturer on Surgical Pathology and Minor Surgery at the Hospital, and Assistant Surgeon, St. Mark's Hospital.

I THOUGHT, as I happened to be on the staff of St. Mark's Hospital, and have considerable experience in the treatment of diseases of the rectum, I might be able to give you some suggestions with regard to the relief of some of these troublesome affections which may occur in your various practices. I find that there is no class of patients more grateful for benefits received, so to speak, than those who are suffering from complaints in this particular portion of their anatomy, and especially from those affections which I shall consider for a few minutes this afternoon, for they cause a great amount of pain, irritation, and chronic discomfort, and are very difficult to cure. I have also three cases in the wards which I shall ask you to look at with me subsequently.

I propose to speak of pruritus ani, fissure, and simple ulcer. The reason I have taken these three conditions together is that I find on investigation of a series of cases which I have had under my observation during the last two or three years, that it is very common to find that the first, that is pruritus, is dependent upon one of the other two.

Let us first take pruritus. We have constitutional causes for it, and also local causes. With regard to the constitutional causes there are one or two things to be said. It is very often the case that a condition of pruritus ani is kept up by some slight local cause, to which is superadded a constitutional condition which keeps up the irritation. Given that there is some such local cause, and that the patient is gouty, or rheumatic, or even neurotic, whatever that may mean really (though we know

among ourselves what we refer to), we find that that is enough to keep up this very distressing discomfort. Among other causes I find that a very common one is the presence of the ordinary threadworm, and I should say that at least 12 per cent. of the cases are due to this alone. Of course the treatment here is obvious, and well known to everybody.

But it is to the local causes that I wish to specially direct your attention, and particularly to one cause which, for some reason, had not been much noticed until I wrote a short paper upon it. The reason for my search for the cause to which I refer was that perhaps thirty cases were hanging on in my out-patient department at St. Mark's Hospital, in spite of every form of treatment which we knew of; they persisted in this miserable plight without amelioration. It therefore seemed to me time to take the matter in hand, and investigate for some real reason for the perseverance of the symptoms. I found that in at least 25 per cent. of the cases there was between the two sphincters (the external and internal) an ulcer, small and superficial, in the mid-line posteriorly between the two sphincters. I have called it "simple ulcer of the rectum." I believe it to be a more common cause than any other for the condition known as chronic pruritus ani. Another situation for it, but not nearly such a common one, is on the *anterior* surface between the two sphincters. When you are at a loss to account for any case of pruritus ani I would really emphasise the advice to look for this little ulcer. You will need to look carefully, because although it is not high up the bowel it is not always an easy matter to detect it. Many people in examining the rectum pass their finger as high up the bowel as possible, whereas those who are familiar with diseases of the lower bowel know that most of the disorders are situated in the first two inches from the anus, or, to be more exact, between the internal and external sphincters. Of course, in saying this I am not referring to new growths. How do you recognise the ulcer? That also is a matter of experience, but one very soon learns. Every one knows the ordinary feeling that smooth membrane gives to the finger. In feeling with the finger in this way you will find it will, in these cases, come upon a surface which is perceptibly rougher than the rest, quite distinct from the smooth glistening feeling

produced by the normal bowel. That is an ulcer; it is not deep, even though it may have lasted for many months, and it never seems to increase much in size. A certain amount of discharge comes from it, and that discharge lodges in the parts around and causes definite irritation, and that irritation is, of course, met by the individual scratching at the parts, and so one thing reacts upon another, and the state of affairs by this process goes from bad to worse, in spite of various forms of treatment, because the local lesion is not discovered and treated. I will not say anything more about the ulcer except that if you want to make doubly sure as to its presence you can always see it by passing an ordinary rectal speculum, which opens in a boat-shape after it has been inserted into the anus, using an electric lamp or even a candle. But remember this: when you push the speculum into the bowel you push the tissues higher up than they are normally, so you must allow for this displacement when searching for the ulcer. Moreover from the same cause—the tissues being on the stretch—the base of the ulcer bulges through the surrounding area. Another means of recognition is that it is livid in colour as compared with the pinkish colour of healthy mucous membrane on either side, and it is a little prone to bleed. You may think I have said a good deal about this simple ulcer, but only those who have seen many of these cases and the misery of the patients, and therefore the burden that they are to themselves and all about them, can appreciate how great is the relief afforded by treating these little lesions. Of course, an ordinary fissure will give rise to pruritus ani in addition to the symptoms of the fissure itself; indeed, a fissure and a simple ulcer are practically almost the same thing.

I will now go on to the treatment of this particular kind of pruritus ani. Of course, generally speaking, you will adopt constitutional methods; and when you find there is a local cause you will treat that also. With regard to the constitutional measures, given that you find no other cause for your ulcer—that is to say, no local cause—you may then be fairly certain that the reason for the local disturbance is a constitutional one, and will treat it as such. I will read you two instances which I quoted in the paper to which I have already alluded. The one—a patient who was a most careful liver—came to see me about a condition of

quite bad pruritus. Knowing him to be careful, I could not find out any dietetic indiscretion until, when dining with him, I noticed he drank a quantity (three tumblerfuls) of ordinary claret, and I elicited from him that he usually took almost as much at luncheon. This was discontinued, and the trouble disappeared in a very short time. With regard to this patient, I had treated him with all sorts of local remedies, and done everything I could for him with the exception of altering this particular diet. After altering this the whole trouble disappeared in a week.

The other case is somewhat similar, in that the patient, who was gouty, indulged in an imperial pint of champagne *every* night at dinner. After much persuasion this was discontinued, and a rapid cure was the result. Coffee, tea, tobacco, various forms of shellfish, &c., are all possible causes of pruritus. I would lay particular stress on cigarette smoking as a causal factor in the condition. Of course, as you probably know better than I do, it is one thing to find out what a patient is doing that he ought not to do, but it is quite another thing to get him to give it up, because you will almost invariably find that that one thing is that which he finds the hardest to relinquish. I have only mentioned these matters to refresh your minds with regard to the possibility of such indiscretions being the cause of the pruritus and subsequent discomfort.

Now as to the local condition. When you see a condition of pruritis ani well marked you will notice that the skin all round the margin of the anus is very much discoloured. In recent cases it is red and much torn by constant scratching, and there is a watery serous exudation mingled with the condition, setting up a local eczema. In chronic conditions—that is to say, where the pruritus ani has been going on for some years—the skin undergoes a marked change; it is found in those cases to be of a dead white colour, having lost all its properties as skin, and assumed more the appearance of parchment. A great deal of stress has been laid upon this state because it is said to be a marked feature of pruritus. As a matter of fact, it is not. Wherever chronic inflammation of skin occurs, there you will have deposits of inflammatory products, and fibrous tissue will be interspersed amongst the ordinary tissue, so that eventually the skin in that part is fibrous tissue with ordinary

epidermis over it. In treating this condition the first thing is plentiful ablution. Amongst ordinary hospital out-patients washing, as a rule, is a luxury, and local washing of this particular part is practically unknown. Therefore one of the instructions, which I have printed in bold type, and with which I impress the patients, is this washing of the parts night and morning, and always after the bowels have acted.

I also instruct these patients to get a double handful of bran, put it into a gauze bag, and place it in a hip-bath, the patient getting into it as warm as he can bear it ten minutes after the bran has been put in. He should then sit in it with his buttocks divided as wide as he can, sitting on the gauze bag for fifteen to twenty minutes. On getting out of the bath he thoroughly dries the parts, and dusts them over with a powder consisting of equal parts of zinc oxide, boric powder, and starch. Then a fairly firm pad of cotton wool should be applied closely by means of a T-bandage, and should be worn at night-time. This reduces the amount of external irritation; and if there is no ulcer or fissure inside, it will go a long way towards cure. If there be a simple ulcer such as I have described, you will not cure the pruritus unless you cure the ulcer. The cure of these small ulcers is one of the most difficult things I have had to effect in surgery. Of course if one could take all these people in and let them lie up, and stretch their sphincters and scrape the margin of the ulcer, taking precautions of cleanliness night and morning, and after every action of the bowels, they would soon get well. I take care to impress upon these patients that the condition is very difficult to cure, but that if it does not give way to one course of treatment it may to another, and they must be prepared to give themselves up entirely to the treatment, and not to be disheartened. If one can get the patient into that state of mind the task is rendered much easier; but as a rule they go from one to another, and do not stay with any long enough to get any great benefit.

The method I have adopted with these ulcers is to take the patient in when I can, stretch the sphincter well, scrape the margin of the ulcer with a spoon, and sometimes to treat it as I would a fissure; that is to say, divide right through the external sphincter and the base of the ulcer, and continue the incision backwards for an inch outside

the anal margin. It gives a free drain to everything, and relieves the whole of the tension to these parts, and allows them to granulate up from the bottom. Under these circumstances the patient does get well, and thoroughly well; he gets quit of the whole thing, and is a different individual. But if he cannot lie up what are you to do? I have adopted the following, though it is not so effective as the course I have just described. I make the patient come to me twice or thrice a week. Then a speculum is passed into the bowel, and a 4 per cent. solution of eucaine is injected or washed over the part so as to render it fairly anæsthetic; then some pure lactic acid is applied on some cotton wool, and the lactic acid is smeared round the ulcer. The surface of the ulcer then turns a brownish-black colour owing to the action of the acid. Having done this, any superfluity of the acid is wiped away with dry cotton wool. The speculum is now withdrawn, and the procedure is finished. The patient himself washes his rectum out, and injects a couple of ounces of fluid, such as a weak boric acid solution, night and morning, and after the bowels have acted. One of the things I am very fond of using as a lotion is a form of peroxide of hydrogen, which goes by the name of "oxydal." This substance applied for a time is very useful. I do not know anything so satisfactory to apply or so beneficial to the patient. I would say that whenever you cure a case of pruritus ani you will have the satisfaction of knowing you have relieved not only the patient, but sometimes his domestic surroundings as well.

Next as to fissure: everybody knows that fissure is a painful ulcer which is between the two sphincters, and according as it invades the external sphincter so the pain increases, and so the symptoms become urgent. The fissure is a continuation downwards of the ulcer. The typical symptoms of the fissure are that the patient has intense pain, varying in degree according to the situation and depth of the fissure, after the bowels have acted, continued it may be for half an hour, or six or seven hours afterwards. The typical condition is that the ulcer has at the end of it a small polypoid tag, which is called Van Buren's sentinel pile, though in reality it is not a pile at all. If you pull this down with the forceps you will see the narrow surface of the ulcer; and if the patient allows you to pass your finger to the

other extremity you will find another sentinel pile at the other end. The anatomical cause of that is that there are certain reduplicated folds of mucous membrane in the bowel called Houston's valves. One of these gets torn down for some reason, and gradually as the contents of the rectum are forced out they press against the valve and tear it; next time it gets torn a little more, and so gradually the fissure is produced. This is the one condition for which patients come to me and urge me to operate upon them, so great is their agony. The remedy is one of the simplest in the world. This small pile is removed, the sphincter is well stretched, and then the knife incises right through the base of the ulcer and also through a portion of the external sphincter. It is not necessary to divide it all, although I do not know that it matters: I generally divide most of it. I dress the wound with $\frac{1}{500}$ perchloride and keep the patient's bowels relaxed. Here again the relief given to these patients is wonderful. I always enforce the lesson that it is a very serious matter to neglect to do an operation for fissure. I had two cases brought to my notice which I have quoted in the paper I have written, and which I will read to you, in the hope that they will be useful in causing you to act without temporising in any of your cases.

E. G—, a girl æt. 18, came to my out-patient department at St. Mark's, on September 16th, 1896. She had pruritus ani and a fissure, for which she was given some medicine and ointment, and her name was put down to come in as soon as possible. She attended once after that, and then her mother came up asking for medicine, saying the girl was too ill to move. On inquiry it was considered advisable to get the patient to the hospital, and she was brought in a cab in the course of an hour. It was at once evident that the girl was extremely ill. The temperature was 103°, pulse quick, tongue furred, and she had an anxious expression. On examination a large bilateral ischio-rectal abscess was found with wide-spread inflammation of skin. Pus of the most foul odour escaped from the bowel. The abscess was opened on one side, and the tension relieved there and then, and the girl came into the hospital, where, two days later, the abscess was very freely opened, and a complete rectal examination made under an anæsthetic. A large internal opening

was discovered well above the internal sphincter. The patient was discharged on November 3rd, 1896; readmitted February, 1897, for a fistula—the remains of the large abscess,—and was discharged cured on April 3rd, 1897, the whole treatment extending over a period of six months.

The other case was a man *æt.* 49, who attended the out-patients' department for a fissure for about three weeks previous to his admission on January 20th, 1897, when he came to the out-patients' department with an enormous ischio-rectal abscess, which was at once freely opened; a sinus ran up alongside the bowel for nearly six inches. The patient was sent to the wards. The temperature on admission was 101.6° , but was normal next day. On February 26th the temperature began to rise, until March 1st, when it reached 103° . On this date a collection of pus at the upper end of the sinus burst into the rectum. The temperature came down gradually to normal. On March 8th I operated on him, laying the whole sinus and track freely open from the outside. There was an internal opening high up. The wound was packed for a few days, and then fomented. From now onwards the man made a steady recovery, the internal opening healing up with a gradual contraction of the wide external operation wound. He was discharged well on June 5th, 1897, having been in hospital four and a half months, and has remained well ever since.

These two cases will illustrate that there is a serious danger, over and above that of the daily pain, in allowing these fissures to go unrelieved. On account of the pain which follows the daily evacuation, patients put off the evil moment as long as possible. The lower bowel becomes loaded with most septic material, which starts by infecting the fissure, and so getting into the sub-cutaneous tissue. Very soon the ischio-rectal fossa falls a victim, and here, owing to the peculiar conformation of the fossa, a large amount of pus may accumulate before any signs of tension make the patient feel that it is necessary to "see some one." By the time that point is arrived at, the whole of one or both fossæ may be tense pus sacs, and, as happened in one of the cases related above, the bowel may give way high up. In the case quoted the internal opening closed, but it is not the fortune of most cases to have such a good result.

DEMONSTRATION OF CASES

AT THE

NORTH-WEST LONDON CLINICAL SOCIETY,

Dr. MACEVOY in the Chair.

A Case of Coxa Vara, by Dr. G. A. Sutherland.

DR. G. A. SUTHERLAND showed a case of coxa vara, or bending of the neck of the femur. The patient was a girl aged 7 years, in whom the condition was bilateral. She was brought to the hospital at the age of two years because she had never learned to stand or walk, and the mother thought her spine was weak. At this time the signs of active rickets were well marked, but certain conditions of the lower extremities were present which were not characteristic of that affection. When the child lay on her back the lower limbs were flexed at the knees, and the feet rested on their outer edges with the soles parallel—an attitude aptly compared to that of a pithed frog. In this position, without any inconvenience to the child, the outer surfaces of the knees rested on the couch, so that with the lower limbs strongly everted and flexed, the buttocks, the knees, and the feet were in the same plane. This is not a position which can be assumed easily by a healthy or a rachitic child. Adduction of the limbs was normal, but abduction was extremely limited, and eversion was so marked that it required considerable force to point the toes upwards. It was further found that flexion at the hip-joints was so free that the toes could easily be raised to the mouth and ears, and the mother observed that the child usually slept with her feet resting on the chest (Fig. 1). Although unable to stand, the patient could crawl along the floor, or even upstairs, and the movements of progression were extremely suggestive of those of a frog. She was put on antirachitic treatment, which was carried out for some months, and then the patient was lost sight of.

Two years later, at the age of four, she was again seen. The art of walking had been acquired, but from an artistic point of view was

not a success. The patient stood with both feet markedly everted, the toes pointing almost directly outwards, and moved with a shuffling gait—the feet being kept everted—and a marked pelvic roll. This gait, accompanied as it was by marked lordosis and prominence of the abdomen, was strongly suggestive of congenital dislocation of the hip, but there was no evidence that the head of the femur was not in its normal position. Definite rachitic changes were manifested in bending of the shafts of the femora and tibiae, in displacement outwards of the patellae, with a tendency to knock-

the trochanter rose to the level of the head of the bone, or even above it. The patient having been seen by Mr. Watson Cheyne, he advised operation with the view of correcting the eversion of the lower limbs, and thus improving the gait. This was carried out by Mr. Cheyne by a method similar to that employed by him in another case of coxa vara.* The left femur having been divided at the junction of the upper and middle thirds, a small plate of aluminium was screwed into the upper fragment, and then, the limb having been fully inverted, into the lower fragment, a long



Fig. 1.

knee, and in flat feet. But these, in the absence of any muscular or nervous lesion, did not explain the peculiar gait and attitude of the patient. Skiagraphy was employed, and the alterations in the neck of the femur and the elevation of the trochanter, which are now recognised as characteristic of coxa vara, were easily made out. These are well shown in the skiagram (Fig. 2), and for purposes of comparison the pelvis and femora of a healthy child were shown (Fig. 3). In the normal femur, with the limb everted, the shaft and neck were seen to be almost in a straight line, whereas in coxa vara the neck was so curved upwards that

splint was applied, so as to keep the limb in the position of extreme inversion. The operation was performed two years ago, and the child was now able to walk in a satisfactory manner. The left foot pointed directly forwards, the right one was still slightly everted, but not so much as before, and the peculiar shuffling gait with pelvic roll had quite disappeared.

Dr. Sutherland laid stress on the early symptoms of this condition because it was only at an early period of the disease that non-operative inter-

* 'Trans. Clin. Soc. Lond.,' 1894, vol. xxvii, p. 297.

ference would afford some prospect of cure. A considerable number of cases of coxa vara had recently been published, occurring mostly in adolescents and young adults; a few were described as congenital, and others as developing during the period of active rachitic changes. It was easy to

factor in producing this effect would be the pressure of another foetus, and this patient was one of twins. He laid stress also on the position occupied by the child in early life when asleep, or even when reclining, the feet resting on the chest, with the hands clasped over them, as shown in the



Fig. 2.



Fig. 3.—Normal.

see how yielding of the neck of the femur might occur in debilitated patients who were exposed to the strain of much standing or walking, but this cause was absent in the case under discussion. He thought the condition might have begun during intra-uterine life, the lower limbs being kept firmly applied to the trunk of the child. A possible

photograph. In some other cases of coxa vara recently seen he had found the same attitude present, although not to such a marked extent. If this position was not sufficient to originate bending of the neck of the femur, it was certainly sufficient to intensify the process when once started.

MEETING OF THE SOCIETY OF ANÆSTHETISTS,

At 20 Hanover Square, December 16th, 1898;

The President, Dr. DUDLEY BUXTON,
in the Chair.

Discussion on "The Choice of an Anæsthetic."

MR. E. F. WHITE opened the discussion with the following paper.

On being asked, gentlemen, to open this discussion on the choice of an anæsthetic, I felt naturally a great diffidence in accepting the position, but as really my opening is only to lead you to expound the views, the outcome of your various experiences, I consented. I know that this subject is of every-day interest to you; you have formed views of your own, founded upon your practice more particularly, and not upon experiments upon the lower creation, valuable no doubt in their way, but not always able to guide us in our work. In choosing an anæsthetic our main object is to select the one that is safest to our patient. That cannot always be done, for it may be the operation is either too short or too long for the anæsthetic considered most safe, so the least dangerous has to be chosen; or this may again be thrown out of court by the nature of the operation to be performed, hence our patient under these conditions has to take an anæsthetic that is not in our opinion the best, but only the best under the circumstances. I will therefore try and argue for the best anæsthetic in each case without attempting to open up the old and much-worn physiological controversy of ether *v.* chloroform.

I will dismiss such agents as pental, ethyl bromide, ethidene, and others with a mere mention, in the belief that all their properties are more than covered by one or other of those great anæsthetics—nitrous oxide, ether, and chloroform. Now, of these three which is the safest? To this question my answer is that nitrous oxide is undoubtedly so. Death under it is almost unknown, and I do not know of any statistics that I can quote with the belief that they represent the facts fully. We have heard of deaths, but I have never really satisfied myself that they have been due to the toxic effect

of the agent, uncomplicated by shock to a patient in feeble health, and administered properly by a skilled anæsthetist. After nitrous oxide in point of security to life comes ether. Ether as we know it is made from methylated spirit, and has a specific gravity of .720; ether made from alcohol is used, but is more costly, and I believe no better than the cheaper preparation, although the usual stereotyped announcement that under it there is less sickness and deeper anæsthesia, &c., is opposed to my experience. The other ethers of this group, although able to produce anæsthesia, are not desirable on account of crudeness. Of chloroform the two varieties in the market are the one made from alcohol and the other from methylated spirit; chemically they are the same, and I have found upon the human subject that the action is equal. Upon the lower animals I hear it is not so, the one from absolute alcohol being clinically safer. Of these two last anæsthetics the death rate is much greater than of nitrous oxide, and of the recognised ratio of deaths chloroform has 1 to 3000, ether 1 to 16,000, to account for; therefore, upon these figures alone the safety of ether appears assured. I know we will hear a great deal about cases having died from ether lung trouble, but we hear nothing of those saved from death during the operation by its stimulating effect, and I may here say that I believe the lung troubles are greatly upon the decrease in skilled hands with a good apparatus, using as the skilled administrator does little ether compared with that of former days, before so much of the present knowledge regarding ether was thought out. For a routine general anæsthetic I believe ether, as shown in the foregoing figures, to be quite five times more suitable than chloroform. In a moderately healthy person ether is practically riskless, whereas chloroform appears to choose the apparently healthy for its victims.

In the case which, either from personal idiosyncrasy or from the fault of the administrator, the line of dangerous symptoms has been reached, more time is available when ether is the anæsthetic, to restore your patient, than when chloroform is used; for say what experimentalists like, the practical anæsthetist does meet with deaths from sudden syncope under chloroform which no means known can overcome. This condition occurs in the early stages of chloroform administration;

perhaps it at times may be reflex, brought about by a too eager surgeon. This does not happen with ether. As an administrator I feel happy when using ether hearing my patient breathing deeply and regularly with a good pulse, a pulse and respiration that is not suddenly acted upon by reflexes due to the operation. With this respiration and pulse of ether any slight deviation is easily noticed, and can as easily be rectified; whereas with chloroform such a change is very sudden, and of grave import. The more I give chloroform the more anxious I feel during the administration of it; the converse with ether, for the more I administer ether the more confidence I gain. In operations that are likely to extend over a considerable time the advantage on the side of ether is very great, for after about three quarters of an hour or even less of administration an anæsthetic sleep can be produced that is alike of advantage to the patient on account of the small amount of ether used, and to the operator on account of the quiet state of respiration, ether all the while helping to ward off shock. But ether cannot keep off shock indefinitely, and under any anæsthetic there must be a limit of time that that anæsthetic can be used, and I must appeal to our friends the surgeons to help in shortening the length of operations by commencing as soon as the case, according to the anæsthetist, is ready for them, and not to address their class even for a few moments when their patient is under an anæsthetic. I believe that patients suffer severely from prolonged anæsthesia alone. My own experience tells me that after one and a half hours a case runs down very rapidly, not only from shock of operation, but from the anæsthetic. I regard ether from the above facts as the most suitable anæsthetic for general surgical work, and I venture to say that unless strongly contra-indicated by the patient or operation, ether should invariably be employed. In practice I go still further, for even if there be a doubt against ether I give it a trial, and often find to my astonishment that the case turns out a most suitable one for the use of this anæsthetic. Thus we have named the most suitable agents in order. It has now been found that their administration safely and efficiently can be improved by their admixture. Thus nitrous oxide gas with oxygen can certainly be regarded as safer, although I doubt if the period of anæsthesia obtained is

longer. Ether with gas given first certainly gets rid of nearly all spasm, which is in relation so often to the fear of the patient, by producing anæsthesia unconsciously; and chloroform is made safer by ether being administered first, so as to tide the patient over the early depressing effect of low blood-pressure so characteristic of chloroform action.

As to the A.C.E. mixture, I consider it has no special place; it is a haphazard mixture of unequal evaporation, and whether your case is under ether, chloroform, or alcohol, no man knows. Closely associated with the anæsthetic is the apparatus for giving it; for a safe anæsthetic may be easily made dangerous by an ill-chosen instrument. With regard to gas, and gas and oxygen, we are all agreed. With regard to chloroform we have the school in the north that is not as mathematically accurate as the Indian teachers who commence by measuring drachm doses. I am glad to say that now we have the Junker's apparatus, we can again, as Clover did in a more cumbersome fashion, measure what we are giving; to this no man can object, not even the most careful drop-bottle user possible. With ether it appears to be more essential than with chloroform to have a proper inhaler. Ether cannot be given from a towel with success. A cone of leather with a sponge in it was for years used; this entailed the use of large quantities of ether, producing icy cold air to be breathed. Who can wonder at pneumonias from cold and œdemas of mucous membrane from direct inhalation? Then came Clover and Ormsby to the rescue. Of the two inhalers I prefer Clover's, for I believe the dose is more easily regulated, and the mixture of respired air and ether more even. The small amount of ether necessary to keep up anæsthesia during the later stages of a long operation is very remarkable. There is one point in the mode of operation of this inhaler that I cannot explain, and therefore ask information from you. Place a well-fitting Clover's inhaler upon a patient, and let him respire his own breath over and over again; he will certainly asphyxiate. Let ether be turned on, and when your case is well under, you find that he can go on without fresh air for a long period without asphyxiation. Unless the patient uses some of the ether oxygen for his own economy I have no explanation, but the fact remains. For old and young I use this inhaler, and believe it

helps one very much in choosing an anæsthetic, and I do not admit air regularly at intervals, but rather try and keep up anæsthesia without it. Although I have stated that I believe ether or gas and ether given by a Clover's inhaler to be the most useful and safest of known anæsthetics for general use, I must admit conditions that modify my practice exist; for instance, extremes of life. The child and old person take chloroform rather better than the adult. I cannot specify any age, for the physical condition rather than the age must guide us. General anæsthetics may be given to patients of any age. In infants, owing to the air-passages being so tender, chloroform has more often been chosen, but it is a mistake to believe that children are not as susceptible as adults to the toxic effects of chloroform. It is found that 10 per cent. of the deaths recorded are in children. Besides this death rate, the common drawback in children is the production of deep anæsthetic sleep that so often occurs; I mean a condition when the pulse becomes quite slow, the breathing very shallow, and pupils contracted. Then suddenly the child is aroused, the pulse quickens, pupils dilate to normal, and it kicks and screams to the discomfiture of the administrator and annoyance of the operator. This condition does not occur under ether to the same extent, nor does the lowering of the breath-sounds. Children take ether much better than is generally supposed, if given from a Clover's or Ormsby's inhaler, and I must confess to having given a child a few weeks old ether from a Clover's inhaler with good result. In fact, if I can get a mouthpiece to fit I prefer ether. In children, if chloroform is given in prolonged operations, ether must also be given as well, perhaps alternately through two Junker's bottles, as the effect of ether is very good in maintaining the strength of the patient. I often in the early stage give a little chloroform, and then ether from a Clover's inhaler if the child be young and restless. Old people take ether as well as children provided they have no organic disease of the lungs, and often take it more quietly than the middle-aged subjects. They usually require less and slower administration, and take longer to go under. In cases where a large amount of emphysema exists, ether may produce excessive secretion of mucus, in which case another anæsthetic must be tried; or where the chest wall

is so rigid that its movements cannot keep pace with the requirements of the increased rhythm of breathing, then chloroform is advisable. But ether given in small quantities and slowly in old people that are healthy, I believe is quite the best and safest anæsthetic. Diseased conditions, of course, modify the choice of an anæsthetic.

In diabetes I should administer as if the patient were quite well; and although cases are recorded where nitrous oxide has produced glycosuria, I know of no reason why the gas should not again be given. In pyrexia, shock, and prostration the anæsthetic should be given slowly and very carefully, ether being my choice, and my aim the production of anæsthetic sleep. In hysteria, epilepsy, and chorea, if for an operation, ether is the safest anæsthetic; but if to help your case over a period of some days, then chloroform must be given at intervals, but not to complete surgical anæsthesia, and only during the exacerbations of the complaint; by this means six or eight days may be tidied over. In the insane I have often given gas and ether while an operation has been performed. In cases where acute bronchitis and affections of the lung are evident, and an operation must be performed, then chloroform is the only anæsthetic; but I would rather not be the anæsthetist. If asthma or chronic bronchitis is present, then, if only slight, ether may certainly be tried at first, but not given quickly or pushed; patience must in these cases be shown. I have seen quite as much harm done by chloroform in chronic bronchitis as by ether. In heart disease I have tried both anæsthetics over and over again. Chloroform I believe to be as safe as it can be elsewhere in valvular disease of the heart, both mitral and aortic, and in large aneurysms perhaps safer than ether with its stimulating effect. My rule is in an ordinary heart disease case to commence with ether, and I cannot remember having regretted it. It is some time now since I used a stethoscope to listen to the heart-sounds before giving the anæsthetic, not knowing how to act differently from my usual mode when discovering disease. In abdominal work, with distension, perhaps obstruction and vomiting, the anæsthetist is taxed greatly, for with these conditions is closely associated shock. The anæsthetic must be most carefully given, and in only very small quantities; therefore Clover's inhaler is very suitable. Ether

must be given in part, if not in whole, as chloroform cannot be trusted to give any assistance in helping the case over the operation. Vomiting is very difficult to treat, for to check it means a deeper degree of anæsthesia than is allowable. I take it that vomiting in ordinary cases one ought to be always able to stop by narcotising the centres governing this act. I always look upon ordinary vomiting to be the fault of the anæsthetist. Owing to the region to be operated upon, the anæsthetic has at times to be altered. In brain surgery chloroform is generally given, so that a more quiet state of respiration and circulation may result; also, with the idea of less bleeding, morphia is given previously. As a rule, I dislike morphia to be given before, owing to its uncertain action with chloroform and ether, and also because it masks some of the reflexes. In ophthalmic surgery the face-piece of the inhaler is in the way in some operations; and where to check bleeding into the anterior chamber is important, chloroform is generally used, and I have seen cases nearly die when the optic nerve has been divided. Mouth surgery, again, calls for a special choice of anæsthetic. In adults I like to give gas and ether until complete anæsthesia, then to go on with chloroform; the little ether does help the case over the shock of the operation and over a dangerous stage in the chloroform administration. I like this method for both short adenoid as well as long tongue cases. In the outside of neck operations, such as glands and thyroid, I now give ether; formerly I gave morphia and chloroform for thyroid, but have discontinued it, as I found no advantage. The movement of the abdomen and rigidity of muscles are the two features that guide one in abdominal surgery. I give ether as the cases are generally long and severe, and I try after the first few minutes to procure anæsthetic sleep, so as to decrease the depth of respiration.

Lastly, gentlemen, I will call to your notice one class of cases that is far more important to the general practitioner than any yet mentioned. In parturition the medical man finds that he has now very often to give an anæsthetic, either to decrease pain simply or to operate. On account of his being generally single-handed he cannot give ether and operate at the same time; and, again, ether does not commend itself to slight anæsthesia, as required in child-birth pains; chloroform does.

Chloroform has been the choice of the general practitioner for a long time; not that it is, as is often supposed, free from risk, for it is not, but on account of its convenience. The circumstances often necessitate the anæsthetic being given by the untrained hand of a nurse. In a small room, with gas or lamp-light, often large quantities of chloroform are used, and the air becomes so impregnated as to make the room almost uninhabitable. I have myself had to leave for fresh air to smother my cough from irritation. The use now of a Junker's apparatus clears the atmosphere and limits waste. Even in the nurse's hands safety has been much increased. The advantage of the introduction of the Junker into this class of practice generally I consider of extreme importance, and I hope all teachers of students will insist upon its value, and help to see that no midwifery bag is sold without a small inhaler in it.

The PRESIDENT said he would like, before the discussion, to say a word or two with regard to the schedule. After the lucid introduction they had had from Mr. White, they would perhaps understand the idea of the scheme for the discussion. He would say, however, that when it was drawn up it was intended simply as an indication, for the line along which it was thought the discussion should wander, rather than to bind down any particular speaker to any one portion of the schedule. It was open to anybody to discuss from the first word of that schedule to the last, and to add any point which had been omitted.

It was evidently an immense difficulty to them not only to suit their own convenience with regard to an anæsthetic, but to ascertain what was in the mind of the surgeon when he was operating. Frequently one asked the surgeon what his own view was, and he often courteously said, "I leave that matter in your hands; if I call in a specialist I expect him to give the best anæsthesia he can." But he, the President, had no doubt that when the surgeon went home he made mental notes as to how far the methods and the anæsthetic had been, from his point of view, right and proper, and it was partly with the aim of probing the surgical mind that this discussion had been initiated. It had met with a great deal of sympathy among their surgical *confrères*, many more of whom would have been present if they had not engagements elsewhere. He was very glad to find that some of

their surgical friends were able to be present, and he hoped that they would give their views in the matter in the most open way possible, because, as he had said before, anæsthetists, like other men, probably learned more by their failures than by their successes. They seldom heard of their successes, but they did occasionally hear of their failures, and it was of great advantage to them to hear of them.

Mr. EDMUND OWEN said he thought the debate which had been arranged should be of the greatest advantage, not only to anæsthetists, but to surgeons in general and to the public. He thought it would be a very considerable pity if they let the result of this debate, whatever it might be, pass into respectable burial, in some magazine article it might be. He was only a visitor that night, but he would like to suggest the expediency of appointing a sub-committee from the Society to consider what was the general opinion of those who took part in the debate, and then to issue it as a sort of pronouncement from the Society of Anæsthetists. He thought it would have a very great effect in guiding wavering surgeons and young anæsthetists, of whom there seemed to be a great abundance in London at the present time. The President was good enough to suggest in his letter that he, Mr. Owen, should make some remarks in connection with the administration of anæsthetics to people on the threshold of life; this he would be able to do, but before that he would like to make a few remarks personal to himself.

Suppose, on leaving that room, he were unfortunate enough to meet with an accident and dislocate the head of his femur on to the dorsum of his ilium, he would not ask the man in the street to administer an anæsthetic to him, he would get one of the members of this Society to be so good as to administer the anæsthetic, and he would say, "If you please do not give me ether, give me chloroform." Now, even if it were the President himself, or even his friend Mr. White, he would say, "Yes, I will give you chloroform," but he felt sure that if he thought his, Mr. Owen's, lungs or heart were not quite right, as soon as he started with the chloroform he would give ether, or just what he thought best. And as the President or Mr. White, as the case might be, would have the responsibility of the case, he thought they were right in so doing. But he would beg for chloroform. A week or two

ago he was operating on a lady who had herself been matron of a surgical home, and who had helped him (Mr. Owen) in very many operations, and who knew a great deal about anæsthetics. He was going to suture her wandering kidney into a proper position. She said, "Let me beg you to have chloroform given, not ether." He said, "Why not ether?" She replied, "Ether is beastly stuff." That was his own opinion; ether was beastly stuff. It was said, and Mr. White had just said, there was more risk with chloroform, and that was the general opinion. He would say as that boy said in Phil May's beautiful picture in 'Punch,' "it's worth it." It was worth the risk to have chloroform rather than ether. He had been a good many years connected with the work of operations on children, and so on, and probably he had had his share of private practice as well as hospital duties, and all this time, at least until a few months ago, he had never seen a child die from the administration of an anæsthetic, and the anæsthetic which had almost invariably been used was chloroform. He had been surprised to hear Mr. White say that a large percentage of the deaths from chloroform were in children. He had been sorely tried by anæsthetists, good, bad, and indifferent, in connection with children, for, unfortunately, every young practitioner, every old practitioner, and everybody thought that he was good enough to administer an anæsthetic for a child, and he, Mr. Owen, had often had to keep one eye on the patient, and another eye on the anæsthetist. The gentleman who gave him most alarm was the man to whom Mr. White had referred as belonging to the Edinburgh School. He dashed the chloroform on to a towel, and he thought his expression was that he "knocked the patient down." He did knock the patient down, and he, Mr. Owen, was sometimes surprised to find that he was ever able to rise again after the blow he had received. Another popular way of administering an anæsthetic to a child was by the flannel cone. He personally had a considerable dread of that flannel cone; he had seen even well-qualified anæsthetists using it, and drop the chloroform through on to the child's face, or possibly even into its mouth. He thought a flannel cone should be used with the very greatest care, and when the child was lying on the back to drop the chloroform on to the front of it was extremely dangerous.

He knew anæsthetists did not like to be told about it, but he had a dread of using chloroform administered in that way. Knowing that dread, some anæsthetists put the child's face on one side to administer the chloroform with the cone. But supposing students saw chloroform administered to a child on the cone, they did not quite recognise whether the child was on the back or on the face. He thought that if an anæsthetist—he did not care who he might be—was giving chloroform to a child, or to any other person, by a flannel cone, he should always, as a matter of ritual, take the cone away before dropping the chloroform on to it. The case to which he was about to refer was an extremely sad one, and it occurred in his practice this year. A gentleman was giving an anæsthetic for a child who was in perfect health; it had some congenital defect which he was about to remedy. The gentleman had had very little experience in administering anæsthetics, but as he had said before, everyone thought he was good enough to administer to a child. He saw there was something which was not quite right, and he said to the anæsthetist, "What is it?" He replied, "Oh, it's all right," and took some other anæsthetic, and began to administer it to the child. There was then a considerable amount of alarm, and, to make a long and extremely sad and painful story short, that child died. They tried everything to bring it round, but without effect. He found that the fluid which the anæsthetist had been administering after the chloroform had been going a little bit wrong was that—if he might be allowed the expression—damnable fluid, A.C.E. mixture. Who knew what it was? They knew how quickly the ether evaporated from the A.C.E.; as Mr. White had said, no one knew what the A.C.E. mixture was when it was being administered from a stock bottle. When it is used once they heard a "pop," and there was some ether gone; next time there was another "pop," more ether gone, and so on. As he looked back upon this case with extreme sorrow, he believed he could have saved the child's life if he had known that it was A.C.E. mixture which was being given. Let them have an understanding about that mixture; if they, as anæsthetists, held a strong opinion upon it let them say so, and let the public know. It was very easily administered, and everybody was inclined to adopt the easy method of administration.

Then they came to cocaine. Mr. White had not said a word about that, but it seemed that that was the anæsthetic which was to be used in connection with operations on the thyroid gland. Mr. White might say it did not concern him (Mr. Owen), because if cocaine was being administered it was not the surgeon or those who were listening to him who were administering it; the people who wanted it should do it themselves, as anybody could inject cocaine into the thyroid gland. He was afraid there would be trouble in connection with that substance in the surgery of childhood, and he would be extremely sorry to see it. He nearly poisoned an adult once by injecting cocaine, and he would never use it on a child. He mentioned cocaine as an anæsthetic only to condemn it.

He would allude briefly to three operations in connection with children. The first was the operation for cleft palate. For that operation he thought it best for the child to be upon the back, with the head thrown far back, so that if any blood flowed it would be into the dome or the nasopharynx, and so up by the nostrils. He had extreme sympathy with the anæsthetist who was officiating in a case of operation for cleft palate, and all the surgeon could do in such a case was to avoid being too severe upon the anæsthetist; he should not ask him to push the anæsthetic, for he must have tremendous anxiety. If in such cases he (Mr. Owen) found the child was beginning to struggle, he did not say to the anæsthetist that there was not enough anæsthetic; he left it in that gentleman's hands. It was a most trying ordeal, and when the operation was over he was thankful for the sake of the anæsthetist as well as for himself, because some children died of shock soon after the operation.

The next operation was for the removal of tonsils and adenoid growths. That was considered by many people to be an operation free from risk. For his own part it was an operation which he performed with considerable anxiety, especially when he had a stranger administering the anæsthetic. Dr. Hewitt had given the anæsthetic for him in many cases, and when he did so he always asked him (Mr. Owen) in what position he would like to have the patient. His reply was that he would like to have the patient sitting up for removal of tonsils, and with the head thrown back for the removal of adenoids, but that he would leave it to

the anæsthetist. It was said—and he thought that would be a point for discussion—that there was more bleeding when ether was given for the removal of adenoids than with chloroform. He was not sure of that; but if chloroform was the anæsthetic employed in removal of tonsils and adenoids, he thought the child should certainly be lying down with the head thrown well back, as in the operation for cleft palate.

They now came to the third of the three operations to which he ventured to call their attention, namely, for the removal of large tuberculous glands from the neck. In his opinion this was an operation surrounded with anxiety so far as the anæsthetist was concerned. Probably everything was going on well while the chloroform was being administered, when suddenly, to all appearances, the child was dead. He had had several shocks of this kind, both in the country, where skilled anæsthetists were not to be had, and in London, where he had had the advantage of specialists. If the members of that Society had met with those extremely critical cases in which the child suddenly ceased to breathe and became apparently dead in connection with this operation, he asked how they would explain them. What had Mr. White to say to this? Could he offer any explanation? He thought it must be in connection with some disturbance of the pneumogastric or superior laryngeal or possibly of the sympathetic nerves, but most probably, he thought, of the pneumogastric.

Next he would say to the members of the Society one word as a surgeon, namely, that he was always ill at ease when the anæsthetist—he did not care who he or she might be—began to talk during the operation. When he was operating on a patient he did not want a word said to him, and he did not want to hear the anæsthetist say a word. If the anæsthetist began to talk, he began to say to himself “He is not realising the importance of his position.” He maintained that no one should speak to the anæsthetist while he was doing his work, and that if anyone should speak to him while so engaged, he should shake his head and say nothing in reply. He would like to write over the anæsthetist’s head, “Do not speak to the man at the wheel.” Sometimes an anæsthetist went as far as to lend the aid of his septic hand in the operation. The anæsthetist should confine himself absolutely to his work. He had known anæsthetists

even give advice to the surgeon in the course of the operation. Well, he did not think they should; he would put it in that way. Lastly, when they, as anæsthetists, had done their work, and when the child was beginning to come to, they sometimes pulled out their watches and said, “The patient is all right now; may I leave?” It would be a hard-hearted surgeon who said, “I should like you to stay five minutes longer till the patient gets properly round.” They left some of the anxiety behind for the surgeon to bear alone.

He was very much obliged to them for having listened to him, and for having asked him to speak on the subject. He had been asked to speak freely, and he had done so.

(To be continued.)

REVIEW.

‘THE TALLERMAN TREATMENT by superheated dry air in Rheumatism, Gout, Rheumatic Arthritis, Stiff and Painful Joints, Sprains, Sciatica, and other affections.’ Case notes and medical reports with numerous illustrations. Edited by ARTHUR SHADWELL, M.A., M.B.Oxon., M.R.C.P.Lond. (Baillière, Tindall, and Cox, King William Street, Strand.)

By means of an apparatus invented by Mr. Lewis A. Tallerman, dry hot air is locally applied to one member or limited part of the body, inducing diaphoresis, increasing the flow of blood in the skin and in the subcutaneous tissues, relieving pain, often entirely removing it and securing increased power in active movement. Put into other words, this remarkable therapeutic invention places within reach of medical men the means of successfully treating obstinate cases of rheumatism, chronic gouty attacks, arthritic contractions, synovitis, gonorrhœal rheumatism, contractions from subacute inflammatory diseases, and many other affections. When it is stated that this discovery has now been in use for some four or five years in various countries it will appear to some a matter for surprise that a more general recognition has not been accorded to this valuable invention of Mr. Tallerman. The publication of this work will do good by changing the indifference of the past

into a lively appreciation of this scientific and excellent method of treatment. One naturally expects medical testimony to substantiate the claims put forward in regard to this discovery, which Mr. Willett of St. Bartholomew's Hospital has well described as "startling." However "startling" the effects of the process may appear the weight of medical evidence is in its favour, and much importance must be attached to the deliberate judgment formed by Dr. Arthur Shadwell, after exceptional opportunities of verifying the results obtained. Dr. Shadwell's reasons for accepting the task of supervising the preparation of this volume are threefold: first, experience has convinced him of the beneficial effect of the practice; secondly, he believes it ought to be very much better known than it is; and thirdly, he has no personal interest in it whatever. The common distrust of novelties so general amongst medical men made Dr. Shadwell originally approach Mr. Tallerman's invention with scepticism, but he tested it on his own *corpus vile*, and found that it did what it pretended to do. He then saw a boy with a knee-joint full of fluid, and wincing at every movement, gradually charmed off within half an hour into a smiling and painless indifference which permitted the free handling and flexion of the limb without a murmur. Since then he has repeatedly seen results produced in old and hopeless cases of rheumatic arthritis which he would not have believed on any lesser evidence than his own eyesight. This experience of Dr. Shadwell is most fully borne out by the facts related in this book, and the body of evidence brought forward is attested by so many independent observers of the highest standing in the medical world that it is impossible for anyone to affect to ignore or despise it.

It is very greatly to the credit of Mr. Tallerman that no case has been permitted to be treated, or apparatus to be let, without direct medical approval and direction; and it would be a public boon if caution were used to avoid cheap imitations infringing the Tallerman patent.

At a meeting of the Executive Committee of the Jenner Society held in Gloucester, resolutions were unanimously passed expressing appreciation of Mr. Rider Haggard's powerful story, 'Dr. Therne,' assuring the author of their sympathy, and congratulating him on his work.

ON THE CAUSE AND MECHANICAL TREATMENT OF SUBLUXATION OF THE SEMILUNAR CARTILAGES OF THE KNEE-JOINT.

SHAFFER ('Annals of Surgery,' October, 1898) calls attention to the fact that "Hey's Internal Derangement of the Knee-joint" occurring as it does from trivial mishaps as well as from major injuries, and being a not infrequent accident, it seems strange that there should be any doubt as to its essential trouble so long ago as 1803. A further study of the subject in Allingham's treatise, "Internal Derangement of the Knee-joint," published in 'Wood's Medical and Surgical Monographs' in 1890, will dispel all reasonable doubt in the matter. It may be profitable, however, to inquire into its exact mode of production, but the essential nature of the trouble is well understood.

From these and other sources it may be assumed that the "internal derangement" described by Hey is occasioned by a varying degree of displacement—by even, in many cases, an imperceptible subluxation of one of the semilunar cartilages. In brief, a visible or manually demonstrated existence of the subluxation is not always necessary. The trouble is so far "internal," that in many cases the ordinary physical signs of a dislocation are not to be observed or felt. The writer has seen several cases where the trouble was diagnosticated as a "sprain," and he knows of two instances where the physical signs of the trouble were so inconspicuous that a diagnosis of an "hysterical joint" was made.

A prolonged study of many cases convinces the author that, aside from the generally accepted view that this subluxation occurs while the knee is flexed and the leg is rotated, there are other contributive causes which have not been carefully investigated,—in short, that it is not the simple fact that rotation of the tibia occurs at the knee during flexion and extension of the joint, for these are normal movements, but rather that there is a delayed or hindered extension and rotation which permits this accident to occur. It would seem, from his studies, that this subluxation is not likely to occur, and he doubts if it ever

does occur except perhaps in cases of violent traumatism while the quadriceps is relaxed.

Nor is the trouble ordinarily considered as one which belongs to orthopædic surgery. Especially since Allingham's time it has been regarded as being within the domain of the general surgeon, and so it is in its purely operative aspects. But, as with chronic diseases of the spine and joints, it has its conservative side, and if orthopædic surgeons can offer relief and cure by mechanical means, the general surgeon will welcome their efforts and aid them in their work.

It seems plain from cases the writer records that in order to prevent the recurrence of a subluxation of the semilunar cartilage it is necessary to correct the undue ligamentous weakness of the joint, and to prevent an abnormal rotation of the tibia; in short, to give the knee and ankle antero-posterior motion only. Under these circumstances undue strain is taken off the quadriceps and the ligamentum patellæ, and under favourable conditions the latter, as well as the relaxed crucial ligaments, may shorten very materially in the course of a few months.

The object is to prevent every movement at the knee and ankle except antero-posterior motion—in short, to turn the knee into true hinge-joint, removing entirely the rotation of the tibia. The important part of the apparatus Shaffer employs, next to its simple hinge movement, is the joint at the knee, which is so arranged that it will stop the extension just at the point of comfort to the patient, and this point of comfort represents an absence of strain upon the knee-joint ligaments. This is very essential to the cure of the trouble, for experience proves that if the strain is taken from the ligaments they will shorten, and the "wobbly" knee will gain stability and strength in a few months.

It is important that the centre of the pad at the knee should be opposite the true centre of motion (opposite the most prominent point on the internal condyle is near enough) at the knee, and that it should rest snugly against the condyle without undue pressure. The apparatus need not be made heavy, the principal strength being necessary in the rod which connects the knee with the ankle-piece. It is preferable to have this rod on the outside.

In many cases of Hey's joint there is an acquired, or perhaps congenital lateral mobility of the knee-

joint. This condition existing, the normal rotation of the tibia in flexion or extension of the knee is greatly increased.

In many cases, if not in all cases, there exists an elongated ligamentum patellæ, which so modifies the action of the quadriceps extensor muscle upon the tibia that the force of its contraction upon the tibia is modified or delayed in such a way that extension and rotation are not synchronously performed. And it seems more than probable that this condition forms an important factor in the production of the subluxation of the semilunar cartilage.—*Therapeutic Gazette*, December, 1898.

Pneumonia following Measles.—Ivan Honl ('Rev. mens. des Mal de l'Enf.' Aug.), as the result of extensive observations upon the subject, concludes that—1. The most frequent cause of death in measles is not tuberculosis, but pulmonary inflammation. 2. In simple measles or in measles complicated by pertussis various forms of pneumonia may be found. In the affected areas various bacteria are found—streptococci, staphylococci, pneumococci, pneumobacilli, the bacilli of diphtheria, and pyocyanic bacilli,—but these cannot be regarded as the specific agents of measles. 3. An injection of the sputum of measles patients into the veins of rabbits which are not entirely immune to the pneumococcus, causes a pneumonia which has every appearance of caseous pneumonia, but in which no tuberculous bacilli are to be found. 4. There is a pneumonia following measles which is characterised by a process of suppuration and proliferation, and by the presence of giant-cells. The existence of these giant-cells must be attributed to defective resorption of the exudate due to emphysema and to functional weakness of the parenchyma of the lungs. The exudate and the microbes which remain in the affected areas act as foreign bodies and irritate the lung tissue, causing the characteristic inflammatory process. This pneumonia is not due to any one ætiological agent. Tuberculous lesions have sometimes been found, but in many cases there has not been the slightest trace. We must, therefore, either admit that caseous pneumonia may exist which is not caused by the tubercle bacillus, or hold that the form of pneumonia studied is special to measles or pseudo-caseous.—*American Journal of Obstetrics*, December, 1898.

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ON SURGICAL DISEASES OF THE KIDNEY.

A Clinical Lecture delivered at Charing Cross Hospital

By JOHN H. MORGAN, F.R.C.S.,

Surgeon to the Hospital; Senior Surgeon to the Hospital for Sick Children, &c.

It would, perhaps, be difficult to assert that the surgery of any one part of the body had advanced with much greater strides than that of another during the past fifteen or twenty years; still more difficult would it be to show that the operative procedures on any one of the organs included in the abdominal cavity had surpassed in magnitude and in results those performed on other parts of equal importance; but it will hardly be asserting too much to say that although much greater perfection may yet be attained both in the diagnosis of causes and in selection of procedure, the surgery of the kidney has developed during this period to an extent which seems almost appalling to those who studied surgery before that era. Since the time that surgeons have learnt that with proper precautions the peritoneum may be treated almost as freely as superficial parts, the organs which it surrounds, or with which it is in contact, may be dealt with both for the purposes of diagnosis, or for remedying effects which are the result of disease.

Thus there have been developed various proceedings to which definite names are given, and which are resorted to for the various conditions which it will be my duty to describe this afternoon.

It will be best to name these operations before describing the conditions which may call for their performance.

Nephrorraphy is the name given to a proceeding by which a kidney that has been deprived of those conditions which should keep the organ more or less fixed is made to adhere to the posterior abdominal wall. The organ is cut down upon by

an incision in the loin, and the fatty capsule is stitched by several sutures to the deeper parts of the wound. One or two deeper stitches are passed through the cortical substance and the fibrous capsule, and are then brought out through the abdominal wall. By this means, after healing has taken place, the kidney is made to adhere to the abdominal wall nearly, if not quite, in the position which it should naturally occupy.

Nephrotomy or incision of the kidney is, as we shall see, an operation of far greater frequency than formerly, and is resorted to in a variety of circumstances, which we shall come to consider. As in a large number of these the kidney is altered in its shape, size, and surroundings, no definite rule can be laid down concerning it, and the direction of the incision may be altered according to the circumstances of the case, but it is made midway between the last rib and the crest of the ilium, and after reaching the abdominal cavity by successive sections of the muscles and fascia, the fat covering the gland is torn through, and the kidney is dealt with according to the condition which is found.

Nephrolithotomy is only a development of the operation just described; that is to say, that while the kidney is exposed by the same means it can be examined by palpation with the finger, or with the fingers of both hands placed above and below, for any indications that may be found on its surface, or it may be punctured by a grooved needle, or a blunt pointed instrument may be passed into any part that is desired in order to ascertain or confirm the presence of a stone. Furthermore, an incision may be made into the pelvis, or even into the cortex of the gland, sufficient to admit the finger, and further exploration by that means, or with the additional assistance of a bent probe, can be pursued. Should a calculus be found, the incision can be extended to a size sufficient to allow of its removal; the bleeding in such proceedings is usually considerable, but can be easily controlled. On completing the operation a drainage-tube is inserted to the fullest depth of the wound, and although the fistulous opening may remain for a long time patent, it as a rule heals without further operative measures.

Nephrectomy is the operation by which the whole gland is removed, and can be performed in two ways: if the incision already described as suitable

for the exploratory operation be adopted, the transverse cut must be supplemented by a vertical one down to the crest of the ilium, which requires to be made in order that sufficient room may be obtained. The mass that requires removal is then separated from its surroundings—of which the peritoneum is of course the most important—by the aid of the finger. The pedicle, which includes the renal artery and vein, must be ligatured firmly with stout silk, and the ureter must, if possible, be secured separately with a similar material, and after removal of the organ a drainage-tube must be inserted. In the abdominal operation the incision may be made either in the middle line or along the linear semilunaris, all the usual precautions being taken which are adopted in the case of other abdominal operations. When the peritoneum is exposed, the colon must be kept out of the way, and the serous membrane opened so as to lay bare the pedicle, which then requires to be ligatured; the ureter is then divided between two ligatures, care being taken that nothing escapes from its lumen. The kidney may then be severed from its attachments, and the rent in the peritoneum carefully united with catgut. When the incision is made in the middle line, as in the operation of ovariectomy, after opening the anterior layer of the mesocolon the proceeding is much the same as in that by the lumbar incision. So far, however, the statistics of this operation do not compare favourably with those of the former method, though the circumstances under which they are chosen are not quite parallel, and may with further experience be found to have been unduly prejudicial against the median operation; there is, however, one very great advantage which is offered by this incision as against the lumbar, namely, that where suspicion may exist that the opposite kidney may be affected by a similar or by a separate condition of disease, both may be explored at the same time, and if thought justifiable by the circumstances thus elucidated, one may be removed or otherwise dealt with.

If we now turn our attention to the conditions under which these operations may be called for, the first one described is that which is of most doubtful necessity and is least often required. Nephrorraphy may very rarely be justified in cases where the kidney is not fixed by the usual structures, and on this account gives rise to distressing

symptoms. There are two conditions under which this organ may be liable to shift its position, and thus to give rise to symptoms which require that it should be fixed artificially to a definite location; either it may be movable behind the peritoneum lying free between it and the abdominal wall, or it may be floating within the cavity of the serous membrane and attached by a mesonephron to the spine, the latter condition being the rarer of the two. Both conditions are met with most frequently in females, particularly in those who have had frequent pregnancies, and it is much more frequent on the right than on the opposite side. The symptoms are those of occasional pain, varying in amount, aggravated by pressure, and often intensified at the period of menstruation, these symptoms being associated with the presence of a palpable movable tumour on one side of the abdomen. Sometimes the urine will contain blood, and the amount will be increased by manipulation, and in other cases the amount of urine will diminish as the size of the swelling may increase, owing to a temporary condition of hydronephrosis. Another cause of this condition is the emaciation which may result from any cause, and thus produce absorption of the fat which surrounds the organ and helps to maintain it in position. In the majority of cases the symptoms can be remedied by attention to the condition of the bowels, by abstention from violent exercise, and by the use of a belt, which in severe cases may be supplemented by an air-pad to support the organ in position. For an exaggerated condition of the above symptoms the kidney has been several times removed, but the mortality has been so serious that a less formidable operation, such as that offered by nephrorraphy, should certainly in the first instance be given a trial, as the results thus obtained have been exceedingly satisfactory, especially when the organ on the opposite side can be ascertained to be healthy.

The condition termed hydronephrosis is brought about by many causes. It consists of an accumulation in the pelvis of the kidney and upper part of the ureter of a non-inflammatory fluid—that is to say, of urine in a modified condition, but not containing pus. It may occur congenitally—that is to say, it may result from some abnormal condition of the ureters, either by the existence of a twist or a valve in their continuity, or by some obstruction at

their entrance into the bladder; and it is often found to co-exist to greater or less extent with the deformity of extroversion of the bladder; if bilateral it is necessarily incompatible with prolonged life, and therefore does not call for surgical interference. Again, it results not infrequently in children as the result of some accident by which the ureter is either temporarily blocked by clot or rupture, but in adults the causes may be summarised as any that lead to the temporary or permanent obstruction to the passage of urine from the kidney, and these include simple or malignant tumours, abscesses, bands or adhesions, the results of former inflammation causing pressure, obstruction to the urine by stricture, enlarged prostate, &c., displacements of the kidney or twisting of the ureter, the impaction of a calculus, or tumours of the bladder. The symptoms felt by the patients in this condition, which is often the result of disease in parts external to the urinary apparatus, vary much with the cause, and vary also greatly according to the amount and character and situation of the obstruction. If it is complete, a swelling caused by the urine accumulating in the parts which retain it increases rapidly, and gives rise to a large and fluctuating tumour in the lumbar region of the side affected; as a result of this, pressure is put upon the secreting structure of the kidney, which checks its functional activity, and in the course of time may cause its obliteration, so that the capsule of the organ forms the boundary of the fluid; but if the obstruction be overcome, a large quantity of urine may pass suddenly into the bladder, and the tumour will for the time disappear, and under such circumstances the absorption of the gland structure is of much less rapid progress. If the fluid from such swelling be drawn off by an aspirator inserted behind at a point midway between the last rib and the crest of the ilium, and two and a half inches from the spinous process, it is not necessary for diagnostic purposes that this should consist of fluid presenting the ordinary characteristics of urine. On the contrary, it may contain but little urea and very large proportions of albumin, and this even in cases where the obstruction is recent or even transitory. The change in the fluid is accounted for by the pressure exerted upon the secreting structure of the gland, which interferes with the proper discharge of its functions, and the normal elimination which it

should bring about. The tumour which is thus produced varies in size, but is situated behind the colon, and increasing from above causes a dulness on percussion which does not shift its position, but may extend beyond the median line; and if the affection be confined to one side, there will be no great alteration found in the urine passed through the bladder, though it may at times be scanty and tinged with blood, and there may be pain due either to the swelling itself or to the cause which gives rise to it.

The treatment of hydronephrosis must depend very largely upon the cause. In some cases following injury repeated tapplings by which large quantities of fluids have been withdrawn, have gradually led to the obliteration of the tumour, either through the channel becoming re-established or by the gradual absorption of all the secreting structure. The same result has been brought about by freely draining the sac through a tube inserted by an incision in the loin similar to that practised in nephrotomy; but this, though relieving the symptoms for a time, is a tedious and even hazardous resort, and in cases where the kidney has become extensively disorganised the total removal of the organ is the speediest and safest expedient.

The condition thus described, although involving destruction of the gland, need not necessarily be accompanied by the presence of products due to inflammatory changes in the fluid, or in the surroundings which contain it. To this condition the term pyonephrosis is applied, as signifying the fact that pus has been developed in some of the parts which form the tumour, and thus causes the swelling to increase with greater rapidity, and to be the seat of much graver pain, and gives rise to symptoms of far greater severity. The causes are, generally speaking, identical with those giving rise to hydronephrosis, but the obstruction caused by a calculus lodged either in the pelvis of the kidney or in the ureter is by far the most frequent origin of this condition. It may here be well to offer a reminder that the term pyelitis is meant to include a different condition—that, namely, in which suppuration arises from inflammatory changes in the mucous membrane of the pelvis of the kidney without previous distension, and which may be the result of cold, or may follow the use of certain drugs such as turpentine, cantharides, &c., or may

be started by accumulations of uric acid, or may be found to accompany pyæmia, diphtheria, or any of the acute specific fevers. Another term, again, which is now in common use, and which calls for definition before describing the symptoms and results of renal calculus, is the word pyelonephrosis, a far happier term than that which formerly included those conditions of the glands which were found to follow many of the diseases of the bladder, viz. "surgical kidney;" the latter term seemed to imply a certain opprobrium to surgical interference, whereas it exists as a result of the disease for which operation may have been imperative; thus it may be expected to exist in all those cases where obstruction to the exit of urine from the bladder has existed for a long period, as in stricture or enlarged prostate, calculus, or growths in the bladder. All these conditions, besides causing hypertrophy of the bladder and ureter, are followed by accumulation of materials which rapidly alter the urine from an acid to an alkaline condition, and tend to promote fermentative changes in it. The product of these changes spreads more or less certainly upward to the kidney, and in the course of time brings about those pathological conditions which we associate with pyelonephrosis. Under such circumstances the organ may be little or hardly at all enlarged, but its surface will appear uneven by reason of the small patches of inflammation which cause the capsule to bulge, and which are found to be abscesses in more or less active stages of formation. The capsule itself will be thickened, but will strip from the gland except at these spots, and when these are exposed they are found to be collections of pus of greater or smaller size, in the shape usually of a cone, the apex of which is directed inwards towards the pelvis; and the amount of gland tissue which is left will depend upon the number and size of such collections. Necessarily this condition is very hard to detect and still more difficult to estimate, since the urine under such circumstances as give rise to this disease can afford but little indication, being altered by the very conditions which cause the destructive changes in the gland. Another condition of the kidney which rarely if ever can call for surgical interference, and which comes more commonly under the care of the physician, is that which is described as acute suppurative nephritis, where the renal

substance is acutely inflamed, and often becomes the site of abscesses, which must be diagnosed by the symptoms evidenced by the seat and character of the pain, and by the indications afforded by the urine, and needs to be treated by the surgeon, if his aid is sought, according to the conditions presented by an abscess occurring in an organ of such importance and with such surroundings. One more condition which resembles the last as regards its treatment, but of which little can be said with respect to its origin or causation, viz. perinephritic abscess, calls for special mention, since its existence may easily be confounded with that of the diseases already mentioned, but which has certain distinctive features that render its diagnosis more feasible. The inflammation of the parts around the kidney may be the result of injury involving the organ itself or the parts in its neighbourhood, or it may be of more or less spontaneous origin, and therefore no evidences may be afforded by the excreted products,—that is, there may be no abnormal condition found in the urine; in fact, the main symptoms are those of abscess, acute or chronic, in the region of the kidney, and therefore all due care must be given to the examination of the urine, and of the parts where abscess commonly occurs as a result of diseases of such structures as the bones comprising the spinal column; but the condition when advanced may be recognised by the marked evidences of the presence of suppuration, as shown by the temperature and other indications of abscess, by the gradually increasing swelling with fluctuation in the lumbar region, which tends to protrude posteriorly rather than, as is the case in pyonephrosis, to displace the organs in front of the kidney.

Thus, having described the various conditions in which surgery may afford relief to the diseases to which the kidney is liable, let us briefly turn to the symptoms for which these operations may be entertained, and the causes which give rise to them. We have already spoken of many of these, but there are others which require more detailed consideration. Into the causes of formation of calculus there is no need to enter, and it is sufficient for this lecture to discuss only the symptoms which indicate the probable presence of stone, either in the pelvis of the kidney or in the ureter. That a calculus of considerable size may

exist for a very long period in either of these situations without giving rise to any reliable indications is proved by many a post-mortem record, and is further evidenced by specimens which are on the table before you; but the great bent of all modern surgery is to seek for early signs of disease in order that operative measures may be undertaken with the greater prospect of success, since the converse condition where operation is undertaken in advanced stages of disease leads only to failure, and casts an unmerited blame upon the surgeon. To what then must we look for early indication of this condition. First in importance is the presence of blood in the urine; this is oftentimes small in amount, and only to be detected with the microscope, and sometimes is not to be found. After violent exertion, as might be expected, it is sometimes profuse, and is always intimately mixed with the urine; but, again, it may not be detected under the most careful examination, and can in no wise be held as a reliable symptom, even when other causes of hæmaturia have been carefully considered. As blood under these circumstances is only derived from the irritation of the mucous membrane of the kidney or ureter, no casts will be found, and the amount of albumin will not be more than is accounted for by the presence of blood; and just as exercise increases, so rest diminishes the amount—a symptom which helps to differentiate this cause from renal tumours, in which the converse is usually found to obtain. The presence of pus in the urine is generally accompanied by considerable pain and swelling in the lumbar region, indicating as it does considerable inflammatory changes in the parts involved. There is not usually much swelling in this region in cases of tubercular disease, and the amount of albumin in this condition is usually excessive, and the urine, besides these ingredients, always contains some calculous material in the former condition, but hardly ever in the latter. As regards the symptom of pain, it may be said generally, as just mentioned, that pain is greater according to the amount of inflammatory disturbance which is excited by the presence of stone; but under all circumstances it is a symptom of varying significance, depending partly on the nature and condition of the stone; it may be that it is confined to the parts about the gland, or it is often referred to parts far distant, as

the thigh, the testicle, or penis; and when a stone passes from the pelvis into the ureter the pain is extreme, and lasts until it passes into the bladder or until the ureter becomes accustomed to its presence; but it must always be remembered that all these symptoms, even in their greatest intensity, may result from the condition which Sir Henry Thompson has aptly termed "a uric acid storm," where no stone exists, but in which, owing to the excessive amount of uric acid present in the urine, all the symptoms of calculus will from time to time arise. As regards the swelling caused by accumulation of fluid in the upper part of the urinary tract, it is impossible to lay down any precise indication as to that which is purely renal, since every tumour which is to be met with in the abdominal cavity has at times been mistaken for renal swelling, but the diagnosis must mainly rest upon the concomitant alterations in the constituents of the urine. A symptom which has lately been pointed out as indicating the probable existence of nephrolithiasis is the increase of pain on movements of the leg which call the psoas muscle into action, and thus put pressure upon the surface of the kidney.

We must pass now to the symptoms which point to another disease of this organ to which allusion has already been made, viz. acute and chronic tuberculosis. It has often been asserted that tubercular disease of these parts commences in the bladder and spreads upwards. As you may see from specimens before you, this is not by any means invariably the case; you will notice in these that the lesion has commenced in the substance of the gland, and that the tubercles which were deposited have gone on to the formation of caseous masses which have invaded largely the secreting structure, especially at the apices of the papillæ. From these, as may be easily imagined, the products which are discharged carry the infective material along the ureter into the bladder, and so the whole tract becomes involved, until the latter viscus shows deep ulcerations on its surface. The signs of this disease are always difficult to detect in their early stages. Frequency of micturition is an almost constant symptom, and, as already stated, although blood may be intermittently present the amount of albumin is excessive, and the urine, although not containing casts, is thick and cloudy with pus and epithelium, is alkaline, and bacilli can almost invariably be

detected. The swelling which takes place is not always excessive, though it may generally be detected on palpation. These symptoms, taken with the age and appearance of the individual, afford sufficient evidence of the probable nature; but in this, as in all other affections of the kidney, it is of the greatest importance, firstly, to detect the nature of the lesion at an early period, and secondly, before the question of operation can be entertained, to endeavour to find out how far the opposite organ is free from disease. When it is possible, as it sometimes may be in the case of females, to obtain the urine from each ureter before it enters the bladder, examination may show immunity on the part of one gland or the other, and the question of nephrectomy may under such circumstances be entertained. The diagnosis between this condition and nephrolithiasis is seldom attended with much difficulty when attention is paid to the gradual onset of the symptoms in the latter, accompanied by little deterioration of general health, as compared with the steady increase of symptoms in the former, with night sweating, fever, and rapid emaciation. The question of operative interference must depend, therefore, on how far it can be ascertained or reasonably suspected that the opposite organ is sufficiently sound to carry on the whole eliminating process; and although this has led to want of success in many cases, there are still many recorded in which life has been prolonged and suffering relieved by the ablation of the organ, and many others in which diagnosis has been confirmed by exploration. Tumours of the kidney are not of very frequent occurrence as primary neoplasms, but they are occasionally found, and the diagnosis has to be made between them and other swellings, such as we have already considered. The majority of these growths are either cancerous or sarcomatous, but innocent neoplasms are occasionally met with. These may have affinities to some form of connective tissue, such as the fibrous or fatty, but one curious and almost unique form of tumour has been met with in this region, viz. one containing striated muscle fibre, and these tumours are supposed to arise in the parts surrounding the gland as an aberrant portion of the tissue from which the spinal column is developed. The malignant tumours are of much more frequent occurrence in the young, are usually confined to one side, and

are rapidly fatal. The diagnosis of tumour is often extremely difficult, and cannot be in many cases determined until an exploratory incision has been made; the difficulty is the greater in that, so far as I have seen, these tumours, although growing rapidly and attaining enormous size, may exist for a long time without causing great distress or emaciation, and this, added to the fact that often little or no indication is afforded by examination of the urine; indeed, it is not until the growth has invaded the pelvis or ureter that bleeding takes place, and it is then often very severe, and is independent of exercise as opposed to the case of calculus, and is not accompanied by severe pain. Very rare, then, must the cases be in which removal of a kidney tumour can be successful in every way. Firstly, there is the fact that a large proportion of these neoplasms are malignant; secondly, there is the difficulty in distinguishing between them and the fluid collections which we have considered; and thirdly, there is the fact that such growths have usually attained a large size before they are detected, and connections have therefore become established with neighbouring parts, and internal organs are infiltrated with secondary deposits. Still, with all these objections, there need be no hesitation as to the propriety of making early an incision by which the nature of the growth can be investigated, and if practicable its removal may subsequently be effected.

Now, having thus briefly reviewed the surgical diseases of the kidney, and the operations by means of which diagnosis may be confirmed or causes removed, let me recall to you some of the cases which have been under my hands, and which will illustrate what I have been saying.

A short time ago a woman of thirty-three years came to Dr. Bruce suffering from all those symptoms which are referable to a movable kidney. She was a Jewess and highly neurotic, and had had several pregnancies. The abdominal walls were flabby, and the right kidney could easily be felt, and could be moved upwards and downwards through a considerable arc. Examination increased the pain, which was at times said to be excessive. There were no urinary symptoms. She had worn various belts without benefit. After due consultation I performed the operation of nephrorrhaphy, using kangaroo tendons to secure

the organ to the fascia of the loin, and passing them through the substance of the cortical portion of the gland. There was some hæmaturia for a day or two, but this soon passed off, and the wound healed quickly. On getting up she resumed the belt, and for a long time expressed herself as much benefitted. About a year later she came to report that the pains had recurred; but I could not on examination find any cause for them, and the kidney appeared to be stationary.

The next case to which I would call attention is a woman at present in the Golding Ward. She was sent in from a dispensary by Mr. Collum, on account of the state of her urine, and on account of having recently passed three separate pieces of calculus. Two of these you can see. They appear to have been broken off another portion, and give the idea that they are casts of the calyces, and have been formed in the kidney. The patient is a delicate-looking woman of thirty-two years, and in early childhood suffered from hip-joint disease. On the inner side of the right thigh is the cicatrix of an abscess, and the hip-joint is completely ankylosed at almost a right angle to the pelvis. The left hip has also been diseased, and though there are no scars of abscesses, the joint is only capable of a very moderate degree of movement. The lordosis is extreme, and in consequence any examination of the loin is exceedingly difficult. Nevertheless she is married, and has had one child about a year ago. About February last she was a patient in Middlesex Hospital, under the care of Mr. Kellock, who performed nephrotomy upon the right kidney, and found it a mere sac with little of the secreting structure remaining. When she was admitted here, after passing three portions of calculus, she looked very ill, and was suffering great pain in the region of the left kidney. Mr. Kellock kindly came to see her with me, and told us the condition in which he had found the gland on the right side. The urine was acid, but contained a very large proportion of pus. No stone was found in the bladder, and the cystoscope showed nothing but a very inflamed mucous membrane, though, owing to the ankylosis of the thighs and the condition of the urine, this form of examination was not so satisfactory as could have been wished.

After learning the condition found by Mr. Kellock, the question of nephrotomy of the left

side became a very serious one, and had her pain continued it might have been necessary to perform it at all risks. But fortunately rest and treatment have for the present greatly relieved her. The pain has ceased, and there is less pus in the urine, but how long this may continue remains to be seen.

Most of you will remember the very successful case of nephrectomy for injury which was under the care of Mr. Wallis a year and a half ago, which has been published in the 'Transactions' of the Clinical Society. The man was aged twenty-two, and he had fallen upon some railings, a spike from one of which had torn the right kidney almost completely across. The kidney was immediately removed, and the patient made an excellent recovery.

A woman who came up to report herself a few weeks ago is a very satisfactory case of recovery after removal of the kidney for strumous pyelitis. When twenty-seven years of age she was under my charge for this affection. In April, 1896, I removed the kidney on the right side, and found it to present the typical appearances of tubercular disease. Her recovery was delayed by reason of some tubercular ulceration occurring at the edges of the wound and at the site of some of the stitches, but after these had been scraped and swabbed with a solution of zinc chlor. the wound healed soundly, and she recovered perfectly, and has since been pregnant, and was carefully watched by Dr. Routh "for any evidence of failure in her renal functions, but no albuminuria was found at any time. Labour was induced at the thirtieth week owing to an inflamed lump, probably a tubercular gland, or perhaps a tubercular ureter, in the hollow of the sacrum, which was seriously reducing the available obstetric diameter of the pelvis. She convalesced normally." ('Clin. Journ.,' November 30th, 1898.)

A stout florid patient in the Alexandra ward, under the care of Dr. Bruce, presents all the symptoms of a large tumour of the left kidney. He has lost a good deal of weight recently. There are no urinary symptoms. Nothing but pure blood came away as the result of a puncture which I made the other day, and we agreed to watch the case.

The boy aged seven years who is now in the Albert Edward Ward, has several times had hæmaturia, with pain in the region of the left kidney. Neither by aid of the Röntgen rays nor by mani-

pulation under anæsthetic can any calculus be detected, nor since admission have any symptoms been present, although we have sent him out for runs in the park and for rides in omnibuses. I am therefore discharging him; but Mr. Goldney, who sent him to be under my care, promises to watch him, and let me know if the symptoms recur.

Recently you have seen Mr. Bloxam operate upon a patient in whom a stone in the ureter was shown upon the screen by the Röntgen apparatus.

In our museum you will see preserved the twenty stones which I removed from the left kidney of a man aged forty-three, and whose case is described in the 'Lancet' of September 12th, 1891. He had suffered from symptoms of calculus since childhood. At twenty-three he passed a small stone after pain in the right kidney. Three years before admission he suffered pain in the left loin, but there was no hæmaturia. On July 4th I performed the operation of nephrolithotomy, and extracted one large mulberry calculus and nineteen smaller ones, the larger of these weighing 141 grs. He recovered in twenty-five days, and I have lately received a letter from Mr. Bunn, who placed him under my care, saying that up to the present he remains well and follows the occupation of a collector, which necessitates a good deal of walking.

A QUESTION in the diagnosis of perforative pneumothorax often arises, whether one has a valvular or free opening, or, if the case is seen after this condition has been present for some time, whether there be any communication remaining between the bronchus and the pleural sac. Some have urged that one can decide upon this question by observing the character of the breath-sounds. If one hears the inspiratory murmur and the expiratory murmur one must conclude that the air enters and leaves the pleural cavity. It would appear, however, from the history of several cases that this is not reliable. Such auscultatory findings show at most, perhaps, that air enters the lung involved, and doubtless the changed character of the respiratory murmur is a product of collapsed lung and resonating chamber—the pleural sac.

HAMILTON, *Montreal Journal*, Dec., 1898.

MEETING OF THE SOCIETY OF ANÆSTHETISTS,

At 20 Hanover Square, December 16th, 1898;

The President, Dr. DUDLEY BUXTON,
in the Chair.

Discussion on "The Choice of an Anæsthetic."

(Continued from p. 206.)

Mr. ARTHUR E. BARKER said that he did not think, in response to the President's very courteous invitation, that he could contribute anything that was really worth the consideration of a society of specialists. He certainly had no accurate statistics, and he would feel it out of place if he were to enter into the physiological side of the question. But perhaps some impressions gathered from a fairly long hospital practice and a good many operations might not be without some value. First of all he would wish to endorse what Mr. Owen had so pointedly said in regard to chloroform. He had said it far better than he could say it, and in a far more amusing way than he could venture to hope for. He had for a long time past had the feeling that he would be perfectly satisfied if his patients had chloroform. Over and over again in the past, and at the present time, he was very much dissatisfied when his patients had ether. That, of course, was but an impression. One had to look at the matter from two points of view—from the anæsthetist's point of view, and from the surgeon's point of view. And if he might be allowed to say so, speaking freely amongst them there, he thought the anæsthetist often viewed the matter a little too much from the anæsthetist's point of view, and perhaps they gave the surgeon's point of view a little too slight amount of consideration. From the surgeon's point of view, next to the safety of the patient his great aim was to have perfect control over the area of operation; he was thinking of the quiet and rest of the patient generally, and of the rest of the part on which he was operating in particular. The anæsthetist was also inclined to put in the first place the intrinsic risks of the chloroform. But speaking as an operator he would himself very much prefer chloroform, whether one was operating upon the abdomen, upon the neck, upon a joint, or any

other part, and whether the patient was young, or whether he was old. Of course he admitted perfectly that the anæsthetists had a good ground for many of their objections to chloroform; that went without saying, but the impression was growing upon him that it was safer to work with chloroform, looking at the matter all round, because in reckoning up the dangers they had to consider those which arose from the surgeon—and the surgeon was a very dangerous person undoubtedly to the patient, probably a greater danger than was the anæsthetist, and if the surgeon's difficulties were enhanced, if he had a restless patient to work upon, he might commit very serious blunders with his hands, and he also might lose that *sang froid* which was so very necessary for the proper conduct of a large operation. The impression which was left upon him was that with ether they did not get the rest and the quiet which was so desirable, even when the anæsthetist was a skilled specialist. Certainly, if he were selecting an anæsthetic for an operation such as Mr. Owen had mentioned, he would select chloroform, although he detested the taste and smell of it very much indeed; it made him sick for nearly a week after he took it, and it was far more unpleasant for him to take than nitrous oxide,—he had had both chloroform and nitrous oxide over and over again. He would prefer chloroform, because he had a feeling that it was safer, and it gave the surgeon a freer hand, and left him unembarrassed by the movements of the patient; the hawking and coughing and general restlessness certainly, to his mind, arose much more frequently after ether than after chloroform. As to the view which Mr. Owen took about cocaine, he would not say that he could go quite as far as Mr. Owen had. His own feeling was that there was a great future for cocaine. [Mr. OWEN interposed that he alluded to children in this connection.] Mr. Barker said that being so he was at one with him entirely. One had to consider the emotional effect of the operation, and a child was not a very convenient person to handle when he saw the surgeon going at him in a white jacket and instruments in his hands, for under these circumstances he lost his self-control. For that reason he thought it would never come into use for children. For general surgery, however, he thought it would have a wide application in the future, and he hoped it would be taken up and

developed by this Society, who were best qualified to develop that branch of anæsthesia, and to estimate its value. He did not see why they should not take it up, and teach them a great deal about it.

He was exceedingly grateful to the President for his very kind invitation to him to take part in the discussion; he only wished he had had something of more value to contribute. He was also very grateful to members of the Society for the kind way in which they had listened to one who could really only furnish material from the purely surgical side of the case.

Mr. HERBERT ALLINGHAM said he really came to the meeting to learn, and he hardly liked to occupy the time of the Society with the few remarks which he would make in regard to the matter under discussion. He hoped Mr. Owen and Mr. Barker would not think him rude if he entirely disagreed with them on the question of chloroform *versus* ether. He had the greatest faith in ether, and as far as he could see he—merely as a surgeon and looker-on without any special knowledge of anæsthetics—thought the reason there were difficulties in some of these operations where ether was administered, was because too many anæsthetists choked their patients, if he might say so, first of all with gas, and then turned on the ether. They were never quite rid of the gas, and were always in an uncomfortable condition while the operation was going on. It had struck him that those anæsthetists who used ether with very little gas, very soon got their patients in a comfortable condition, such patients remaining perfectly comfortable and quiet for the operator for any length of time. He was speaking more particularly of rectal operations. Chloroform in rectal operations seemed to be an extremely dangerous drug, for unless patients were very thoroughly under the anæsthetic for such operations, especially for dilating the sphincter or anything very severe, they gave a rectal cry and took in an enormous quantity of the drug. When that drug were chloroform, he had seen very serious and uncomfortable results, although fortunately it had been possible to correct them, but if the matter had gone a little further it might have been still more uncomfortable. Therefore, speaking especially in connection with rectal operations, he felt very strongly a preference for ether as against

chloroform. Of course, with regard to an anæsthetic for abdominal operations he quite agreed with what Mr. Owen and Mr. Barker had said, that it was infinitely more comfortable for the surgeon to have his patient under chloroform anæsthesia, because he was perfectly quiet, there was less breathing effort, the patient was more comfortable, and there was less movement about the intestines, the abdominal muscles were not rigid, and everything could be done in the most comfortable manner. Taking the matter all round, he felt most strongly in favour of ether as against chloroform.

Mrs. SCHARLIEB said she simply came hoping to learn a great deal from what would be said in the discussion; but since the President had been good enough to ask her to take part in the debate, and give her experience in the matter, she must say that if she had any choice in the form of anæsthetic to be given it was in favour of gas and ether. In the operations which she had done on the abdomen, vaginal hysterectomy and so on, the patients had been beautifully quiet, and the operation had been in no way interrupted by their movements. If the anæsthetic was given by her friends in the hospital, and the patient was got completely under, the anæsthetic was given deliberately, and nothing was hurried; and if the surgeons would wait until the patient was thoroughly under the anæsthetic nothing could be more comfortable and desirable than ether.

Mr. TYRRELL said he had little to contribute. Mr. Allingham had regretted disagreeing with Mr. Owen and Mr. Barker. On his part he had the pleasure of agreeing with Mr. Owen, Mr. Barker, Mr. Allingham, and Mrs. Scharlieb! He certainly did prefer ether for Mr. Allingham's operations—rectal operations. He thought it was *the* anæsthetic for those particular operations. Also when he had to give an anæsthetic for vaginal hysterectomy he preferred ether. It seemed to be pretty well known that he was partial to chloroform, and he probably gave more chloroform than many anæsthetists. On the other hand, Mr. White had always been well known as an able etherist, and it had very often happened that when a surgeon at St. Thomas's specially wished to have ether administered, he had asked for Mr. White, and if he specially wished for chloroform he had asked for him (Mr. Tyrrell), for he knew he would get it

then. With regard to one or two points in Mr. White's paper, he would just air one of his fads again, namely, that when the patient did go downhill, as Mr. White had said, after three quarters of an hour or an hour, he had a very strong belief that it was not the ether which caused it, but the absence of oxygen. It was not the absence of oxygen which made a patient blue, but it was the chronic absence of oxygen which made them grey and made the pulse very quick and shallow. He had a very strong opinion that in patients with comparatively weak hearts the ether acted like a whip to the heart, and that whipping was then performed under the most unfavourable circumstances, quite an insufficient amount of oxygen being allowed. He believed the President of the Society had been giving oxygen, as he had himself, for many months past, and he hoped they would have some definite record to publish some time soon.

With regard to operations for removal of glands in the neck, he believed that the sudden failure in the pulse which sometimes occurred while the glands in the neck were being operated upon was probably due to interference with nerves, and it was chiefly that reason which led him to use his double-bottle method, by which he could always turn on a little ether. By that means he had perhaps given a little chloroform to begin with, and then a little ether, and then the patient would be kept going in that manner for one and a half to two hours without any untoward results which they used to hear of when giving chloroform. When he had read a short paper on the double-bottle method of giving it through the Junker's inhaler, Mr. White had said he did not believe he (Mr. Tyrrell) got any good out of the ether; it was only a salve to his conscience. But he could assure him that it had often occurred that after giving chloroform for half to three quarters of an hour he had been able to keep the patient going, if he was inclined to be faint, with only ether through the Junker's inhaler. The anæsthetists did labour under a good deal of disadvantage sometimes, because they got the message to give chloroform or to give ether, as the case might be, very frequently for surgeons without any particulars as to the case at all. He knew an eminent surgeon who hated the sight of chloroform, and if he thought the anæsthetist were going to give chloroform he would not send for him. Yet that gentleman told him (Mr.

Tyrrell) recently that he had to do a gastro-enterostomy, and that the patient took ether, but he took it so badly that the abdomen went up and down to such an extent as to make him quite seasick.

Dr. SILK thought the discussion which had hitherto taken place was rather unfortunate, in so far as it seemed to have rotated round the old question of the preference for ether or for chloroform in operations generally. He would have imagined that the great object of the discussion that evening was not a question as to the general utility of chloroform or ether, but in order to help them to arrive at some conclusions as to whether there were any conditions which might determine the choice of the one anæsthetic in preference to the other under certain circumstances. Of course, it was perfectly obvious that the great element must be the question of skill and practice, and it was quite impossible to lay down rules which would be universally applicable both to the specialist who was constantly using anæsthetics and to the general practitioner, who probably had given little, if any, anæsthetic at all. So that to try to determine whether chloroform or ether was best in all cases was, it seemed to him, a wild-goose chase. There were, of course, very various determining factors, to his mind, in the choice of an anæsthetic. There was the question of the duration of the operation. For very brief operations it was obvious that nitrous oxide was preferable to any other, but that would not apply to longer operations. Again, the position of the patient was a question which must largely determine the anæsthetic which should be chosen, because if the sitting posture were required chloroform could not be given; that circumstance alone put that particular drug out of the field. Another determining factor was the age of the patient. They had had the advantage of Mr. Owen's great experience in that matter, and Dr. Silk was very much inclined to agree with Mr. Owen in preferring to give children under three chloroform. Coming to the age of three to twelve, looking at the matter entirely from the anæsthetist's point of view, his preference would be for the A.C.E. mixture. That mixture had been condemned rather severely that evening, but he could not quite see the grounds upon which the condemnation had been urged. Possibly, theoretically, the constituents were given off in unequal proportions, but practically it had been

used very largely, and from his own personal experience he would distinctly claim for it that it had this great advantage, that there was none of that sudden syncope, those dreadful attacks, which one occasionally saw or heard of when chloroform was the anæsthetic used. That he believed to be due to the stimulating effect of the ether. No one could doubt, of course, that the proportions sometimes varied, just as by Mr. Tyrrell's double-bottle method, but by repeated small doses this variation was reduced to a minimum. He would say that from the age of twelve to sixty was the preferable period for ether. Over sixty he would prefer to give either A.C.E. mixture or chloroform, because the arteries were very much changed, and there was a greater tendency to venous congestion. The next, and probably the most important determining factor in the choice of an anæsthetic was the condition of the patient. It seemed to him that there were several points here which were of great importance in choosing an anæsthetic. If the patient were fat and plethoric he could not be given the same anæsthetic, other things being equal, as if he were a thin spare man. For fat plethoric people he thought something in the way of A.C.E. mixture was decidedly to be preferred, but he thought the proportion of ether might be increased as the operation went on. Then there was that very large question of lung troubles, acute or chronic. Acute or recently acute lung trouble rendered the administration of ether, from the point of view of the anæsthetist, quite inadmissible, and he would say that chloroform was distinctly the drug to be chosen. Coming, however, to chronic lung troubles, in which he included emphysematous conditions, with perhaps a winter cough, but in which the condition was not acute when placed upon the operating table, those were the patients in whom he thought one could commence with A.C.E. mixture, and add larger proportions of ether as the patient appeared to tolerate it. With regard to organic heart disease, there he differed somewhat from the introducer of the discussion; his own experience with regard to those cases was that with the organic disease in which there was not a sufficient amount of compensation he certainly would not like to give ether. Unfortunately he had given it, at any rate once, to a patient in that condition; the patient did not die under the anæsthetic but a day or two after, and

although the patient was very ill at the time of the operation, Dr. Silk had it on his conscience that he accelerated the end. On the other hand, in organic heart disease when there was sufficient compensation there might be no objection to giving ether, other things being equal. Very much the same remark applied to conditions of marked atheroma, and it applied especially to cases over sixty. Patients with marked atheroma did not take ether well to his mind. Such patients exhibit all sorts of troubles when ether is given, and therefore it was better to administer A.C.E. or chloroform. With regard to renal diseases, that was a question which had been more or less debated, or discussed, or alluded to in this Society on more than one occasion, and there seemed to be an opinion generally that there was no reason which could be definitely stated capable of precluding the use of ether in renal cases. That was an opinion with which he did not at all agree. He thought that renal disease was distinctly a condition in which the ether should be avoided, and by preference A.C.E. mixture should be given. He had in his mind's eye a case in which he gave ether, or ether was given, and in which the patient had hæmaturia for a day or two afterwards, the attack having come on very rapidly after the operation, although the operation was on the neck or at a spot far removed from the pelvis. Another great question was the nature of the operation to be performed, and that factor could not be neglected in considering what anæsthetic to choose. That was the point of view which had been taken very largely by speakers hitherto, and although one admitted its great importance, he did not think it was the only question; the patient's condition, age, habits, and so on had to be reckoned with. In operations upon the head and neck he had always preferred to use the A.C.E. mixture to introduce anæsthesia, increasing the proportion of ether if the operation was at all a prolonged one, his reason for that being that with ether he had found it difficult very often to avoid the congestion into which the tissues of the neck were thrown by the ether on some occasions. In intra-cranial operations—operations within the skull in contradistinction to those outside—he preferred to give chloroform, and he liked to precede it by a small amount of morphia, one sixth to one fourth of a grain. For operations on the tongue and mouth he would commence

with A.C.E., and go on with the chloroform afterwards. Under that heading he supposed one should allude more particularly to one's practice in the matter of operations such as those alluded to by Mr. Owen—cleft palate and operations for removal of adenoids. For cleft palate operations he confessed that from the anæsthetist's point of view he did not like the dependent position of the head, though he knew it was a position the anæsthetist had to put up with and make the best of it. In those cases he always liked to administer the chloroform through a small tube out of a Junker's bottle. It had been stated by a very well-known authority on the subject that this use of the vapour of chloroform was apt to retard the healing of the wound. That must be a matter for the surgeon, and was a matter on which the anæsthetist could hardly express an opinion. From the theoretical point of view he did not see, given equal degrees of anæsthesia, how it should be better to give it off the corner of a towel or piece of lint than out of a tube. In operations on the big joints he would like to give ether; and, in fact, in all operations which involved much shock. This would apply to rectal and genito-urinary operations, and upon that point he was glad to find himself in agreement with so great an authority as Mr. Allingham.

He now came to speak of abdominal operations, and he had by preference left these to the last because, to his mind, they were very much on the borderland. His own advice to the student always was that in abdominal operations they must pay more than the usual amount of attention to the opinion of the surgeon; they must not let the surgeon have, or imagine he had, anything to complain of in the matter of the anæsthetic. Abdominal operations, speaking still from the anæsthetist's point of view, might, he thought, be roughly divided into those which did not involve the peritoneal cavity, like lumbar colotomy, those which involved it by a diverticulum like a hernia, and those which involved the whole peritoneal cavity like an ovariectomy. Those were the three main types. He had very much hesitation in expressing opinions about them. It seemed to him that the lumbar colotomy type might and should have ether, because they were operations involving great shock. In the hernia type of operations if ether could be given it should. With regard to laparotomy operations, he thought

they were in somewhat of a transition period. They all remembered the time when the surgeon or the ovariectomist made an enormous amount of what might be termed fuss about abdominal operations—special rooms, special nurses, special beds, special everything. Now-a-days one often saw ovariectomies done in the theatres of hospitals. He merely mentioned that in order to emphasise what he believed to be the fact, that they were in a similar condition with regard to the choice of anæsthetics in these operations. At one time it was thought that chloroform was the only anæsthetic possible for laparotomy or ovariectomy. No doubt there must be something in the view of the surgeon that the abdomen was quieter, and that the patient was quieter generally, and that therefore chloroform was better than ether. Although that was the view of the surgeon, he believed there were many anæsthetists present who had themselves administered gas and ether in fitting cases of ovariectomy in which there was no cause given for complaint, and everything went right. But of course they could not expect to get a six-inch tumour out of a two-inch wall, and that to his mind was the reason for a lot of the objections from the point of view of the abdomen not being relaxed enough. The surgeon was ambitious to get a large tumour out of a small hole, and because he could not do it he was apt to blame the anæsthetist. Of course, there were abdominal conditions other than those alluded to, like those of distension, vomiting, and regurgitation. He did not know that he had any personal experience to give in regard to them. Broadly, he would say that these were conditions in which the great aim should be to give as little of the anæsthetic as possible; and that being so, his argument would be that they were cases for chloroform or a mixture in preference to ether.

Mr. HENRY DAVIS proposed that the discussion should be adjourned, as no doubt there were many others who would like to say something on the matter, which would be impossible on the present occasion owing to the lateness of the hour.

THE PRESIDENT said it seemed to him most undesirable that they should rush through a discussion of such importance in one night, more especially as he thought they might have some others who would be very pleased to be present on another occasion, and continue the discussion from

the surgical side. They had had some frank speaking from both sides, which no doubt would help forward the good cause of unearthing the truth, hauling it "out of the well" in which it so often lay. He hoped the motion for adjournment would be seconded and carried. He had in his possession several letters from surgeons whom he hoped to see present if they continued the discussion. He had also a letter from Dr. Savage, which he might read before closing, which showed that the subject had a great deal of interest not only from the surgical, but also from the medical point of view. After expressing his regret at being unable to be present, Dr. Savage said, "I fear I could not say from personal experience that I know of one form of anæsthetic which is preferable in states of mental disorder, but I have come to certain very definite conclusions. First, any form of anæsthetic may in some few predisposed subjects start insanity of one form or another, the common forms being maniacal or associated with mental confusion or weakness. Next, most insane persons will bear anæsthetics well, but there is danger of starting attacks in those convalescing from forms of insanity, in those subject to recurring attacks, and those belonging to very alcoholic or neurotic stocks."

Mr. AUGUSTUS COOK said he had much pleasure in seconding the motion that the discussion be adjourned, and especially in supporting Mr. Owen's suggestion that something more should come of the discussion than simply a pleasurable evening, from the appearance of interesting articles in the papers. If some practical outcome could result, such as some authoritative statement drawn up from the majority of a sub-committee or the whole meeting, it might be regarded as laying down a canon, for all anæsthetists who were not able to be present, such as those in the country; he thought great and lasting good would result.

The resolution was then put to the meeting and carried.

The PRESIDENT said it became his pleasant duty to thank the visitors for their attendance that evening, and he did that most heartily because he felt most strongly that the speeches, such as that of Mr. Owen, could not but do the greatest amount of good. Mr. Owen had spoken most frankly, and at the same time most kindly, and he was sure that those in the room who disagreed

with him most would feel most cordially the pleasant way in which he had set forth what he regarded as the deficiencies in anæsthetists. If all their surgical friends would be equally frank he believed that even the anæsthetist might find salvation.

The meeting then adjourned.

CHAPTERS FROM THE TEACHING OF DR. G. V. POORE.

No. XIV.

GENTLEMEN,—I have shown that the strong fact in connection with phosphorus poisoning is fatty degeneration, which is practically universal, and that although it is most strikingly marked in the liver, it is still observable in the heart, in the voluntary muscles, in the kidneys, and in the blood-vessels. Now the question is, Why do you get fatty degeneration when a person takes phosphorus? That is a question which I do not think we are able to answer. Fatty degeneration is only caused by fuming phosphorus, and whether the fact of the fatty degeneration is in connection with the greediness of the phosphorus for oxygen is not solved. I should think, however, that it is a very unlikely explanation. Phosphorus, like arsenic and antimony, combines with hydrogen; and phosphorus, arsenic, and antimony are chemically allied, and all three of these bodies seem to have the power of producing fatty degeneration. That is a matter of great interest.

The next question I will ask is, Why does phosphorus cause jaundice? I take it that jaundice from phosphorus poisoning is a real obstructive jaundice, that phosphorus gets into the liver, and you get a swelling of the liver and obliteration of the bile-ducts in that organ, and the bile reabsorbed into the blood. You must remember this in regard to the jaundice of phosphorus as against other kinds of jaundice, that the jaundice of phosphorus poisoning is the last biliary offering of an expiring liver; in a very little time the whole liver cells undergo fatty degeneration, and the bile-forming function of that liver ceases to go on.

The next point—also a very important one—is that phosphorus poisoning very closely resembles a

rare disease of which you have read, and which, I take it, you are certain to see but seldom, namely, *acute yellow atrophy* of the liver. Clinically the two are indistinguishable, or I may say that some cases of phosphorus poisoning and some cases of acute yellow atrophy of the liver are indistinguishable. There is the same jaundice, the same insidious course, the same absence of rise of temperature, the same delirium before death, and the same almost inevitably fatal result. It is absolutely certain that many cases of phosphorus poisoning have been reported as cases of acute yellow atrophy of the liver: there can be no doubt about that. A case occurred some years ago at Netley Hospital, and if anywhere in the wide world they understand liver diseases, having patients from the tropics in every part of the world, they understand them at Netley. One of the *employés* about the gardens at Netley Hospital died, it was said of acute yellow atrophy of the liver, and it was not until after the post-mortem examination, and after the case, as far as the pathology was concerned, was considered closed, that they found out that he had taken phosphorus paste. In short, it was a case of phosphorus poisoning, not acute yellow atrophy of the liver. A similar case occurred in our hospital here not very many years ago, and a case occurred to Dr. Leonard Hill, a former student of this College, who is now Lecturer on Physiology at the London Hospital. The patient was a girl, who died of "acute yellow atrophy," and on making inquiries he found that she had been playing tricks with her friends, and had given some sort of "spiritualistic" entertainment, and had rubbed her face with phosphorus paste to get luminosity of countenance.

Now it is said that in acute yellow atrophy the liver is always small, and that in phosphorus poisoning the liver is always big. That is not true; certainly not. Many cases of so-called acute yellow atrophy have been reported in which the atrophy of the liver has been slight, if present at all; and, on the other hand, there are many cases of phosphorus poisoning where the liver has been reported small. Though I do not wish to dogmatise on the matter, and although the fact is doubted by some, I am inclined to think that the most probable explanation of that is the time which the patient lives. If the dose has been a big one, and the patient dies in the early days of

the jaundice, you may find the liver big, because it has not had time to atrophy; but if the patient continues to live after the blocking of the ducts, and the fatty degeneration of the cells sets in, the liver atrophies rapidly and becomes small.

The next point is this. It is said that in acute yellow atrophy of the liver the urine contains leucin and tyrosin, but in phosphorus poisoning it does not. I have a very interesting tale in connection with that.

In the case of which I am telling you I made the post-mortem examination. The patient I saw during life; we got some of the urine, and in this urine were a number of fine star-shaped crystals. The urine was very acid, and I looked at it and said to myself, "I wonder if that is tyrosin." I do not think I was to be blamed for saying "I wonder if that is tyrosin," because tyrosin is the rarest of urinary deposits. I remembered the pictures in the books. I went off to a urinary expert, and took the sample with me, and I said, "Look at that urine. What is it?" He said, "That is tyrosin undoubtedly." But it was not. Now that is rather interesting in connection with expert evidence. The deposit was uric acid in the form of stellar crystals, such as occur sometimes in patients who are the subjects of diabetes; and it was an interesting fact about that case of phosphorus poisoning that the urine contained sugar. Although the urine did not contain tyrosin in this case, tyrosin was obtained from the liver in large quantities. To put it shortly, I do not think one can distinguish between "acute yellow atrophy" of the liver and phosphorus poisoning.

Now comes another point. You may say "acute yellow atrophy is a disease which occurs mainly in a particular class of people; it occurs mainly in women, and it occurs largely in pregnant women." It clearly has occurred, particularly in Vienna, in women of loose moral character. Quite true; those are facts which have been established by statistics, and they mark out acute yellow atrophy of the liver as a disease mainly affecting a peculiar class. The interesting point is that the statistics of phosphorus poisoning show exactly the same thing. The majority of the victims of phosphorus poisoning are women, the majority of them have been pregnant women, and a large number of these have been women of loose moral character. Why should women suffer more from phosphorus

poisoning than men? The reason is that phosphorus is a domestic poison, and many housewives troubled with blackbeetles have phosphorus in the house, and even if they have not phosphorus paste they have matches. And a woman is nothing if not impetuous, and when she makes up her mind to suicide she takes the first poison which comes handy, and does not think much of the consequences. That I believe is the reason for the undoubted fact that phosphorus is more used by women suicides than by men. You will find that men commit suicide by the more deadly poisons, and that a man who is bent on suicide will go and get prussic acid, or take a dose of laudanum, or drown himself, or shoot himself. A woman who commits suicide very seldom uses firearms.

Phosphorus has been largely used in procuring abortion, not so much in this country as on the Continent, and it has some reputation of being an aphrodisiac. In these two facts you will find another explanation of the preponderance in women, and a preponderance among women of a particular class. The identity of the statistics of phosphorus poisoning and acute yellow atrophy of the liver is very interesting.

Acute yellow atrophy of the liver was first described by Rokitsansky in Vienna in 1842, and acute yellow atrophy of the liver does not seem to have gained a footing until after the establishment of the lucifer match industry. It is a very interesting fact that we find the most accounts of acute yellow atrophy emanating from the town where the lucifer match industry was first established. I admit that Vienna was and still is one of the great centres of pathology, but still the fact is remarkable.

Another fact is this:—In Grave's 'Clinical Medicine' mention is made of acute yellow atrophy occurring in three members of the same family. I can double that in phosphorus poisoning. The case upon which my remarks have been largely founded, and which led me to investigate the whole subject, was a case which occurred twenty or thirty miles from London. The patient was a woman who was living under the "protection" of a gentleman in the country, and this gentleman was going to get married, and his *quasi*-connubial arrangement was to come to an end, which caused great trouble to the lady.

She accordingly took phosphorous and poisoned herself. I went down to see her before she died; and I had to make the post-mortem. This girl came of what I should call queer parentage; I suppose she had come of a family of prostitutes. And a very curious fact is that her own sister, under exactly parallel circumstances, had done precisely the same thing. That came out in the evidence before the coroner; and her mother, who was a woman of the same class, it was whispered but not proved, had also done the same thing. So that it was a tradition in this family. There I leave the matter, and please do not come to the conclusion that I say for a minute that there is no such thing as acute yellow atrophy apart from phosphorus poisoning. Phosphorus is an element which in a state of combination we carry about us in very large quantities, yet so little as a grain or two of pure phosphorus suffices to kill. But again we must remember that this acute fatty degeneration is after all only a symptom, and it is quite possible it may be brought about in more ways than one. I have said enough to make you careful when you have a case of acute yellow atrophy, to search about, as did Mr. Hill, for evidence of phosphorus poisoning, remembering that that evidence is often exceedingly difficult to get. I have told you that phosphorus is taken for unworthy purposes, and people do not admit a fact of that kind without a great deal of cross-examination.

Again, there may be such a thing as indirect phosphorus poisoning, *via* blackbeetles and rats, and through them to the animals we consume for food. That is a point which I think is well worthy of attention, and certainly it is a point which should always be inquired into when you are confronted with these cases.

(To be continued.)

Parasites of the Blood.—The claim is made by Dr. Löwit ('Centralblatt für Bakteriologie,' p. 206, 1898) that a protozoon has been found by him in the white corpuscles of leukæmia, and Grassi is reported to have demonstrated an amœba very similar to the malarial parasite in no less than eight cases of goitre. This parasite, it is stated, contains a red instead of black pigment.

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URÆMIA.

An Address delivered before the North London Medical and Chirurgical Society, January 12th, 1899,

BY

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F.R.S.,

Physician to University College Hospital; Professor
Superintendent of the Brown Institution.

MR. PRESIDENT AND GENTLEMEN,—I have first of all to express to you my best thanks for your kindness in asking me to address you, and to ask your indulgence if the views advanced and the observations on which they are made seem to rest on a too exclusive personal basis.

Uræmia may be defined as a toxæmic condition arising usually in cases of acute or chronic renal disease, but occasionally seen with the kidneys practically healthy. It is allied to other toxæmic states such as acetonæmia and cholæmia, and it is interesting to observe that the names of all these conditions are misnomers, inasmuch as uræmia probably in no way depends on the toxic action of urea, any more than acetonæmia is dependent on the presence of acetone or cholæmia on the mere poisoning of the blood with bile. Uræmia bears the same relation to renal disease that cholæmia does to hepatic disorder, and acetonæmia to pancreatic and hepatic affections. There are other analogies between these three toxæmic conditions which are of some clinical interest. It is universally recognised that acetonæmia and cholæmia are occasionally at the same time the earliest and the terminal symptom of grave disease which has up to that time been overlooked. Every physician knows that diabetic coma may be the first symptom of diabetes to attract notice, and diabetic coma is associated invariably with so-called acetonæmia. Similarly cholæmia may occur with great suddenness, not only in cases of obstruction of the biliary passages, but also in more obscure and latent diseases of the liver—such, for example, as cirrhosis. In like manner uræmia may be the first symptom that directs attention to serious and

very often fatal renal disease. Again, acetonæmia and diabetic coma are often brought about as a result of operative interference, perhaps not so frequently now as formerly, but still every practitioner is acquainted with cases where diabetic coma has supervened, and led to a fatal result either as a result of some trivial injury, or as some complication of surgical procedures in patients who, notwithstanding the existence of diabetes, were in good health at the time. I am not aware of cases of cholæmia arising in this way, but uræmia is not infrequently seen as a result of operative interference in cases of renal disease. Again, acetonæmia is known to arise as a complication of some comparatively trivial intercurrent illness arising in a diabetic, and uræmia may occur under similar conditions. Every hospital physician is struck with the frequency with which diabetic coma is apt to ensue in diabetics who have been sent into hospital from the country, and where the fatigue of a long railway journey is supposed to predispose to the onset of this grave complication. It is not uncommon to see fatal uræmia arise under similar circumstances. Finally, whether diabetic coma may or may not arise as a result of indiscretions in diet, it is, at any rate, certain that fatal uræmia may be so produced; so that we see that traumas, intercurrent diseases, fatigue, exhaustion, and changes of diet may in the diabetic lead to the production of diabetic coma, and in those suffering from renal disease to uræmia.

In discussing the subject of uræmia it will be convenient to consider it from the clinical, pathological, and the experimental sides, and first of all we will deal with the clinical.

It is most convenient to classify the varieties of uræmia clinically, owing to the very great variety in the symptoms produced in this condition. Strictly speaking there are probably five clinical varieties of uræmia, viz. the fulminating, the acute, the subacute, the chronic, and the latent; but for practical purposes we may be content with three main varieties—the acute, the chronic, and the latent.

Uræmia may also be classified according to the character of the individual symptoms seen. In this classification two types are recognised—that in which the nervous symptoms preponderate, and that in which the so called gastro-intestinal symptoms are marked. The nervous type corre-

sponds roughly with that form described as acute uræmia, and the gastro-intestinal type with that form described as chronic. This classification, however, is unsatisfactory owing to the fact that many of the so-called gastro-intestinal symptoms are really of nervous origin, and therefore the classification based on the course that the malady follows, whether it is acute, chronic, or latent, is the more satisfactory.

Acute uræmia.—Acute uræmia is characterised not only by the severity of the symptoms but also by the suddenness of their onset and their rapid course. In acute uræmia undoubtedly the most common symptom is coma, or drowsiness rapidly deepening into coma, but even in the most marked forms of uræmic coma the coma is rarely as intense as that seen in apoplexy or in opium poisoning and in other conditions, and the patient can usually be at any rate partially roused. The coma is almost invariably accompanied by some disorder of respiration. Most usually the respiration is of the hissing character described by Addison as distinct from the stertorous character noticed in apoplexy. This hissing respiration is very similar in its character to the respiration seen as the result of the administration of very large doses of salicylate of soda. Not uncommonly the dyspnoea is of the Cheyne-Stokes type, and usually of the true Cheyne-Stokes; that is to say, a state of periodic respiration where the change from the violent breathing to the quiet breathing is gradual and not sudden as in the periodic respiration so frequently seen in cerebral compression. In the Cheyne-Stokes respiration associated with uræmic coma, it is common for the periodic rhythmic discharge of nerve centres not to be limited to that of the respiratory centre, so that in addition to the waxing and waning of the activity of the respiratory centre there is also a waxing and waning of the activity of other centres, more especially those controlling the movements of the pupil and those nervous centres connected with the rhythm of the heart. Further, the activity of the excito-motor centres of the brain undergoes periodic variations, so that in a typical case of Cheyne-Stokes respiration in uræmia, during the period of dyspnoea, the pupil is seen to dilate and the pulse to quicken, and the patient to become restless and to carry out more or less purposeful movements. It is interesting to note that the changes in the pupil, in the

pulse rate, and in the activity of the motor centres, do not coincide absolutely in time with the variations in the respiratory rhythm; so that, for instance, the pupil often dilates just previously to the onset of the dyspnoea, and by watching the pupil it can be ascertained that the period of dyspnoea will shortly commence. These two forms of dyspnoea, the hissing and the Cheyne-Stokes, are those most frequently seen in acute uræmia, but occasionally a third variety of dyspnoea, characterised by being paroxysmal in its onset and more or less transitory in its duration, is seen, frequently known as uræmic asthma. This form is, however, very frequently seen in uræmia apart from coma, and the same remark applies to a modified extent in all cases of Cheyne-Stokes respiration. Further, this asthmatic form may be the only prominent symptom in some cases of acute and fatal uræmia. The most frequent type of disturbance of the respiration seen in acute uræmic coma is undoubtedly the hissing variety. *

Although coma is by far the most frequent disturbance of the nervous system seen in uræmia, there are others of considerable interest. Epileptiform seizures are common, as is well known, but in my experience they are not so common as coma without seizures. The epileptiform seizures of uræmia differ but little, if at all, from epileptiform seizures due to other causes, and it is of considerable interest that epileptiform seizures in uræmia not uncommonly begin locally, and may be even confined to one side of the body. Hence, if they are produced by the action of a poison, this poison not only has a selective action on certain nerve-cells, but it may only have that action on the nerve-cells of one cerebral hemisphere.

The fact that epileptiform seizures in uræmia may be unilateral, or even only confined to one limb, is not only of theoretical interest but introduces very considerable difficulties in diagnosis, inasmuch as when fits, local in their origin or confined to one part, are seen they necessarily suggest a local gross lesion as their cause.

Uræmic epileptiform seizures, like other epileptiform fits, are liable to begin in the movements of the small muscles, such as those of the eyes, the face, the hands. There is therefore a certain amount of evidence that the centres for small and highly developed movements are not only more excitable to mechanical stimuli, such as those

produced by a blood-clot or a tumour, but also that they are more excitable to chemical stimuli, such as the circulation of a poison in the blood. It is well known to the physiologist that these centres are more excitable to electrical excitation than those innervating the coarser movements.

Uræmic epileptiform seizures reach their highest development in the condition known as eclampsia, but eclampsia in many ways differs from the uræmia of renal disease, and I do not propose to discuss its nature to-night.

Delirium is, of course, a common phenomenon in uræmia, but rarer mental derangements in this condition are maniacal excitement and catalepsy. It is important to recognise that violent maniacal excitement is by no means an uncommon phenomenon in acute uræmia complicating certain forms of chronic renal disease, and it may be an initial phenomenon, and then the nature of the case is liable to be overlooked. It is well known in asylums that granular kidneys are often found associated with various forms of insanity, but quite apart from this violent mania is seen as a phenomenon of uræmia in patients previously perfectly sane.

Catalepsy is also not very rare in uræmia, and two marked instances have fallen under my own observation. In one of these, in a young man, the patient was admitted owing to his cataleptic condition.

It is clear, therefore, that a great variety of nervous symptoms may be met with in acute uræmia. Sometimes these are psychical, as, for instance, when mania is present, and in others the motor centres are excited, as where epileptiform seizures are produced; in others where coma is seen, the activity of nerve-centres is diminished.

In these respects uræmia resembles the action of many poisons, as it is well known that one and the same poison may under different circumstances produce different effects, and lead amongst others is a striking instance of this.

There are many other effects produced in acute uræmia, as, for instance, twitchings of the muscles, more especially those of the extremities, sometimes sufficiently severe to cause cramp; and I have known of a patient who died of acute uræmia, and whose first symptoms only forty-eight hours before his death were severe cramps in the calves of the legs. It is perhaps more usual to

see twitchings of the extremities, especially of the hands, in chronic or even in latent uræmia, but they are undoubtedly present in some cases of acute uræmia.

Hiccough is also a marked symptom of acute uræmia, and I have known of one case which was rapidly fatal in some thirty-six hours, and where it was not only the earliest symptom, but where throughout the duration of the illness it was the only prominent symptom apart from dyspnœa.

It will be noticed that all the symptoms described hitherto as present in acute uræmia are either disturbances of the higher cerebral functions, as, for instance, mania, delirium, or else are due to interference with the activity of the motor centres, as, for instance, fits, hiccough; and it is remarkable that disturbance of the sensory functions of the nervous system are by no means so common. In fact, the only one that is at all common is amaurosis, and this, although very striking when present, and especially when of sudden onset, yet is not in my experience a very common manifestation.

The activity of the motor centres in uræmia may not only be increased so as to lead to the occurrence of fits, but it also may be diminished so as to cause palsy, and that again is one of the most remarkable effects of this toxæmic condition. Thus hemiplegia, or even a monoplegia without the previous occurrence of any epileptiform seizures, is not very uncommon in uræmia, but personally I have only seen it in subacute or chronic uræmia.

The body temperature in uræmia is usually, as in diabetic coma, subnormal, but there are two conditions which are apt to cause pyrexia; one is the repeated occurrence of epileptiform seizures, and under these conditions pyrexia, or even hyperpyrexia, is not uncommon without the occurrence of any inflammatory lesion to account for the fever. Some authorities hold that pyrexia, or even hyperpyrexia, may be seen in uræmia without epileptiform seizures and without the presence of inflammatory complications. In my own experience hyperpyrexia in uræmia has been always associated either with epileptiform seizures or with the presence of some inflammatory complication such as pneumonia, which frequently, however, has not been detected until a post-mortem examination.

Although inflammatory complications often account for the presence of pyrexia and hyperpyrexia

in uræmia, it must not be forgotten that in renal disease and in uræmia serious inflammatory complications, and more especially pericarditis, may exist with a subnormal temperature and also without any symptoms drawing attention to their presence.

Chronic uræmia.—In chronic uræmia, although twitchings of the voluntary muscles and disturbance of the respiratory rhythm, as mentioned above, and a subnormal temperature are prominent symptoms, yet the most prominent symptoms are those to be referred to the alimentary canal, such as nausea, vomiting, hiccough, and diarrhœa.

In many cases, no doubt, the vomiting and diarrhœa are dependent on local lesions of varying degrees of severity in the alimentary canal, but the nausea and the hiccough, and perhaps even the vomiting, are not uncommonly present without any gross lesions in the alimentary canal, and are very probably dependent on the action of poisons on the nerve centres.

Headache is, of course, also a marked symptom, but here we do not know to what extent it may not be dependent on the frequently accompanying lesions of the vessels of the brain, and although a prominent symptom of renal disease, and often associated with uræmia, it is doubtful whether it is really dependent on the toxæmia.

Cheyne-Stokes breathing is a very characteristic phenomenon of chronic uræmia, and every practitioner must have seen cases where it was present for weeks or even for months. I have known of a case where Cheyne-Stokes respiration and pericarditis, both of them serious phenomena in renal disease, have persisted for many weeks, although ultimately fatal.

Latent uræmia is from many points of view the most interesting form of uræmia. By latent uræmia I mean the condition that is associated most usually with complete obstructive suppression, and which was first described by Sir William Roberts. In this condition the patient presents few if any symptoms for many days thus usually such patients, although they have complete suppression, or at the most pass a few drachms of urine, are perfectly conscious and rational, often to within a few hours of their death. I have known such a case quite rational, talking sensibly, to within half an hour of death. The pupils are usually contracted, and the temperature is subnormal

The skin is usually dry, but slight œdema is sometimes seen. Apart, however, from these three facts there is little or nothing to draw attention to the very serious condition of the patient, and they themselves rarely realise their dangerous condition.

After the suppression has lasted with these symptoms for several days or even a week, or at the outside ten days, there may be some nausea and a little vomiting, and perhaps on careful examination a little twitching of the voluntary muscles, especially of the hands, may be seen. But there are few clinical pictures presented by disease more remarkable than the fact that such patients may live for a week or even ten days with complete suppression of urine, and without any further disturbance of their faculties than those just mentioned.

This condition of latent uræmia, although usually associated with complete obstruction due to calculous or other disease, yet is not restricted to this condition. I want to bring prominently before you the fact that complete suppression of urine dependent on interference of the circulation of previously healthy kidneys will also produce this same clinical picture of latent uræmia. In one case that was under my care some years ago thrombosis occurred in the renal vessels of both kidneys, and the patient lived for seven days without passing more than two drachms of urine. She presented the typical picture of latent uræmia, such as is seen in calculous obstruction. In another case complete suppression occurred apparently as a result of reflex interference with the renal circulation. This patient had gangrenous cystitis of unknown origin. The kidneys presented no sign of disease post mortem, yet complete suppression of urine existed for some five days, and during the whole of this time there were no manifestations of ordinary uræmia, the symptoms were those seen in calculous obstruction. It is a matter of great theoretical and practical importance that complete suppression, arising as a result either of obstruction of the ureters, or from interference with the circulation in healthy kidneys, is followed not by ordinary acute or chronic uræmia, but by what I have termed latent uræmia.

In uræmia, acute, chronic, or latent, the tongue is usually dry, brown, cracked, and furred, and the patients usually complain of an exceedingly

offensive taste and smell in the mouth. The skin is usually dry. Personally, I have never seen crystals of urea in the sweat as has been described by some foreign authors, more especially Bartels. The gums are usually spongy, and frequently bleed, so that the mouth, what with the swollen and bleeding gums and dry tongue, is often quite characteristic. A peculiar smell, often urinous, sometimes earthy, is noticeable from the body of uræmic patients.

The examination of the fundus oculi often gives valuable information in cases of uræmia. I have never seen a case of acute or subacute uræmia in chronic renal disease without the occurrence of albuminuric retinitis and papillitis, although the acute uræmia of acute nephritis may obviously occur without such retinitis.

Retinitis is also usually present in cases of chronic uræmia, but not, of course, in cases of latent uræmia, inasmuch as this last condition arises usually in patients with comparatively healthy kidneys.

Urine in Uræmia.—The urine in uræmia presents great varieties; usually, of course, it is diminished in amount, and complete or partial suppression may occur in all forms of uræmia; latent uræmia being invariably associated with practically complete suppression. Complete suppression, however, is by no means usually frequent in acute and chronic uræmia, and I have seen several cases of fatal acute uræmia where even in the last twenty-four hours of life considerable quantities of urine have been passed. Such patients are unconscious, and it is difficult to recover all the urine secreted, but there can be no question that acute uræmia may supervene whilst considerable quantities of urine are being excreted, and this is a point of considerable theoretical interest.

Exceptionally, fatal uræmia may be seen with the urine free from albumen. I have seen three such cases all confirmed by post-mortem examination, and in two of them I failed myself to detect any albumen or albumoses in the urine; in the third case the urine had been tested by another observer.

The blood pressure in uræmia is usually high, and it is not uncommon for uræmia to occur coincidentally with temporary increase in the arterial tension; so much so is this the case that bleeding

is strongly recommended by many not only as a means of treating, but as a means of preventing the onset of uræmia.

Rashes are not uncommon in uræmia; scarlatiniform, vesicular, papular, and purpuric are the most common.

Morbid anatomy of uræmia.—Uræmia may occur, as mentioned above, in almost any form of renal disease, and also occasionally in cases of reflex suppression with practically healthy kidneys.

Acute uræmia is, in my opinion, more especially associated with a small contracted white and more or less granular kidney, seen especially in young persons. This is the most common form of chronic renal disease to be associated with acute uræmia, but of course acute uræmia is by no means uncommon in acute nephritis or in acute Bright's disease. The large white kidney of the text-books is by no means so common in the post-mortem room now as formerly, probably owing to the great diminution in the frequency of waxy degeneration of the kidney; and hence the most common forms of chronic renal disease seen in the post-mortem room are (a) one in which the kidney is about the normal size, or it may be slightly larger, but in which the organ is white or mottled and the surface is fairly smooth. This kidney clinically is usually associated with the presence of dropsy and the secretion of a scanty, highly albuminous urine, and I would suggest that chronic uræmia is perhaps more often associated with this kidney than acute uræmia.

(b) The second variety of kidney is one in which the organ is considerably smaller than normal; it is white or mottled in colour, the capsule is thickened, and on stripping leaves a highly granular surface. This kidney clinically is associated with the secretion of a more or less copious urine, containing often a considerable quantity of albumen, and dropsy is commonly absent, and in many cases there is no history of the patient ever having had dropsy, or, indeed, of having suffered from any symptoms previously to the fatal illness. This kidney, in my opinion, is peculiarly associated with the production of acute or even fulminating uræmia.

The red granular kidney or raspberry kidney, no doubt, is occasionally associated with uræmia, but it is more usual for this to cause death either by the production of cerebral hæmorrhage, or else

by inducing the onset of cardiac failure. It is true that the small white kidney described above may occasionally cause death from cardiac failure, and the large white kidney from cerebral hæmorrhage; but, broadly speaking, the small white kidney is associated with acute uræmia, the so-called large white kidney with dropsy and chronic uræmia, and the red granular kidney with cardiac failure and cerebral hæmorrhage.

Inflammatory complications are often present in fatal uræmia, more especially pneumonia, pericarditis, and occasionally pleurisy and peritonitis; but pericarditis, either dry or with effusion, is unquestionably associated with uræmia.

Cedema of the lungs is an almost invariable accompaniment of fatal uræmia, and I have frequently observed it without the occurrence of any general cedema or anasarca. Cedema of the brain has also been often described, and sometimes the excess of cerebro-spinal fluid is very striking, but it is perhaps more probable that the excess of fluid is secondary to the cerebral atrophy than that it is an active cedema causing cerebral compression.

Analyses of the blood and tissues of patients dying from uræmia show that there is a great excess of nitrogenous extractive matter present. The amount of urea in the blood, for instance, may be twenty times greater than normal, and whereas in health it is doubtful whether the muscles contain more than traces of urea, in cases of uræmia the percentage amount of urea in the muscles may equal that present in the blood. Inasmuch as the muscles form, roughly speaking, half the body, the quantity of urea present in the muscles is far greater than that present in the blood, but the percentage amount in the blood gives a very fair indication of the amount present in the muscles. In the blood there is but little accumulation of any nitrogenous extractive other than urea; in the muscles and other tissues, however, there is a very great increase of the bodies of the creatin group, so that besides the great excess of urea in the muscles and other tissues, these tissues contain also very large quantities of other nitrogenous bodies not present in large amount in the blood. Although this great excess of nitrogenous extractives is found in the blood and tissues of patients dying of uræmia, it is also present to a less extent in cases of renal disease without uræmia; but under these

circumstances the quantities present in the blood, for instance, are not more than two or three times greater than those present in health. The analysis of pleuritic or ascitic fluid in cases of renal disease forms a ready means of determining roughly the amount of nitrogenous extractives present in the blood, and in this way information can be obtained clinically as to the amount of urea and other bodies present in the blood and tissues during life. In eclampsia, however, there is a comparatively small increase in these nitrogenous extractives, so that the greatest excess that I have myself found is only some three or four times the normal amount. It is important to bear in mind that the very large quantities of nitrogenous extractives present in the blood and tissues in cases of uræmia are present at a time when the excretion of urine and of urea is still fairly copious. Thus frequently a greater excess of these bodies is found in fatal cases of acute uræmia than in the fatal cases of latent uræmia associated with suppression. It is a point of some importance in the pathology of uræmia to emphasise that the blood and tissues of cases of acute uræmia are loaded with these large amounts of nitrogenous extractives.

Uræmia is a condition which can be investigated experimentally, and three methods are available for this purpose. The function of the kidneys may be interfered with by ligature of the renal artery, or by ligature of the ureters, or by complete or partial nephrectomy. Ligature of the renal arteries, ligature of both ureters, complete double nephrectomy in animals, all produce the same clinical picture, and the condition that is produced by these experimental procedures is never that of acute uræmia, nor even of chronic uræmia, but rather that of latent uræmia. The animal after these operations, provided they have been carried out with complete antiseptic precautions, dies usually on the fourth or fifth day, and during the first two or three days after the operation no very serious disturbance of health is obvious, the muscular strength is diminished, but there is no marked vomiting or diarrhoea, convulsions and coma are conspicuous by their absence, and the only obvious effects are a progressive fall in the body temperature, beginning immediately after operation, and reaching its minimum as death approaches. The fall amounts in some cases to as much as 7° F., the temperature of the dog

falling from a normal of 102° to 95° . On the third day muscular weakness begins to be shown, and there may be slight vomiting, but I have never in a large number of experiments seen coma or convulsions, and, as mentioned before, the whole condition is closely similar, if not identical, with that seen in the human subject in calculous obstruction, and in the other cases described above where the functions of the kidney were arrested as a result of interference with the circulation.

Former experimenters have described vomiting as a marked symptom of double nephrectomy; it is at any rate possible, if not probable, that this was observed owing to the lack of antiseptic precautions, and the occurrence of peritonitis.

In all my experiments I failed to obtain any difference in the symptoms whether the animal was deprived of its kidneys by double nephrectomy or by ligature of the renal vessels or by ligature of both ureters. In cases in which double nephrectomy was performed it was always done in two stages, an interval of several weeks being allowed to elapse between the two operations. Formerly it was thought that double nephrectomy produced rather different results from those following double ligature of the ureters, but in my opinion this is simply due to the fact that double nephrectomy, if both kidneys are removed at the same time, is a severe and formidable operation compared to the ligature of both ureters; if it be done in two stages there is no difference to be detected in the results produced. I have laid some stress on these facts because it seems a matter of great theoretical interest if it can be shown that complete abrogation of the functions of the kidney produced experimentally does not lead to the production of acute uræmia, but only to the condition described above as latent uræmia. Further, it would afford another illustration of the similarity of the phenomena produced experimentally in animals and those seen in the human subject as a result of disease. *I would therefore emphasise the conclusion that complete removal of the kidneys in animals and the complete cessation of the functions of the healthy kidneys in man, either as a result of obstruction of the ureters or as a result of circulatory disturbance, produces not acute uræmia, but latent uræmia.*

Experimentally, after the operations just described there is an accumulation of nitrogenous

extractives in the blood and tissues very similar, if not identical, with that seen in the human subject in uræmia. The blood is loaded with urea, the tissues and more especially the muscles also contain not only very large quantities of urea, but in addition, other nitrogenous bodies of the creatin group, so that the parallel between experimental and clinical results holds good also as regards the presence of these nitrogenous extractives in increased amounts in the tissues.

I have made many attempts to produce chronic uræmia experimentally, more especially by two methods; one by performing partial nephrectomy, that is to say the removal of a portion of one kidney and the whole of the other, leaving the animal with the fragment of one. Under these conditions nothing analogous to chronic uræmia is seen, but a condition is produced which I have described fully elsewhere, in which the animal passes a greatly increased quantity of urine; and provided the fragment of kidney remaining approximates to one third of the original kidney weight, the animal remains in good health for periods as long as two years, and a condition is produced that closely resembles that seen in the human subject in diabetes insipidus. There is no other departure from health except, perhaps, a slight loss of weight and the excretion of double or treble the normal amount of urinary water. If the remaining fragment amounts to but one fourth or less of the original kidney weight, in addition to the increased excretion of urinary water there is an increased excretion of urea and great emaciation is produced, and the animal dies more or less rapidly with extreme marasmus. It is remarkable that even small fragments amounting to about one sixth or even less of the original kidney are able to excrete quantities of urea far greater than those normally excreted, and under no circumstances, even when the quantity of kidney left was but one tenth of the original total kidney weight, was acute or chronic uræmia ever produced, therefore drowsiness, coma, convulsions, &c., were never seen.

Another method by means of which I tried to produce chronic uræmia was by experimentally producing atrophy of the kidney. This atrophy was brought about by first ligaturing the ureter and causing a hydronephrosis, and subsequently opening the ureter and so draining the hydronephrosis, the end of the ureter being brought

to the surface of the abdomen and the kidney permanently drained. In this way very perfect atrophy of the kidney is produced. On subsequent removal of the opposite healthy kidney I hoped to produce chronic uræmia, but I found that the atrophied kidney, although secreting an acid fluid, was quite insufficient to maintain even temporarily the renal functions, and the phenomena seen were similar to those produced by double nephrectomy, namely, latent uræmia.

Theories of Uræmia.

Many views have been held as regards the nature of uræmia, but broadly speaking they are divisible into two groups, mechanical theories and the chemical theories.

It has long been known that interference with the cerebral circulation will produce drowsiness, convulsions, and even coma, and on this basis Traube elaborated his celebrated theory that uræmic convulsions were dependent on a disordered cerebral circulation, and that the phenomena of uræmia were dependent either on cerebral anæmia produced by constriction, partial or general, of the cerebral arterioles, or else by cerebral œdema. This theory had the advantage of explaining both generalised and localised uræmic manifestations, as it was supposed that local vascular constriction or local cerebral œdema might cause convulsions beginning locally, or a palsy limited to some part. Further, uræmia is frequently associated with those renal diseases in which high tension and arterial degeneration are marked features; and further, uræmia or uræmic phenomena often occur synchronously in such cases with an increase in the general blood pressure. Lastly, venesection or purging, conditions which lower the general blood pressure, have often been found useful in the treatment of uræmia. The principal objection to this view has always been the difficulty of demonstrating cerebral œdema in the post-mortem room, and that the excess of cerebro-spinal fluid often found in cases of uræmia or renal disease is to be looked upon rather as secondary to the atrophy of the cerebral structures than as an active and compressing agent.

Further recent observations by Leonard Hill have shown that the higher the general blood pressure the greater the dilatation of the cerebral

vessels, so that high tension would tend to be accompanied by dilatation of the cerebral vessels, and not by constriction. Formerly a difficulty was felt in believing that such varied symptoms as those seen in uræmia could be produced by the action of a poison or poisons; and further, it was not recognised that a poison or poisons had such selective actions as they are known to possess, so that for many years it was not thought possible that a poison circulating generally in the blood-stream could act on one portion of the cerebral cortex, and cause a fit beginning locally. We know that a substance like arsenic may, for instance, produce herpes, and that when it produces herpes it produces unilateral herpes,—that is to say, that the metallic poison arsenic circulating in the general blood-stream picks out one or more ganglia of the posterior roots on one side of the body, and not the corresponding ganglia on the opposite side. The same is true of other poisons, as, for instance, lead, &c. It is notorious that poisons may produce diametrically opposite effects in small and in large doses. Since such a poison as arsenic has such a selective action as that mentioned above, the difficulty in accounting for the varied or even unilateral manifestations, or uræmia, to a great extent disappears.

The further difficulty in accepting the toxic nature of uræmia lies in the fact that the poison or poisons has not hitherto been isolated. A great number of observations have been made with regard to the toxic action of urine in health and in disease, and it has even been asserted that the urine secreted in the day-time has a different toxic action to the nocturnal urine; and further, that in uræmia the toxicity of the urine diminishes or even disappears, in other words, that the toxic substances normally excreted are for some reason or other retained in uræmia. I have made a few observations on the toxicity of the urine secreted in uræmia, and I have failed to find that its toxicity is seriously diminished.

For my own part I think we must admit that uræmia is probably of toxic origin, and the next question that presents itself is whether the poison is simply a substance that is normally excreted, and that uræmia results from its retention; or secondly, whether uræmia is not dependent on the formation in the body of toxic substances not usually present, and perhaps their retention. If the views advanced

above rest on a sound basis, it is clear that uræmia cannot be due to mere retention owing to the deficient excretion of some substance normally produced in the economy, for if such were the case acute uræmia ought certainly to supervene both in the human subject and experimentally as a result of the removal of the kidneys or as a result of the suppression of their function. Whereas we have seen that under those circumstances latent uræmia and not acute uræmia is produced. It is possible that latent uræmia is dependent simply on the retention of the substances normally excreted by the kidneys, but in acute and chronic uræmia the problem is not so simple.

I have advanced elsewhere the view that the kidney, in addition to excreting the urine, has an influence on the nutrition of the body, and more especially on that of the muscular system. This view is mainly based on the fact that if large quantities—namely, three quarters—of the total kidney substance are removed, the animal undergoes great wasting, and the urine contains an excessive amount of urea, and that death results from marasmus. If this view be correct, it is clear that when the kidneys are diseased, and their functions more or less cease, it is possible that the tissues of the body, more especially the muscles, may undergo disintegration changes similar to those seen experimentally as a result of these extensive partial nephrectomies. The loading of the muscles with these nitrogenous extractives, both experimentally and clinically, in uræmia is another argument in favour of this view, and I would suggest that it is possible and probable that in uræmia the toxic bodies are derived not from simple retention of normal urinary constituents, but owing to the formation of abnormal substances in the tissues owing to the cessation of the metabolic activity of the kidney. It seems to me that only in this way can we reconcile the facts that uræmia is of frequent occurrence in acute or chronic renal disease, with or without complete suppression of urine; and on the other hand, that latent uræmia is invariably the accompaniment of complete suppression of urine when this occurs apart from long-continued disease of the kidneys.

Latent uræmia would then be dependent simply on the retention of the normal constituents of the urine, acute and chronic uræmia would be dependent on the presence in the blood and tissues

of the products of abnormal tissue disintegration, and this tissue disintegration would be a direct consequence of the diminution in the amount of healthy kidney substance dependent on the acute or chronic disease of the kidney.

The *prognosis* of uræmia depends essentially on its cause. The acute uræmia complicating acute diseases of the kidney is by no means necessarily so serious as acute uræmia complicating chronic disease of the kidney, and the latter I would look upon as almost on the same footing as diabetic coma in its severity. Acute uræmia of acute renal disease is not uncommonly recovered from, even when the symptoms such as coma and convulsions are most alarming in their severity; and the same remarks apply to eclampsia, recovery here being by no means uncommon even in its most severe forms. I have never myself seen recovery occur in acute uræmia occurring in chronic renal disease, and more especially in the form described above associated with the small white kidney seen in young adults.

The *prognosis* in chronic uræmia is most difficult, as it is not uncommon to see patients drowsy, if not comatose, with Cheyne-Stokes breathing, who seem to be at the verge of death, and who live for weeks or months.

Even the presence of pericarditis in cases of chronic uræmia is not an absolute bar to at any rate temporary recovery.

The *prognosis* in latent uræmia necessarily depends on the cause. In calculous obstruction, if the condition is recognised and operative interference is carried out before the fourth or fifth day, complete recovery is frequent.

The latent uræmia due to renal thrombosis and to reflex suppression is necessarily much more serious, if not invariably fatal, as the conditions are beyond the reach of surgery.

The *treatment* of uræmia, like its prognosis, depends very largely on its cause. In acute uræmia of acute renal disease venesection and free purging are undoubtedly of great value. It is doubtful whether venesection, even copious, is of much value in the treatment of acute uræmia complicating chronic renal disease, and it is obvious that the two conditions are on a totally different footing. In the one case the patient has merely to be tided over for a few days; in the others there is this long-standing and necessarily progressive renal

mischievousness, and it is clear that venesection can only relieve temporarily if at all.

Venesection is also of use as a preventive of uræmia, as it is not uncommon, if the high tension of renal disease be relieved by copious bleeding, for imminent uræmic symptoms to disappear. Inasmuch as general high tension leads to dilatation and not constriction of the cerebral vessels, it is clear that high tension would tend to flood the brain with any toxic matter present in the blood.

In all cases of uræmia it is obvious that the diet should be of the simplest character, as the condition has been known to be produced by excess in diet. Generally the patient's condition is one of starvation, owing to the nausea and vomiting.

Some relief may be obtained by causing the skin to act by pilocarpine and hot air baths, but these modes of treatment are perhaps of more use as preventives to the onset of uræmia than as affording any great relief when the condition is fully established, and one has known very serious uræmic fits to be apparently produced by the hot air bath.

Finally, morphia is undoubtedly a remedy of great value in certain cases, and more especially for the relief of uræmic convulsions and uræmic dyspnoea, but, as is well known, its administration is not free from risk.

The Diagnosis of Streptococcus Pneumonia.—Lewin ('Bolnitschn. Gas. Bothnia,' No. 16, 1898; 'Clinica moderna,' November 23rd) arrives at the conclusion, from a study of three cases, that in the streptococcus form of pneumonia the temperature maintains a more or less intermittent character, with rigors and sweating at the beginning of each successive access; the duration of the disease is from one to two months, and the bacteriological examination of the sputum, as of the splenic juice, obtained by aspiration of the spleen, shows streptococci only. In all other respects the clinical picture of this form of pneumonia is exactly like that of the broncho-pneumonia of diplococcus.—*N. Y. Med. Journ.*, Jan. 7th, 1899.

CHAPTERS FROM THE TEACHING OF DR. G. V. POORE.

No. XIV.

(Concluded from p. 224.)

The next point is how to treat phosphorus poisoning. If you came quickly to the patient you would of course wash out the stomach, so as to try to get the phosphorus out of it, and I think perhaps you would be right in giving a big dose of castor oil afterwards. Phosphorus is soluble in oil, and if the oil is going to course through the alimentary tract and be expelled, very likely that would do good. There is one thing which serves as an antidote for phosphorus, and that is crude turpentine. In the vapour of crude turpentine phosphorus ceases to be luminous, and the only treatment which seems to be of any use is the administration of repeated doses, perhaps as much as thirty drops every half-hour, to the patient who is the subject of phosphorus poisoning. There was a case in the hospital a few years ago under Dr. Ringer, and the patient was treated in that way, and went out well. Another interesting fact about that patient who went out well was that she came back into the hospital a few weeks later with Bright's disease; and it is very likely indeed, or at least possible, that that which cured the phosphorus poisoning had irritated the kidneys, and set up a form of Bright's disease. After being in the hospital for some time she recovered.

There is one other point with regard to phosphorus which is of very great interest, and I find that Dr. Luff in his book on 'Medical Jurisprudence' quotes a case which was described in the 'Lancet' of 1893, in which the liver symptoms were delayed, he says, for nine weeks after swallowing phosphorus. That, of course, is very exceptional, but exceptional cases do arise, and your investigations as to whether the patient has had phosphorus or not must have reference not only to the few days before the accident, but to some time previously.

There is a form of chronic phosphorus poisoning which consists of necrosis of the jaw; it occurs in those match factories where yellow phosphorus is used. You cannot read the papers, especially some of them, without finding allusions to "phossy

jaw," as they call it. The papers make the most of a few cases, for it is a very rare trouble; but it seems to be avoidable in several ways: first of all by giving good ventilation to the room; secondly, by great cleanliness in the matter of washing the hands before meals, the methodical use of the tooth-brush, and so forth; and thirdly, by excluding from the factory anybody with carious teeth. Those with carious teeth are more liable to get phosphorus necrosis than are others. In fifty-four cases of phosphorus necrosis which have been collected, twenty-seven were in the lower jaw, twenty-two affected the upper jaw, and five affected both jaws.

With regard to the detection of phosphorus, this may be easy or difficult as the case may be. I would say that in phosphorus poisoning of few days' duration the whole of the phosphorus is oxidised, and you fail to find crude phosphorus in the body. We have any amount of combined phosphorus in the body normally, and the discovery of that would be of no import whatever. Phosphorus is a luminous body, and in a case of phosphorus poisoning you should take some of the contents of the stomach and put them on a dish in a darkened room, and gently heat it over a flame. In vomit containing phosphorus you will see when it is warmed little points of flame, and you will perceive a characteristic smell of garlic. One of the best ways of detecting phosphorus was the method used by Mitscherlich of distilling it in the dark. Put the contents of the stomach into a retort with a very long tube and heat gently in the dark, and you will find the delivery tube into your condenser is luminous. There is perhaps another way of detecting phosphorus poisoning, and that is the fact that phosphorus reduces nitrate of silver. If I take a piece of paper and wrap some phosphorus paste in it so as to get the paste safely to the bottom of a test-tube, then make some filter-paper into a cone, and after moistening it with a solution of nitrate of silver put it on the top of the tube and apply heat, the tip of the cone of paper will become darkened, and have a metallic lustre. It should be left for several minutes. In making that experiment you must be careful that you have not got sulphuretted hydrogen present, for if you darkened the nitrate of silver paper in the presence of sulphuretted hydrogen you would get a false result, as I will show you. But that may be

rectified by trying another paper moistened with lead acetate; if the silver paper is blackened and the lead acetate paper is not, you may come to the conclusion that phosphorus is present. I have dwelt fully upon the pathology of phosphorus poisoning, and it is a matter of great pathological interest that though we have so much combined phosphorus in our bodies, yet a grain or two of it uncombined will cause almost certain death.

I now come to that very important poison *arsenic*. Arsenic is the most important poison with which we have to deal, and it has been used again and again. When the early chemists discovered white arsenic it was soon used by poisoners, and was said to be contained in the aqua tophana which was so largely used in Italy. White arsenic is largely employed in the arts, and it is largely employed in medicine. It has been used also for dipping sheep, and for steeping wheat in before it is sown. Therefore, arsenic is one of those things which is found largely in agricultural districts, and may be got nearly everywhere. But when sold for economic purposes of that kind it is obliged by law to be mixed with colouring matter, such as soot or indigo. White arsenic has no colour and very little taste, and it can be given without the victim being immediately aware of it either by the tongue, or the nose, or the eye. In consequence of its lack of colour it has been accidentally mixed with flour, and with violet powder, and dusted over babies, to their great detriment. In one notable case at Bradford, the so-called Bradford lozenge case, a confectioner had the intention of only mixing plaster of Paris with the lozenges, but he put into them white arsenic, with the result that over two hundred people who partook of those lozenges were made seriously ill, and seventeen died. That occurred many years ago. The solubility of arsenic is not great, and in pure water and in boiling water not much of it is dissolved. But if to the boiling water you add a little alkali, then the solution of the arsenic takes place quickly. Owing to the difficult solubility of arsenic it has often been given suspended in thick liquids. We shall have to deal with a case presently in which the arsenic was given in cocoa, and in another case arsenic was given in a patent food—Revelenta Arabica. Arsenic has usually been given suspended in that way.

Now when arsenic is taken the symptoms do not

begin at once; there is generally an interval. Of course one need hardly say that the length of that interval depends upon the state of the stomach and the way in which arsenic is given. If it is given on a full stomach, and with a thick stodgy meal, the interval may be considerable; but if it is given on an empty stomach in a state of fine comminution, the interval may be slight. In recorded cases the interval before the commencement of the bad symptoms seems to have varied from as little as eight minutes to as long as nine hours.

As you are aware, arsenic has been used as a caustic; arsenical paste has been largely used by cancer quacks for destroying growths, and a speck of arsenic is often employed by dentists for destroying the nerve-ending in the tooth before stopping. Arsenic, being a caustic, causes symptoms of irritation in the stomach. There is burning pain and intense thirst. There is vomiting; and the vomit, as with other corrosive irritants, may consist of bloody material, mucus, and coloured material if indigo or soot has been mixed with the arsenic. With the vomiting there is usually purging and collapse, the patient being so feeble that he is almost unable to do anything—that is very characteristic,—he can hardly speak or move. In addition there are very often cramps in the legs. I would call your attention to the not infrequent association of gastro-intestinal disturbance with cramps in the legs; it is a reflex phenomenon, and is frequent in poisons which cause gastro-enteritis, and it is frequent also when gastro-enteritis arises from any other cause. It is very common in cholera, and not uncommon in acute dysentery. Towards the end of a case of arsenical poisoning there may be delirium, coma, and convulsions. The patient looks exceedingly ill, and not infrequently, if life is prolonged at all, there is jaundice, though slight in degree. The next point—and it is a very important one—is that in arsenical poisoning as a rule there is no rise of temperature; that has a bearing on the diagnosis. Not only may arsenical poisoning be acute, but it may also be chronic. The latter occurs where arsenic is extensively used, as in the arts. There are many arsenical pigments. Arsenic is also used as an antiseptic by furriers and by so-called taxidermists. Many wall-papers are made with arsenical pigments. Now-a-days arsenical pigments are very largely replaced by aniline pigments. There,

again, in order to produce these colours, arsenic acid—the highest oxide—has been used as the oxidising agent, and many years ago there was an outcry that people who wore magenta-coloured socks got an eczematous eruption up the legs, and this was put down to the arsenic which had been used for the colours. In the same way some of the other pigments are injurious; although they have not got an arsenical base, arsenic has been used for their development.

The Treatment of Sore Mouth in Pregnant Women.

—For the prevention of this complication of pregnancy 'La Presse Médicale' of September 21st, 1898, recommends the habitual use for four months prior to parturition of a mouth-wash designed to prevent acid formations in the mouth. The patient should visit a dentist in order to have the teeth thoroughly cleansed, and carious places attended to. In many instances it is necessary to restore or maintain the normal alkalinity of the buccal secretion by alkalies such as the bicarbonate of sodium. The following may be used as a mouth-wash or dentifrice:

℞ Bicarbonate of sodium,
Carbonate of lime ... each 10 drs.
Refined camphor 2 "

Or—

℞ Carbonate of lime 10 "
Salol 2 "

In rebellious cases where there is much swelling of the gums and mouth a wash of chlorate of potassium may be necessary, or it may even be necessary to touch the ulcerated spots with permanganate of potassium. The following formula may prove useful:

℞ Chlorate of potassium ... 1 dr.
Rose honey 2 drs.
Glycerine 6 "

In very obstinate forms the spots may be touched with the following mixture:

℞ Tincture of iodine ... 2 drs.
Glycerine 6 "

When permanganate of potassium is used, we employ permanganate of potassium 3 grains, distilled water 1 ounce.

Therapeutic Gazette, December, 1898.

THE TREATMENT OF YELLOW FEVER BY CHLOROFORM INHALATIONS.

An Abstract of a Lecture delivered before the Academy of Brazil

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THE lecturer commenced by saying that his attention had some years before been attracted to the fact that in certain women of the white race who had been under his care, and in whom yellow fever had occurred during pregnancy, its course seemed to be different from ordinary cases. As was to be expected, it precipitated the labour, but after that had taken place it assumed a benign character, and the patients recovered.

Contrasted with these, it was in the experience of every one that as a rule parturient women had no more immunity or resisting power than others, generally rather less.

After long consideration of these facts, he had come to the conclusion that the benignity in the cases which came under his own observation was to be traced to the use of chloroform inhalations during the labour. He was in the habit of using chloroform in this way up to a point of partial narcotism, which was marked by the power of the patient to answer when addressed. This he considered the primary agent in the beneficial result, but he could not shut his eyes to the fact that there might also in the puerperal state be other agencies tending to the same good result; among these a reduction of temperature caused by the loss of blood during the labour, the increased metabolism to which the puerperal state gave rise, aiding in increasing excretion, and to elimination of the poisonous material. These, however, by themselves, and apart from the action of chloroform, were not sufficient to combat the evil.

He observed that the action of chloroform thus administered was to produce a normal diuresis, a diminution in the amount of albumin, if not a disappearance thereof, a cessation or decrease of the vomiting, a general diaphoresis, and above

all, a beneficial interference with the remittency of the fever. He therefore claims that chloroform inhalations are of beneficial use in the treatment of yellow fever. The author further explains this beneficial use of chloroform by the following specific actions of that drug:

Chloroform, besides its analgesic or anæsthetic, also possesses antithermic and antiseptic properties. The antithermic action, however, is not attained until the chloroform has been pushed to a surgical degree; and as it is not the author's habit to do this, he attaches no value to this action of the drug.

Passing, then, to the microbicide or antizymotic property of chloroform, the author contends that the result is produced first by the decreased exhalation of carbonic acid, and therefore reduced cellular combustion. This also implies a diminution of the supply of oxygen to the tissues. This involves a reduction in the rate of tissue metabolism. The diminution of the supply of oxygen to the tissues does not necessarily, however, mean an actual diminution of the amount of that element in the economy as a whole, but that the blood-corpuscles economise their expenditure thereof.

Applying these views to the action of chloroform in yellow fever, the author postulates with regard to that disease that it results from the action of specific microbes which infected the blood, the result of such action being the production of a toxin which acts on the thermogenic nerve centres or vaso-motor system, the condition of fever being due to the excessive cellular metabolism thus excited. These were the admitted phenomena of yellow fever.

Chloroform, then, being absorbed into the blood, caused a modification in the blood corpuscles, by which they not only resisted the inroads of the microbes circulating with them, but were enabled to economise the expenditure of oxygen even to the point of refusing to yield it up to the tissues.

Again, it is postulated by the science of pharmacology that chloroform circulating in the blood is decomposed into chlorine and formic acid; for this reason the author claims that in addition to the above formulated action of the chloroform on the blood-corpuscles, whereby they defend themselves and the oxygen they convey from the attacks of the microbes, there is also an offensive action, antagonistic and destructive to the microbes

themselves, such destruction being brought about by the action of the chlorides and formates thus set free in the blood.

If, on the other hand, this decomposition postulated by pharmacologists did not take place, was not the action of the chloroform as chloroform sufficient to produce the effect on these low forms of plant life as well as it could produce lethargy in plants more highly organised?

Or again, the author suggests that by the action of the chloroform the entire economy of the patient gains time, and while protected from the morbid action of the toxin and the microbes, has his potential immunity from their action increased by their very presence in his system.

He also considers that in virtue of the increased resisting power of the bioplasm thus caused, an increased number of the white corpuscles would be at liberty to exert their phagocytic powers over the microbes. For these reasons, and in view of the fact that to this present day there is no known efficacious treatment of yellow fever, the author claims that a fair trial should be given to the method which he brings forward, and which he formulates thus:—The patient should be kept for the greater part of each twenty-four hours under the influence of chloroform inhalations to the degree that he can always answer when addressed.

The translator of the above has tried this treatment in one case, that of a young sailor under his care. This lad was ill five days. The disease was clearly diagnosed on the second day, in the evening of which the chloroform inhalations were commenced, and maintained for three hours in every twenty-four. The patient thoroughly enjoyed the chloroform, and called for it. The only apparent difference noted was an increase in the amount of albumen passed, and the occurrence of nocturnal semi-delirium. On the fourth day he passed a little blood (a streak) with his fæces. Fifth day apparently better in the morning, but at 2.30 in afternoon had a regular tetanic seizure, with opisthotonos and difficulty of breathing, shortly followed by a second fit, and at 3.30 by a third. This was succeeded with hæmatemesis (black vomit), which persisted till 6.45, when he died.

Fascinating as is the hypothesis put forward as to the action of chloroform in this disease, the above case cannot fail to give rise to the question

as to whether the chloroform could by its prolonged action on the nervous system have in any way induced a vascular condition provocative of the tetanic convulsions. Tetanus is not an ordinary conclusion to yellow fever.

BRONCHOTOMY.

THE accidental entrance of a foreign body into one of the bronchi by inhalation is an occurrence common enough to make any advance in the treatment of the condition worth noting. Methods until recently employed were such as inversion of the patient, and the administration of drugs to cause coughing or vomiting, but it is only within the last few years that attempts have been made to remove the offending body through an external incision. When a body cannot be reached with some kind of a forceps through a deep tracheotomy wound, or is too firmly fixed to be removed with safety by such means, we are compelled to seek other avenues of attack. If the foreign body is left in the bronchus, it is practically certain to cause suppuration, and then a suppurative pneumonia and death, so we are justified in taking very considerable risks in order to remove it. The route most often selected is through the back, and it is usual to resect one or more ribs. The shape of the incision has been variable, and is not of much consequence; and most operators have not encountered any great difficulty in reaching the posterior mediastinum, after which point in the operation cases apparently vary very much. Very recently Curtis (*'Annals of Surgery,'* November, 1898) has reported a very interesting case. The object inhaled was the seed-pod of some plant which had been transfixed with a pin, and the patient was a boy of twelve.

Efforts were made to reach the body through a deep tracheotomy wound, and it was grasped in the forceps repeatedly, but could not be removed. On the following day the operation through the back was attempted, but had to be discontinued on account of impending collapse, and the wound was temporarily packed. Next day a second attempt was made, and the body could be felt through lung tissue after the pleura had been

opened between two sutures. It could not be reached with forceps through an opening in the bronchus, and the lung was therefore incised with the thermo-cautery at the point where the body was felt; but the latter could not be found, possibly on account of its partially macerated condition.

The patient unfortunately succumbed to the septic pneumonia which soon developed, and we therefore cannot call the treatment successful. However, the case has encouraging features. We know that we can reach the bronchi from behind with safety, as it has been done a number of times, and that therefore the accessible area in the respiratory apparatus is enormously enlarged. That the element of time is of very great importance is another lesson of such an experience. After a suppurative inflammation is once started around a foreign body in a bronchus, there is great danger that the process will cause a fatal pneumonia, even if the foreign body is removed and the inflammation stopped at the locality where it began.

Twenty-four hours ought to be the limit of time allowed for all ordinary methods of getting rid of the intruding object. These methods include low tracheotomy. At the end of this period we ought to be prepared to undertake the more extensive operation. It should be possible in almost all cases to come to a pretty definite conclusion as to the location of the body before we operate, and during operation it will probably often be possible to feel it in the bronchus. The use of the thermo-cautery is the safest procedure in case it is necessary to invade lung tissue, but if a large vessel should be opened, the bleeding might be troublesome in such a deep cavity. This operation of bronchotomy undoubtedly has a useful future, but it is a good illustration of what very highly specialised knowledge modern surgery demands, and of how much more exact and specialised this knowledge must be as the most inaccessible regions in the body become accessible to the surgeon's knife and finger. Almost anyone might find himself the victim of an accident such as we have been considering, and there would be only one way of sure relief, and the danger of that would be inversely proportional to the skill and experience of the operator.

Medical Record, January 7th, 1899.

Statistics of Serotherapy in Tuberculosis.—

J. E. Stubbett ('Boston Medical and Surgical Journal,' November 24th, 1898) read a paper before the American Climatological Association based on a study of eighty-two cases. He stated that the experimental results lead undoubtedly to the conclusion that the treatment with antitoxic serum is still in the experimental stage, and should as yet be used only in sanatoriums and under the best conditions. "We are on the road to success in this disease, and nearer the goal than ever before." The serum used by the author was prepared by Dr. de Schweinitz, director of the Biochemic Laboratory of the U.S. Bureau of Animal Industry at Washington, D.C. It was manufactured by the Government, and was to be used for scientific purposes only. Other serums were used in a few cases. Dr. Stubbett believes that the antitubercle serum is contraindicated in the third stage of pulmonary tuberculosis, and also in cases of mixed infection; contrary to Maragliano, who claims that antitubercle serum is indicated in all cases and in all stages. The author referred to some unpleasant symptoms following the injections, but did not find them dangerous. In one case only was there syncope. The advantages are—(1) This method does not tax the functions of digestion or produce gastritis, diarrhoea, or loss of appetite; (2) the bacilli have disappeared in cases where the sputum was still abundant; (3) up to the present time in his experience no relapses have occurred. The serum can be given advantageously about six or seven months. In cases complicated with the streptococcic infection the author used Pasteur's antistreptococcic serum, believing that if the secondary infection could be neutralised, a clear field would be left for the action of the antitubercle serum. Six cases of this class are described. The author found that the streptococci decreased very greatly or disappeared entirely after the second injection. The largest number of injections given in this class was four. In the total of eighty-three cases the expectoration decreased in 82 per cent.; the appetite improved in 81 per cent.; weight was gained in 78 per cent.; physical signs improved in 78 per cent.; temperature decreased in 49 per cent.; bacilli disappeared in 13 per cent. and decreased in 35 per cent.; cough decreased in 79 per cent.; apparent immunity established in 21 per cent.; generally improved, 78 per cent. The

author is not yet a thorough convert to serotherapy in the treatment of tuberculosis, but believes that these investigations are along the right line, and that this line of treatment on the whole is more satisfactory than treatment by drugs. He does not regard it as a specific.—*Medicine*, January, 1899.

Sporozoa of Carcinoma.—Jurgens has succeeded in isolating from the mucus surrounding the neoplasm a micro-organism evidently belonging to the Gregarinidæ in a number of cases of cancer of the stomach and one cancer of the bronchi, in which the air-passages were occluded, resulting in a collection of several generations of the sporozoa. It is much larger than the usual Gregarina, which he explains by the discovery of the fact that the micro-organism develops by the conjugation peculiar to this species, the two bodies blending to form a single large one, which invades the cell until the shell of the cell enclosing the micro-organism is all that is left of it. In this condition it resembles Miescher's tubes and Rainey's bodies, and he queries whether they may not be developed Gregarinidæ.

Journ. Amer. Med. Assoc., Dec. 31st, 1898.

Picrotoxin.—The conclusions of a recent study of the cocculus indicus and picrotoxin sent in to the Paris Académie de Médecin are that it is a dangerous poison, with an almost exclusively bulbar action, extremely slow of absorption and impregnation. It is the type of a bulbar convulsive, and has no direct effect upon the muscular fibre. The cardiac and respiratory manifestations depend upon the effect produced on the medulla, although to this must be added a certain influence of the toxic substance on the nerves of the vegetative life. The central influence is also responsible for the salivary and intestinal hypersecretion observed in the dog, and the sudoral, salivary, and lachrymal hypersecretion in the horse. The writers are extremely pessimistic in regard to its application in therapeutics.—*Bulletin*, Nov. 15th.

A MEDICAL GOLF TOURNAMENT has been arranged for 1899 on similar lines to that held last year. Entries, with lowest handicap and subscription (5s.), to be sent not later than February 6th to the Honorary Secretary, Medical Golf Tournament, Windlesham, Surrey.

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by the Author.

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THE

PULSE AND BLOOD PRESSURE.

A Clinical Lecture delivered at the Central London Sick Asylum, November 17th, 1898,

By GEORGE OLIVER, M.D.Lond.,
F.R.C.P.Lond., &c.

GENTLEMEN,—The subject of my lecture, the pulse and blood pressure, is too large for adequate treatment within an hour. I will therefore confine myself to some introductory remarks on digital observation of the pulse, and then I will adduce some of the main results of a research on blood pressure in man, in which I have for some time been engaged.

Now first of all I will remind you of a few points bearing on the observation of the pulse through the finger. In estimating the pulse the practical physician does his best to set his patient at ease, and uses all the tact at his disposal to subdue all nervous agitation. This is a very important practical matter, because emotional perturbation not only affects the rate of the pulse, but has a marked influence, as I shall show you presently, on the blood pressure. In examining the pulse it is always desirable to use not one or two fingers, but three. There are certain reasons for this practical point. By using three fingers you amplify the tactile sense very considerably, and consequently you get a very much keener, more delicate appreciation of the blood pressure than by the use of one or two fingers. Besides, the use of three fingers is convenient in testing the pulse in certain ways, which I will refer to subsequently. In applying the finger it is exceedingly important not to put on your pressure all at once; first let the weight of the fingers, as it were, rest on the artery, using scarcely any pressure at all; then graduate your pressure very gently and slowly. The objects of pulse feeling are mainly two; in the first place you study thereby the state of the artery

and in the second place you test the character of each pulsation.

Let us first consider the artery. The artery may be changed in certain respects; its wall may be thickened or it may be contracted, or it may be stretched or distended. The bore of the vessel is also an important point, as will appear later on; this may be enlarged or reduced. The size of the artery is not the size of the bore. The first point I wish specially to insist upon is that a normal artery cannot be felt by the finger. When an artery can be felt (and it may even be cord-like) it is either thickened or distended. If it be thickened, and you empty the vessel by placing the finger on the central end of a portion of it, and another finger on the distal end so as to prevent what we call the reflux beat, and so that you get that portion of the artery quite free from blood, you will most likely be able to define with less or more distinctness the walls of the artery. But if, having done this, you cannot feel the wall of the vessel, you may infer that the artery is distended, and not appreciably thickened. These two things in clinical observation are apt to be confounded, though they not infrequently co-exist. Again, the wall of the artery may be contracted. How is this to be ascertained? By a simple mode of observation it is easy to determine this point. When in a normal state of the vessels a finger suddenly arrests the circulation through the artery, the fingers placed immediately on the central side of the block feel a swelling of the beats, which subsides at once on removal of the obstruction. This is what I have elsewhere called the "stopcock expansion" of an artery, because it resembles what happens when a stopcock is suddenly turned; the portion of the artery immediately above the block receiving the full force of the onward flow expands, and the beats enlarge. If the arterial wall be contracted and you suddenly stop the circulation, this expansion is limited or is prevented; then the pulsations do not enlarge so much as in the normal state of the artery, or they do not enlarge at all. The bore of a vessel cannot be discriminated by the finger, and you must also remember that the size of the pulse is not always in proportion to that of the bore of the vessel. These facts I have ascertained by the use of a little instrument called the arteriometer, by which the bore of an artery can be measured. It is, as you see, like a

watch, with a dial on which millimetres and tenths of millimetres are marked off.* When placed immediately over the radial artery, the wrist being supported on a rest at an angle of 45° , you press it until you just close the vessel. The diameter of this gentleman's vessel is 1.9 mm.; that is nearly the average, which is about 2 mm. The diseases in which I have found the bore of the vessel reduced are atheroma, syphilis, chronic gout, and in some nervous affections, such as hysteria and migraine. I have found the bore enlarged, as a rule, when the blood pressure is considerably above the normal. It is distended by the high arterial pressure.

We now come to the study of the pulsation. This may be viewed in relation to three aspects,—the rate, the duration, and the resistance. Firstly the rate. In this way we study the frequency, the regularity, and so forth. Next duration, that is to say, the duration or length of each beat. There are two great classes of cases, one in which the beat is of short duration, and the other in which it is prolonged. The first class you find well illustrated in young subjects, and perhaps the best illustration of all is to be found in aortic regurgitation, and in all cases of low tension. It is in detecting the long beat that the use of the three fingers is most useful. The lengthened beat is found where there is high tension, as in interstitial nephritis, and in old subjects. In such cases, under pressure, you get the prolonged swell of the pulse which is not observed in young subjects, or in cases of low tension. In olden days these two qualities of the pulse were called quick and slow, but this use of these terms is rather misleading, because quick and slow are generally associated with the frequency of the pulse, therefore these terms have been dropped in relation to the duration of the beats.

The third point with regard to pulsation is resistance, and this is one of the most important clinical aspects of pulse feeling. You may broadly divide your cases into two classes—weak and strong. In the former you have cases of low tension and of asthenia; in the latter those of high tension. It is well to remember that weak and short are not always allied. I have found in cases of aortic regurgitation not merely a short

* The construction and use of the arteriometer are elsewhere described. See 'Pulse-gauging,' 1895.

beat, but a powerful beat. Strong and long are generally allied, but again we have exceptions. For instance, you will find in the senile pulse with a somewhat failing ventricle, a long but weak pulse. In olden days these qualities of resistance went by the name of soft and hard respectively. The old writers laid great stress upon this prime clinical distinction; they regarded it as of great importance, and undoubtedly it is. These two terms soft and hard are the historical analogues of what we in these days call low and high tension. A very acute old writer, Latham, makes an exceedingly true remark in reference to the quality of hardness; he says, "Hardness is not physiological, it is always pathological." That, written over half a century ago, is as true to-day as it was then.

Now all these distinctions of the pulse are confirmed by methods of instrumental observation; for instance, by the graphic method. The sphygmograph has led to a great advance in our knowledge of causes, and has been of great educational value; but it is of small clinical value. The sphygmogram is a mere diagram of the forces in operation; it leaves out the most important clinical fact of all, namely, the blood pressure.

I now come to the estimation of blood pressure for clinical purposes. This is undoubtedly the central fact of all clinically useful observation of the pulse and the circulation. An extended experience of the instrumental methods has repeatedly demonstrated to me that digital observation affords but a vague and undefined impression of the blood pressure. Each observer sets up for himself an independent and variable standard, and many observers cannot learn, or they learn with difficulty, to appreciate the necessarily delicate shades or variations of resistance to be met with under pathological conditions. Moreover, observation by the finger has already taught us all that is to be gleaned by the unaided tactile sense; it is certainly not likely to reveal any further knowledge. Therefore it seems highly desirable that we should go a step further; that we should try to supplement digital observation, which is rough and vague and indefinite, by some methods of greater exactitude. I should also mention that I have repeatedly proved by these methods that the sense of touch, like all the other senses, has its pitfalls or illusions, which may mar even the well-trained finger-tip. I

have found that the varying calibre of the artery is the main factor in misleading the clinical observer in regard to his estimate of the arterial blood pressure. The arteriometer and the blood pressure gauge, to be presently referred to, have shown that the appreciation of the blood pressure diminishes when the calibre is small or is reduced, and that it becomes misleading, even to the well-trained finger, when the calibre falls below a certain point. By these two instruments I have proved that when the blood pressure is taken through the intervention of a solid pad, the record obtained is vitiated by variations in the calibre of the artery. In this fact we doubtless have a clue explaining why the finger may frequently mislead the observer in estimating the arterial blood pressure when the artery is small or is reduced in calibre. For the finger is a solid pad. The blood pressure can only be determined with accuracy through the intervention of a fluid medium.

I will now briefly refer to a little portable instrument which has afforded me much satisfaction in estimating the blood pressure in man under different physiological conditions, and also in clinical observation. For further details than can be given here I must refer you to the 'Edinb. Med. Journ.' for July, 1898,* and to the 'Journ. of Physiol.,' 1898. It is called a hæmadynamometer (literally a blood-pressure gauge).† It consists of two parts, the pad and the recorder. The blood pressure is obtained through a small cylindrical-shaped pad of thin rubber containing fluid. The recorder (resembling a watch in appearance and size) is standardised in millimetres of mercury from the mercurial manometer, and is provided with an indicator. The pad having been placed over the artery, you merely require to press forward the dial part (the recorder) of the instrument, when the indicator will begin to pulsate (generally after you have recorded a pressure of 50 mm.), and as you increase the pressure the pulsations gradually become larger until they attain to a maximum excursion; and then, as you further increase the pressure, they as gradually diminish, the progressive rise and fall of the motion being perfectly equable throughout. You read the mean arterial pressure when the motion of the indicator

* Article, "The Clinical Aspects of Arterial Pressure."

† This pocket blood-pressure gauge is made by Mr. T. Hawksley, 357 Oxford Street, W.

is greatest, and in doing so you take the point midway between the two limits of the oscillations. There is another reading of the arterial blood pressure which you may make, namely, that required to arrest the pulse wave. This point may be said to represent the maximum arterial pressure produced by the contraction of the ventricle. For this purpose you place your finger beyond the pad and press forward the recorder until you cease to feel the pulsations. Of the two readings, that of the mean arterial pressure is perhaps of greater clinical importance. The principle followed in estimating the blood pressure by a fluid medium (as in this portable little instrument) is to equilibrate the fluid pressure within the pad to that within the artery, whatever its calibre; so that when that point is reached, the two pressures being exactly balanced, and the pad then being for the moment but a diverticulum of the artery, as it were, the maximum motion of the pulse wave is developed, this being the visible indication of the perfect continuity of the two fluid pressures separated merely by membranes, namely, the thin layer of rubber, the skin, subcutaneous tissue, and the arterial wall; a slight reduction or a slight excess of the pressure within the pad impairing the conduction of all the motory waves of the pulse-beat. Fluid used in this way is therefore a device enabling you to obtain a connection with the contents of the circulatory system—akin, in fact, to that of manometric observation in animals save the breach of tissue. You may apply the instrument to any superficial artery, such as the radial, temporal, carotid, ulnar, posterior tibial, superficialis volæ, &c., and so long as these different-sized arteries are on the same level, you will obtain exactly identical readings in all; whereas a solid pad would provide you with readings which differ according to the calibre of the vessels.

Now in the course of the work in which I have been engaged, I found that the venous blood pressure can likewise be measured by the same instrument. You know, of course, that there are no pulsations in the veins; therefore some other indicator is needed for reading the venous pressure. I have adopted the following method:—You select a portion of vein free from tributaries in its course on the dorsum of the hand, or on the arm, and you apply the pad over the distal end of it; you then put on the pressure of 50 or 60 mm. (that is

a pressure in excess of any ordinary venous pressure), and you draw the finger along the vein from the pad upwards, so as to empty it. It will then remain empty if there be a valve at the proximal end of the piece of vein selected, or in its course. You now gradually reduce the pressure until you reach a point when the vein refills from the distal end. The refilling of the empty portion of the vein is an indication of how many millimetres of mercury is the venous pressure. In this way we can study the resistance in the arteriolar and capillary vessels between the arteries and the veins. As a useful and important preliminary to the clinical observation of the blood pressure, you should know something of the physiological variations of that pressure. I will, therefore, now sketch an epitome of the physiological results I have observed, and for fuller details I must refer you to a series of articles which appeared in the '*Edinburgh Medical Journal*' during the last few months.* Gravitation causes a marked variation in the blood pressure in the arteries and veins; that is, the vertical relation of the point of observation to the heart affects the record—increasing it when that point is below, and diminishing it when above the cardiac level. For every inch there is a variation of nearly 2 mm. Hg., and this I have found to hold good equally for arteries and veins throughout the body. So it follows as a very important corollary from this, that the blood pressure should be taken in such a way as to eliminate any disturbing influence from gravitation. Now, in the sitting posture the blood pressure should be taken pretty much in a line with the epigastrium, as I am doing here. In the recumbent posture the arm should be laid on the same plane as the back, so that we get very much the same vertical distance from the aortic valve in both the postures.

Now as to the effect of posture. In the sitting or standing posture the arterial pressure is 10 to 15 mm. Hg. higher than in recumbency; that is an average observation, but the difference may reach 20 or 25 mm. The venous pressure is very remarkable, being doubled from recumbency to sitting or standing; for example, 20 mm. venous pressure

* "The Clinical Aspects of Arterial Pressure: some physiological data bearing on the clinical observation of the blood pressure," by George Oliver, M.D., F.R.C.P., '*Edin. Med. Journ.*,' July, Aug., Sept., Dec., 1898.

in recumbency will become 40 in the erect posture. The variation produced by posture is therefore very much larger in a vein than it is in an artery. Muscular exercise raises the arterial and venous pressure very considerably, but this effect passes off very quickly, the repose of ten to fifteen minutes sufficing to remove it. Therefore the effects of exercise can readily be eliminated as a source of error in clinical observation of the blood pressure. One of the most powerful causes of the rise of blood pressure in the arteries and in the veins is mental exercise, and especially if accompanied by emotional excitement. This is one of the great pitfalls of clinical observation in certain cases. You require to have your patient calm and free from emotional excitement, and the tact of the physician must be exercised to secure this repose. Fatigue may be so marked in its effect as to entirely reverse the postural variation of blood pressure; so that as the subject lies down he gets a higher blood pressure both in the arteries and in the veins; and as he sits it falls. Rest and sleep settle the blood pressure to the lowest normal point. A practical illustration of this is found in bed cases, where there is a general tendency for the blood pressure to fall somewhat below what it would be in the recumbent posture when patients are following their ordinary life. Next as to digestion. The mere taking of food or liquids produces a rise of blood pressure, before the act of digestion sets in. This has a practical bearing in connection with getting a fainting person to swallow something, for by reflex action the heart is roused, whether the liquid swallowed be a stimulant or not. The digestive process itself raises the arterial pressure, but lowers the venous pressure. That accounts for the old adage, "If you eat to be cold, you will live to be old." The constriction in the periphery of the circulation produces the "cold feeling" which many of you may have experienced during the act of digestion. Breakfast generally furnishes an exceptional effect on the venous pressure. When warm fluid is taken at this meal, the venous pressure usually rises instead of falling as after the solid meals. That shows the useful purpose of the custom of taking a warm fluid the first thing in the morning in order to rouse the whole circulation, and to open up the vessels of the periphery, which shrink during sleep.

Temperature has a marked effect; warmth relaxes the arterioles, and consequently lowers the arterial and raises the venous blood pressure; and cold contracts the arterioles, and thus augments the arterial and diminishes the venous pressure. Pulse rate also has an important effect. When the pulse is considerably raised in frequency, say to 30 or 40 beats over the normal, the pressure rises. A rise of not more than 20 beats per minute has, however, no appreciable effect in normal subjects. The influence of the frequency of pulse is most marked when there exists peripheral resistance. Therefore the frequency is a matter to be considered in clinical work by the side of the blood pressure, especially in certain cases.

You will observe that all the physiological causes of variation raise the blood pressure except sleep and rest. Exercise and emotional excitement are the most important prominent causes of disturbance. I have already pointed out that the effects of exercise subside quickly, and should not vitiate clinical observation, and that in the vast majority of subjects emotional disturbance is also a transitory affair, and without significance as a source of error.

I must now say a word about the normal readings. The readings in women and men are very much alike. I regard 100 mm. Hg. as an average normal for the arterial blood pressure in recumbency, and 110 or 115 mm. in the sitting or standing posture. Then I should add or subtract 10 mm. for individual and physiological variation; therefore you may say the normal ranges from 90 to 120 mm. There is a slight increase of blood pressure in the years after forty-five or fifty, but this is not observable in some. I have been very much struck with certain cases where, even after sixty, the blood-pressure has not been higher than it might be at twenty or thirty years of age; and it is curious to observe that on inquiry I found that in several of these cases there has been a marked history of longevity in the family. I examined the pulse of a medical man in middle age the other day, and in the sitting posture it was 95 mm. I said to him, "Perhaps there is a history of longevity in your family." He said, "Yes, my grandfather lived to 104, and my father was over ninety." Such cases are probably exceptional instances of the postponement of the peripheral

changes incident to age. Observation has shown that there are far greater differences in the normal arterial pressure in persons over fifty years of age than at earlier periods of life—differences which may, in fact, extend from the lower limit of 90 or 95 mm. to 10 mm. above the upper limit of 110 mm. in the recumbent posture. The average venous pressure for recumbency (the hand level with the back) varies from 10 to 20 mm. Hg., and in the sitting posture (the hand level with the epigastrium) from 20 to 40 mm.

Now I come to the clinical observation of the blood pressure. It is the increase, not the decrease of blood pressure that is of clinical significance and importance. What are the causes of increased arterial pressure? In physiological observation there are two main causes, central or cardiac—that is, increased energy of the contractions of the heart—and a peripheral cause in the form of contractile resistance. But in the clinical field nearly all increased blood pressure is due primarily to a peripheral cause. The peripheral causes are either organic changes in the peripheral vessels or contraction of the arterioles. This contraction, which is tonic in character, may, however, extend to middle-sized arteries like the radial, and therefore a large area of the arterial system may be under the influence of constriction.

I should like now to refer to some of the effects of high arterial pressure. First of all I will refer to dilatation of the heart. I have lately been devoting a good deal of attention to this subject—dilatation of the heart as a result of high peripheral resistance. I have found dilatation of the heart to be an extremely frequent result of high arterial pressure. I have discarded, as a rule, all the uncertainties of percussion, and have relied upon the exact position of the apex beat. On finding a condition of high arterial pressure, I advise you to at once go for the apex beat.

I have repeatedly observed the interesting clinical fact, that in many cases apparently free from organic disease the maximum apex-beat was found to be considerably displaced to the left while the increased arterial pressure existed, and returned to the normal site when that pressure subsided. I think the production of this temporary dilatation under the stress of high arterial blood-pressure is mainly a question of muscular resistance or tone in the ventricular wall, because

I have observed that all ventricles do not so dilate, though it is a frequent clinical fact. It is much more common in women than in men, and we know that in women the muscular tissue has less tone and less resistance than in men. In these cases of temporary dilatation of the left ventricle I have generally found that the cause is peripheral constriction, as shown by the arteriometer—a constriction of purely neurotic origin as a rule. I will give you just one case extracted from my notes:—This patient had venous pressure 24 mm., Hg., arterial pressure 135 mm., calibre of radial artery 1.5 mm., relative position of heart impulse to the nipple line one inch outside. After the peripheral resistance had been relieved the venous pressure rose to 32 mm., the arterial pressure fell to 105 mm., the calibre of the artery went up to 2.2 mm., and the position of the apex-beat became normal, being well within the nipple line. You see that in this case while the resistance caused by a constricted condition of the periphery continued the arterial pressure was raised, the venous pressure was relatively lowered, and the ventricular beat was considerably outside the nipple line, but when the constriction was relieved the arterial pressure fell, the venous pressure rose, and the apex of the heart resumed its normal position.

Now this state of things is often accompanied by a feeling of precordial tension. A large number of women who suffer from neurotic ailments inducing tonic contraction of the arterioles and arteries have this precordial tension, though I should observe that I have not infrequently met with complaints of precordial weight and tension with high arterial pressure from constriction without a displacement of the apex. Probably in such cases the tone of the wall of the heart was sufficiently good to resist the increased stress thrown upon it, or this stress was only occasional (being relieved by relaxation of the arterioles), or was less continuous than in the cases in which the ventricle becomes dilated.

Now how is this dilatation produced? The ventricle is a distensible chamber, which in health is probably liable to a certain range of variation in its internal capacity in response to the physiological alterations that are continually taking place in the arterial blood-pressure; and when in disease this pressure exceeds the normal range the ventricular distension becomes quite apparent and

demonstrable. I may remind you that years ago Professor Roy showed that if a normal ventricle contracts a certain amount of blood remains residual to the cavity, the blood never being completely expelled from it. I have no doubt whatever that in these cases of high arterial tension this normal residuum of blood increases very considerably in the cavity and dilates it. Now, though as a rule in the cases to which I refer this effect on the ventricle is temporary, and frequently responds to treatment, should the dilatation persist it may become the foundation of chronic heart disease. These cases, to my mind, form a good illustration as to how certain forms of heart disease may be set up from an obstructed state of the periphery of the circulation; for example, many of the cases of cardiac disease met with after fifty years of age.

Now another effect of increased arterial blood-pressure is disturbance of the breathing, especially on movement, and more particularly on walking on rising ground. This probably indicates disturbance of the pulmonary circulation through the increased ventricular strain and tension. You can easily understand how dyspnoea may arise in this way. A person who presents a mean arterial pressure of say 150 or 160 mm. Hg., while quiescent, has a higher degree of arterial blood pressure than anyone in health would have while going up a hill, for in hill-climbing the mean arterial pressure is seldom more than 140 mm. Well, any additional movement in such a subject will be apt to readily disturb the pulmonary circulation and the breathing by still further raising the intra-ventricular pressure already in considerable excess.

A third effect of high tension is dilatation of the arteries. As a rule, when the arterial blood-pressure is excessive, I have found on measuring the radial artery with the arteriometer that there is a considerable increase in the calibre. This is a veritable condition of arterial tension, for there is a stretching of the wall (*tendo*, I stretch). But it is well for you to bear in mind that the term "arterial tension" is generally made to include also quite the opposite condition, namely, that of contraction, where, for instance, the radial as well as the peripheral part of the arterial circulation is constricted. There you get a rise of blood-pressure with reduction of the calibre of the radial instead of distension of the artery. But still in such

cases the larger arteries would be distended, and when you get distension in the radial, as you do in the majority of cases of very high or moderately high arterial tension, you have an overmastering of the muscular contractile power of the wall. This arterial distension must have a deleterious effect on the nutrition of the arterial wall, for the circulation through the vasa vasorum will be obstructed by the stretching consequent on the increased intra-arterial pressure, just as the circulation through the coronary arteries is considerably impaired when the left ventricle is distended. Then you may get head symptoms from high arterial pressure, such as giddiness, weight, and mental sluggishness. Postural forms of vertigo are quite common in these cases.

I will just glance at an interesting question in connection with high arterial blood pressure, namely, may not such a pressure be sometimes necessary and salutary, or, at any rate, may it not be other than an unmixed evil? I think this is quite likely, for when the peripheral vessels are narrowed by unalterable organic changes—for example, in renal disease and in arterio-capillary fibrosis, or by tonic muscular contraction, as in many ailments—a certain quantity of blood must be delivered into the veins, and it can only be so delivered under very high arterial pressure. Now to lower that pressure without relieving the cause would surely do harm. The only proper treatment of high arterial pressure is that which will relieve the obstruction in the periphery of the circulation.

I come now to the question of venous pressure in clinical work. Time fails me for an adequate handling of the clinical side of this part of the subject. I can only refer to one or two points. A high arterial pressure, as a rule, maintains the normal venous pressure, or may maintain a somewhat heightened venous pressure. Then the obstruction in the peripheral circulation may be said to be compensated, the high arterial pressure caused by it maintaining a normal venous pressure. Now when the venous pressure is excessive (sometimes with a high mean arterial pressure of 150 to 166 mm., it becomes doubled), it at any rate shows that the peripheral obstruction cannot be very great, and that the heart over-compensates it. On the other hand, the arterial pressure may be very high, while the venous pressure is very low—as low, for example, as 10 or 5 mm., or it may not

even exceed zero. In such cases of course the peripheral resistance must be enormous. Occasionally this high resistance is quite temporary and of nervous origin—nervous constriction,—but it is often permanent. Again, sometimes the venous pressure is extremely low, with a normal or sub-normal arterial pressure. This lowness is due to deficient *vis a tergo*, and is raised by cardiac tonics and stimulants. I should mention that the venous pressure is not always in proportion to the fulness of the veins. When a hand is immersed into hot water to bring up the veins the venous pressure is unaltered. The rate at which the empty vein refills is generally proportionate to the pressure; therefore, if you notice that the return of blood in a portion of vein which has been emptied is slow, you may conclude that the venous pressure is low.

Had time permitted, it was my intention to have adduced a few clinical illustrations, showing the valuable aid afforded by the instrumental determination of blood pressure in the diagnosis and treatment of various chronic ailments—renal disease, gout, arterio-capillary fibrosis, atheroma, eczema, and various neuroses. I must reserve my remarks on these and other points to some other occasion.

I will conclude by epitomising the leading signs of increased and diminished arterial blood pressure. In increased arterial blood pressure you will observe—(1) The artery is felt to be full, and can often be rolled under the finger. (2) The ictus or beat of the pulse is prolonged, a quality which is not recognised under a light touch, the swell and duration of the pulsation increasing with the compression. (3) The pulse is only obliterated under increased pressure, the finger, however, very frequently failing to recognise this fact when the calibre of the radial artery is reduced. (4) The hæmadynamometer amplifies all these tactile signs of the pulse, and defines the blood pressure with certainty and accuracy. (5) The blood pressure, arterial and venous, is frequently uniform, or alters but little on changes of posture, and the radial pulse is well sustained when the wrist is elevated above the head (the gravity test). (6) The venous blood pressure may be normal, or may be increased when the peripheral resistance is compensated; or it may be subnormal, or even so low as to be immeasurable when the peripheral resistance is so excessive as to reduce the high arterial pressure

to a minimum or to zero in the veins. (7) The second aortic sound is generally accentuated. (8) The apex-beat is frequently moved to the left of its normal position. (9) The breathing is easily disturbed by slight exertion.

In diminished arterial pressure we have the following signs:—(1) the artery is flaccid, and cannot be felt; (2) the beat of the pulse is short and full, a quality easily recognised under the lightest touch,—that is to say, with little more than the mere weight of the finger upon the artery; (3) the pulsation is obliterated under slight compression; (4) on applying the hæmadynamometer the index begins to pulsate under a low pressure, and soon attains its maximum excursion; (5) the blood pressure, arterial and venous, always varies considerably with changes of posture, generally falling with recumbency, but sometimes rising for a time with the assumption of that posture when the arterial tonus is greatly reduced and the radial pulse is greatly lessened, or may even vanish when the wrist is raised above the head; (6) the venous pressure is generally normal, or may be in excess of normal, but is sometimes reduced from diminished *vis a tergo*, and not from obstruction of the peripheral vessels.

Hot Water in the Treatment of Gonorrhœa.

—Murrell ('Massachusetts Med. Journ.,' 1898, vol. xviii) advocates hot water irrigations in the treatment of acute and chronic gonorrhœa. The apparatus consists of a soft catheter, which is passed to within one inch of the prostatic urethra. It is then connected with a "gravity apparatus," in which the water is gradually heated up to the point of tolerance. The stream flows in through the catheter, and returns between the catheter and mucous membrane. Several quarts of warm water may be used at each treatment. Some patients can tolerate a temperature as high as 180° or 190° F. The following advantages are claimed for this method of treatment:—The course of the disease is shortened by at least two thirds, making the average limit of the case—viz. stoppage of the discharge—nearer one week than three. The discharge immediately changes from a purulent to that of the nature of gleet, and is reduced to a very small quantity. There is absence of chordee and pain in passing urine. Stricture, as a sequel, which is well understood to be the frequent result of using strong astringents, is improbable.

Modern Medicine, Cincinnati Lancet Clinic.

ON THE APPLICATIONS OF ELECTRICITY IN MEDICINE.

A Clinical Lecture delivered at the West London Hospital, Hammersmith, November 16th, 1898, by

W. ALDREN TURNER, M.D., F.R.C.P.,

Assistant Physician to the Hospital.

I.

GENTLEMEN,—To-day the subject for lecture is the application and uses of electricity in clinical medicine. There are three forms of electricity used in medicine—and when I say medicine I exclude surgery, for there is no intention to speak of electrolysis or the use of the cautery. The three forms are (1) static, (2) faradic, and (3) galvanic electricity. I do not propose to say anything about static electricity; it requires for its use and application complicated machines, and, except in a few instances, this form of electricity has no great practical value. Very different, however, are the applications of faradism and galvanism. The best way of introducing the subject is to show you the normal reactions on a healthy man.

Normal reactions.—Having placed the flat electrode of a faradic battery over the back of the neck, I first of all apply the sponge electrode to my hand to ascertain if the current is bearable, and then apply it to the anterior tibial muscle, with the result that you notice a very definite contraction. It may, however, have been noticed that the reaction varies according to the position of the electrode, the best contraction being obtained from the upper part, which is the motor point of the muscle. It should be borne in mind that this is not the only point from which reaction is obtained, but that point at which it is most pronounced. Although a knowledge of motor points is very desirable in this subject, it is not indispensable.

Now find the nerve which supplies the anterior tibial group of muscles behind the head of the fibula, and apply the sponge electrode to it. The result is a strong contraction of the anterior tibial and peroneal muscles.

There are therefore two ways of testing the reactions; either by applying the electrode over

the muscle itself or over its motor nerve. Exactly the same holds good for the galvanic reactions, but there are several varieties of contraction resulting from the application of the galvanic current, which may be explained in the following way. There are two poles, one positive (+) and the other negative (-). When one electrode is placed on the spine, and the other over the muscle or nerve, the circuit is completed, that is to say, the current begins to flow. As soon as one electrode is removed the circuit is opened, meaning that the current has stopped flowing. Therefore there is both a closing and an opening contraction at each pole; in other words, there are four contractions of muscle from the application of the galvanic current. The negative contraction at closing is symbolised as K.C.C. (kathodal closing contraction), and the positive as A.C.C. (anodal closing contraction); the negative contraction on opening as K.O.C. (kathodal opening contraction), and the positive as A.O.C. (anodal opening contraction).

I next want to point out to you that the strength of current required to produce visible opening contractions is usually unbearable, owing to the sensitiveness of the skin; therefore, in actual practice we only look for the closing contractions. You will naturally ask what is the order in which those contractions occur? We find by common observation that the order is as follows:—the lowest number of cells will bring out the K.C.C. By adding a few more cells we bring out the A.C.C. If a person can bear the increased current one gets next the A.O.C., and if still more cells are added there is produced the K.O.C. How many cells are required to bring out the kathodal, and how many the anodal contraction depends on the strength of individual batteries. In the battery I have here the K.C.C. is brought out by ten cells, and the A.C.C. by fifteen cells. To obtain these reactions in the case before us, first wet the skin of the forearm. The anode is placed over the neck, and the kathode over the extensors of the wrist; when the skin is touched with the latter, the circuit is made and a closing contraction produced. The poles may now be reversed by switching the battery without changing the position of the electrodes. A similar result now follows on making the circuit; but notice that of the two the kathodal closing contraction is the greater.

It will also be seen that as long as the electrode is held on the skin there is no contraction; but when removed, *i. e.* when the circuit is broken, there would be a contraction if a sufficient number of cells were in use. Such are the reactions obtained in health.

Diagnosis.—We now pass to the condition *in disease*. Electricity is used for three purposes in diseased conditions. First of all to make a diagnosis, secondly to give a prognosis, and thirdly for treatment. I shall now take up these subjects in detail.

You are confronted with, say, a case of paralysis affecting the face or the limbs, and it is desirable to ascertain whether the lesion is central or peripheral. First test the faradic excitability of the muscle and nerve. The patient now before you presents double drop-wrist, and states that he is a worker in lead. On applying the faradic current to the extensors of the forearm, there is observed a flexor contraction. From this are deduced two facts; first, owing to the degeneration of the nerves supplying the extensors there is no contraction in the extensor muscles; and secondly, owing to the absence of contraction in the extensor muscles the current is carried through to the flexor surface, and the flexor muscles are stimulated. In a condition of health, on stimulation of the extensors there is marked extension of the wrist and fingers. In this case of lead palsy there is no contraction in the extensor muscles on applying faradic electricity.

Now apply the galvanic current. You will remember what was said when showing the normal reactions to galvanism, that there were four contractions to be noted, two at closing and two at opening. On applying the kathode to the extensors of the wrist, the anode being over the nape of the neck, there is produced a definite and distinct reaction producing extension of the wrist and fingers. On reversing the current so that the anode is brought over the extensors, the contraction is decidedly greater, that is, there is ampler range of extension of the wrist. It was pointed out that the healthy reactions were in the order K.C.C., A.C.C., A.O.C., K.O.C. In this patient A.C.C. is the greater contraction, and K.C.C. the less. If one could also demonstrate the opening contractions the sequence would be K.O.C., A.O.C. Therefore, in paralysis from peripheral nerve

disease there is an alteration in the order of the galvanic reactions. Thus

Health.	Neuritis.
K.C.C.	A.C.C.
A.C.C.	K.C.C.
A.O.C.	K.O.C.
K.O.C.	A.O.C.

Hence the reaction of degeneration consists in—

- (1) Diminution or loss of faradic excitability.
- (2) Altered galvanic excitability in the order just demonstrated, that is to say, A.C.C. greater than K.C.C., and K.O.C. greater than A.O.C.

There are several points upon which one might profitably enlarge for a moment. First of all, in testing the faradic reaction in a case of paralysis, one may find hyperexcitability to faradism, but this is unusual. There may also be galvanic hyperexcitability. This is common, and is usually met with in the early stages of the reaction of degeneration. There may also be lessened galvanic excitability, commonly met with in old-standing cases.

It is now necessary to sketch the course of a case of peripheral paralysis, because what has been shown you and illustrated here occurs only at a certain period; it does not occur throughout the whole duration of the disease. The case has to be studied both as regards movement, galvanic reaction, and also faradic reaction.

Take, for example, a case of facial paralysis, which presents itself a few hours after the onset. On applying the faradic current you find that the reaction to faradism is normal. When galvanism is applied the galvanic reactions are also normal. Here, then, appears to be a contradiction of what has been already said. But if the reactions are again taken on the seventh day, or between that and the tenth day, in a severe case, the faradic reaction has disappeared, and the galvanic reaction shows the qualitative changes which have just been described. Should the patient be again tested at the end of the third week, it will be seen that faradic excitability is still lost, and that the galvanic reaction is more readily obtained and with fewer cells; that there is, in fact, increased galvanic excitability. Moreover, you get the qualitative changes which I have already pointed out. Suppose you test again at the sixth week, because at that time the patient may perhaps report that there is an increase of movement, some return of

the faradic irritability in the muscle of the face may be found, the galvanic reactions remaining the same. About the tenth week, or more commonly at the end of the third month, motion is regained, and faradic irritability has returned to the normal. Such is the course of an ordinary case of Bell's paralysis, which shows the reaction of degeneration at the tenth day, and undergoes cure at the end of three months. But variations from this are common, many cases merely presenting a diminution in faradic excitability. If, however, at the end of the twelfth week there is no return of faradic reaction, the case is a serious one. I have one patient at present in whose facial muscles there is no faradic reaction nine months after the onset. In such a case the prognosis as regards recovery of facial movement is very bad.

There are one or two practical points which it is desirable to emphasise :

(1) At the onset of a peripheral palsy, such as a Bell's palsy, absolute motor paralysis may coexist with retained and apparently normal electrical reactions. This is an important point, because one supposes that if motion is lost the galvanic reactions ought to be altered and the faradic excitability lost. But this does not occur until the end of the first week. If on the tenth day one finds the trio of symptoms already mentioned—viz. loss of motion, altered galvanic and loss of faradic excitability—the reaction of degeneration has been established.

(2) The next point is that return of motor power is usually associated with a return of faradic excitability. Thus, if the patient says he thinks there is a little movement about the mouth, or that he can close his eye better than he could, it is probable that you will find some return of faradic excitability in these muscles.

(3) Galvanic and mechanical muscular excitability go together. That is to say, that if in an old-standing case of, for instance, alcoholic neuritis, with complete foot drop and wasting of the anterior tibial muscles, hitting the muscles with the stethoscope produces a fibrillary contraction, the galvanic excitability is probably still retained. If, however, mechanical excitability is absent, the galvanic excitability is probably also gone, and the condition is serious, as the muscles have lost their proper muscular structure, and become fibrous.

A common class of case is that in which loss of

motion is associated with diminished faradic reaction and the altered galvanic reactions which have been pointed out. The case before you is one in point. It is an instance of loss of power in one leg of three weeks' duration. There is absence of the movement of dorsiflexion of the right foot, and for some little time the patient has had a feeling of numbness or pins and needles in the foot. The right knee-jerk is diminished in force, and there is blunting of sensation to touch and pain over the right leg. The gastrocnemius acts normally.

We now apply the faradic current by putting one pole at the nape of the neck and the other over the right posterior tibial nerve, and you will see there is a medium contraction in the peroneus longus and a slight reaction in the anterior tibial muscle. The galvanic reaction is altered in the usual way, A.C.C. being greater than K.C.C.

ELLIS'S PATENT SYPHON is an important advance in aerated water manufacture. The demand for aerated waters is an ever-growing one, and there is an increasing call for improved facilities for distribution, and increased protection against the waters being made the vehicle of disease. A new syphon recently patented by Messrs. R. Ellis and Son, the well-known manufacturers of table waters, of Ruthin, North Wales, is likely to prove valuable in this respect. The invention applies to the head, or the metallic part of the syphon, and is designed to overcome two recognised imperfections of the syphon head of ordinary construction. In the first place, the ordinary syphon is filled and emptied by means of the same orifice, and the act of filling forces into the body of the syphon any dust or dirt which may have collected in the outlet spout. In the second place, the mechanism of the head is inaccessible for the purpose of repairs without removing the entire head from the vase, and this must also be done to effectually clean the latter receptacle. The new syphon introduced by Messrs. R. Ellis and Son is a very ingenious variation upon the ordinary article. The outlet valve usually found at the top is here turned into an horizontal position, and placed upon the outlet spout, with lever upright. The new arrangement greatly facilitates the emptying of the syphon, as the valve can be controlled to a nicety with the thumb of either hand. The valve having thus been removed from the top of the head, the orifice for refilling is here provided, and is automatically closed with a cone-shaped plug. When filled this opening is protected with a little screw cover, and the general appearance of the new syphon, after the first impression of novelty has passed, is found to be entirely artistic. The valves may easily be repaired without expense and loss of time incurred in dismounting the head, which must be done in the case of the ordinary form of syphon. Another advantage which will appeal to the consumer is the fact that not only is the syphon easier to open, and not likely to splutter the contents out of the glass, but, owing to the arrangement of the inner chambers, less friction is caused during the process of drawing the contents, and consequently the gas does not separate itself from the water in the same degree as in the old head, the result being that a fresher, sharper aerated water is obtained for drinking. It is obvious that syphons are much more convenient to the consumers than are bottles, if only the contents can be delivered therefrom in as good condition.

MEETING OF THE SOCIETY OF ANÆSTHETISTS,

At 20 Hanover Square, January 20th, 1899;

The President, Dr. DUDLEY BUXTON,
in the Chair.

Resumed Discussion on "The Choice of an Anæsthetic."

THE PRESIDENT said they had several visitors present, and he hoped that they would take part in the discussion, considering themselves for the time being members of the Society.

Mr. HENRY DAVIS, continuing the discussion, said: Since the President had honoured him by asking him to reopen the adjourned discussion, he must first be allowed to add his tribute of praise to Mr. White for the most able manner in which he had opened the discussion. They had not yet arrived at the ideal anæsthetic, but if they could put their patients into a state of unconsciousness quickly, safely, and pleasantly, he hoped they were not far from it. But he would put safety in the first place; safety ought to be pre-eminently before their eyes when they were selecting an anæsthetic. In the ordinary run of surgical operations, he, as a rule, and unless there was any reason to the contrary, preferred gas and ether as a commencement. By that plan the patient was rendered unconscious quickly and pleasantly; he had no time to think of his heart, no time to say "he was not asleep yet," or that "he could hear what was being said around him;" the patient was rapidly got under, the heart which was beating tumultuously before, settled down into its ordinary rhythm, and the anæsthetist was then able to make up his mind what particular anæsthetic he should continue with. With regard to giving an anæsthetic to patients with surgical trouble in the upper air passages, they all knew the difficulties of these cases. Many surgeons preferred to have their patients sitting bolt upright in a chair; and if that was their preference, the anæsthetist had to give the anæsthetic to the patient in that position, though personally he never felt comfortable when the patient was so placed. But in such cases he generally started with gas and ether, unless there was any particular objection to it. After

the patient was under the anæsthetic he continued bolstering him up, as it were, with ether. If there was any prolonged operation, he did not now refer to operations on tonsils or adenoids, which were done in a few seconds, or at all events in about a half a minute, but he alluded to long operations on the cords, or about the larynx, ether could not be used, as the apparatus would be in the way. There was thus no choice but to give chloroform. He always felt unhappy when he was giving chloroform to a patient in the upright position. Still, he confessed he felt less unhappy than formerly since he read the lectures by Dr. Hewitt in the 'Lancet' at the beginning of last year. Dr. Hewitt mentioned that he, in similar cases, gave first gas and then ether, and after that felt that he could safely continue with chloroform, and he believed he had a very great success by that means. With regard to operations for adenoids, for which they all of them were frequently giving an anæsthetic, there were always difficulties and troubles connected with them. Personally, he preferred in these cases having the patient's head turned completely over the pillow, so that the blood could pass freely out, or be swabbed out with sponges. At the last meeting it was questioned whether it tended to produce more hæmorrhage than if the patient's face were on the side. He had never seen any extraordinary hæmorrhage; all bleeding ceased after a very little time. But generally the subjects of adenoids were children, and for them he was extremely fond of chloroform, as he believed ether did increase the hæmorrhage in the case of children. He thought that very young children, that is to say, under six years of age, took chloroform very well if it was administered with care. Next he would say a few words about cleft palate, in which cases he had had some little experience. He invariably had his patients right over the pillow. First he put them into the supine position, lying on the table, and gave chloroform in the ordinary way with a little flannel mask, and continued it with a little tube and a Junker. From adenoids it was an easy step to glands in the neck in young children. For these subjects he also preferred chloroform; they had no fear of impending death, such as young adults have. With regard to the reference made by Mr. Owen at the previous meeting to the deeper glands of the neck, he, Mr. Davis, had seen most fearful shock arise when these glands were being

pulled on. Next as to abdominal surgery. Unless there was any distinct indication to the contrary, he generally commenced with gas and ether, bolstering the patient up with ether; sometimes the muscles were rigid, and would not relax, and in such case chloroform had to be employed in order to relax those muscles, particularly when the hand was in the abdomen. After the muscles had relaxed, he resumed the use of ether.

A subject of importance was the administration of anæsthetics to alcoholics, and it was of interest to them all; such subjects occurred largely in hospital practice, and also, unfortunately, in private practice as well. He started with gas in these instances. As a rule it was very difficult to make such people unconscious, therefore he continued with ether to stimulate their hearts. If after having given a good dose of ether without producing unconsciousness, he felt justified in giving chloroform until he "knocked the patient down." Mr. Owen, at the last meeting, objected to that phrase, but still it was literally true, for the patient was "knocked down." Having once got the alcoholic patient under the chloroform, the anæsthetic condition could be kept up with ether.

Next he intended to briefly refer to shock. He thought dilatation of the anus caused as much shock and mental excitement as anything. If the anus was going to be dilated they of course put the patient under gas and ether, and when the stretching of the sphincters was being done they could hear the snoring breathing which they knew—he was accustomed to call it "rectal breathing," because it generally occurred in association with operations on the rectum. In such instances the patient very often required a little chloroform after the ether to make him quiet. He had also seen frightful shock after the optic nerve had been divided. If in such cases he was giving chloroform, he took care to have his ether bottle handy so as to give the patient some just before the division of the nerve.

Next as to diseases of the heart. He did not pay any attention as a rule to valvular disease of the heart in giving an anæsthetic except in those cases where compensation was not present. Where there was dropsy, or effusions into the pleuritic or peritoneal cavities, or general anasarca, he would not like to give ether. He might commence in such cases with a little gas and ether,

but he would be very cautious how he used the ether afterwards. Where one was justified in suspecting degeneration of the heart muscle, as in fatty heart, he would give as much ether as he could, provided compensation was fair. The same applied to renal disease. If there were poor compensation or dropsy was present he would give chloroform; if on the other hand there was ample compensation he would go on with ether.

In brain surgery, where the trephine was to be used, they might start with a little ether so as to stimulate the patient up, but after the portion of bone had been actually removed he would prefer to go on with the chloroform, and continue with it in all operations on the brain matter.

In conclusion he would like to say a word about the Junker's apparatus. Mr. White used the Junker very much more than he did himself. The only time he, Mr. Davis, used a "Junker" was when he wanted to use the tube as in tracheotomy or in the removal of the upper or lower jaw, and in cleft palate operations. But he did not use the apparatus with the mask; he very rarely used the mask at all. He meant in future to use the mask in the presence of gas-light to avoid the irritation caused by burning chloroform. He did not generally use the Junker face-piece, because he believed the hands of the anæsthetist were better employed in looking after the patient's head. He would certainly insist that the anæsthetists should be antiseptic. Mr. Owen thought they were very often septic.

The PRESIDENT said Mr. Warrington Haward's name was associated with the use of ether. He came forward at a time when little was known about ether, and showed the value of that anæsthetic. It was therefore particularly gratifying to find Mr. Haward present that evening.

Mr. WARRINGTON HAWARD said he must first thank the President of the Society for the courteous invitation to be present at the discussion on the choice of an anæsthetic. He would have been content to come merely as a listener and as a learner but for one thing, that he felt a very great desire to controvert something that fell from his old friend Mr. Owen at the last meeting. He had so great a regard for Mr. Owen and for all he said and did, that he felt some justification was needed for rising to oppose what he so well said at the Society's last meeting. He would only give

that small justification for venturing to do so. The point upon which he especially wished to speak was in reference to Mr. Owen's remark as to the most appropriate anæsthetic for children. Mr. Owen advocated chloroform especially for children. It so happened that a rather large proportion of his own practice years ago at the Children's Hospital, and since in a fairly extensive private practice, was amongst children; and beside that, many years ago—he did not like to think how many years ago—he had a large experience in the practical administration of anæsthetics at St. George's Hospital, and a good deal in private practice. It was then his duty to give an anæsthetic to a large number of people of all ages in all conditions. It happened that his experience was pretty equally divided between ether and chloroform, because in the first part of his anæsthetic practice he gave chloroform almost exclusively, except for small operations for which gas was adequate; in the latter part of his experience he administered ether almost entirely. He did not wish to go into the relative merits of ether and chloroform, except especially with regard to what Mr. Owen said about children. He would be very sorry indeed if the authority of this Society endorsed the advice given by Mr. Owen as to the appropriateness of chloroform for children, for the following reasons: first of all because he thought it was wrong, and secondly because there seemed to be an idea prevalent in many quarters that whatever might be the facts about chloroform and its danger in the case of adults, for children, if given with due care and skill, it was a safe anæsthetic.

Now he wished most emphatically to say that that was not the case. Chloroform was not a safe anæsthetic for children any more than he believed it was for adults. Mr. White had quoted statistics showing that 10 per cent. of the recorded deaths under anæsthetics occurred in children; and it so happened that nearly all the cases in which he had seen a patient brought into serious peril from the effects of an anæsthetic had been those in young children in whom chloroform was the anæsthetic given. It must, he was sure, be familiar to all those who had practised in children's hospitals that children were very sensitive, and were apt to become very suddenly profoundly under the influence of an anæsthetic. For a time they seemed

to be only gently passing under the influence of the drug; but then—apparently, he supposed, because of the comparatively small amount of blood in their bodies—they suddenly became overpowered, so much so as to excite apprehension in the mind of the administrator or operator. He recollected at the Children's Hospital years ago, when chloroform was given by an excellent administrator, they were often obliged to stop the operation and to stimulate the patient, using artificial respiration and other steps to recover the patient. The child was in a condition of extreme and profound insensibility, with dilated pupils and diminished respiratory movement. That experience occurred not uncommonly with chloroform. Moreover, they found a considerable number of cases on record of children having died quite suddenly under the influence of chloroform. He had not seen these cases under ether, and he was not familiar with any such occurrence having taken place under ether. Mr. White had given statistics to show that the mortality under chloroform was about 1 in 3000, while under ether the mortality was 1 in 16,000; or, as one of the French surgeons had said, 1 in 23,000. It was very difficult to give accurate statistics on that matter, but he did not think any one present would be prepared to deny that the mortality under chloroform administration was higher than under ether. Mr. Barker at the last meeting said that there were two points of view from which an anæsthetic might be regarded—from the point of view of the anæsthetist, and from the point of view of the surgeon. True, those were not always identical. He would strongly urge upon their Society the necessity for regarding it from the point of view of the patient.

If a patient about to have an anæsthetic were asked the point which he chiefly insisted upon, he would say "safety; before all other things let it be a safe one." Therefore it seemed to him that anyone who advocated the use of an anæsthetic under which the mortality was 1 in 3000 as against one in which the mortality was 1 in 16,000, incurred a very grave responsibility, and ought to have very strong reasons indeed for his preference. He had already said that he had the greatest respect for all Mr. Owen said and did, and in looking over his speech he searched expectantly for some very cogent reasons for his preference for chloroform as against ether for children. But he could only

find that he stated it was very nasty, that it was "beastly stuff." The taste of both chloroform and ether he admitted was not nice, but it was quite a matter of choice which was the nastiest. He could speak from personal experience. He had to take ether at the end of a long and exhausting illness, and it was so given by his old friend, Mr. Woodhouse Braine, that he had not the smallest discomfort or unpleasantness, because first of all gas was administered, and then he was not conscious of the taste of the ether. The gas given ought to be only of sufficient quantity to produce that effect—the dulling of the taste perception of the ether. He did not think there was any necessity for getting the patient into a very extreme state of anæsthesia by its use. Therefore the nastiness of ether to the taste and smell was not worthy of much consideration; the nastiness could be got rid of, and the safety of the ether remained. It seemed to him also that children were very liable to the effects of shock. An operation sometimes produced an effect on the child which was quite out of proportion to that which would be produced on an adult. Children suffered severely from shock, notwithstanding that they had not the same fear and dread as adults, and that was particularly the case in operations on the nervous system. Ether seemed to be of great service as an antagonist to this shock. He would reiterate that the dangers appertaining to chloroform were of a kind which arise as a rule suddenly, and which therefore it was very difficult to avert, such as sudden failure of the heart's action. They did not find the same thing with ether. When there was any difficulty or death with ether, the trouble was with the respiratory function, from the danger of a sad end from which the patient could usually be recovered by the alert administrator.

What he wanted to protest against was the notion that chloroform was a safe anæsthetic for children. He did not mean to convey the impression that any anæsthetic was absolutely safe; most anæsthetics would be safe in the hands of members of that Society. But it could not be expected that they could bring a patient into a state of profound insensibility without some risk, and he was strongly of opinion that no anæsthetic should be given to anyone without good and sufficient cause. He thought the benevolence of the profession in the present day, and their

desire to avoid any pain to patients, often led medical men to give an anæsthetic for small matters which could really be done without it. But the giving of an anæsthetic should always be looked upon as attended with some risk. That was a very strong reason for the observation of that excellent maxim of an old surgeon, "we will do one thing at a time, gentlemen, and have no talk." That was an excellent proverb, both for the surgeon and for the anæsthetist.

With regard to the use of anæsthetics in abdominal operations, it has been stated by some speakers that the relaxation of the abdominal muscles could not be so well obtained or produced by ether as by chloroform. That was not his own experience at all. He had had occasion, as every hospital surgeon now-a-days had, to do a good many abdominal operations; hardly a week passed without one, and he had not observed that there had been less muscular relaxation under ether than under chloroform. It seemed to him that ether was as well suited for abdominal operations as for any others.

With regard to operations on the throat and the removal of adenoid growths and tonsils, he had no reason at all to think that in these operations there was more hæmorrhage or anything which made ether a less appropriate anæsthetic than chloroform. For cleft palate, however, he believed chloroform was the proper anæsthetic. He had given ether, years ago, for a good many operations of the kind, and he had also done many operations on children for the same condition, but he had no doubt that chloroform was the proper anæsthetic for that, as ether produced a great secretion of thyroïdal mucus, which was a great nuisance in the operation, and acted to the detriment of the operator. So also for operations on the larynx and upper air passages, and for tracheotomy, ether could not be conveniently given. He had no doubt that a great many more operations could be done under the influence of nitrous oxide than were done at the present time. Gas was universally admitted to be very safe, and not by any means unpleasant.

He would like to say a word about cocaine. He agreed with what had been said by others there, that cocaine was not a proper thing to use, that is to say, by injection for the production of local anæsthesia. It was not proper for children,

they seemed to be very susceptible to its poisonous effects. But he thought that cocaine might be used for many severe abdominal operations. Every one familiar with the administration of anæsthetics would admit that amongst the most embarrassing cases were those in which there were abdominal distension, and intestinal obstruction with vomiting. These were always very disagreeable cases to give anæsthetics to. He had on a good many occasions operated on cases of this sort without a general anæsthetic, substituting the local application of cocaine. Some little time ago he had occasion to operate on an extremely fat woman (she had several inches of fat on the abdominal surface, and was four or five months pregnant) who had intestinal obstruction and an old irreducible hernia. It was doubtful whether the obstruction was anything to do with the hernia or not, but it was found not to be. He had to open the abdomen, and he found a band stretching from the hernial sac across the abdomen, causing strangulation. He divided this, and sewed up the abdomen. The patient was vomiting and breathing very badly, and her pulse was bad. The cocaine was injected at 1 per cent. strength by a long syringe, gradually withdrawing the syringe and injecting as one went along, thus leaving a line of anæsthesia in the track of the incision. The patient assured him that she had no material pain, except when some part of the intestine had to be pushed back, when she said there was a griping pain. That was the kind of movement which was known to produce intestinal pain, and therefore it was not surprising that she felt pain then. The patient did not complain of pain during the sewing, or afterwards. He had several times operated for hernia in the same way, and also for conditions causing intestinal obstruction; there being very little pain and perfect quietude on the part of the patient. Therefore he thought cocaine might reasonably be used more than it was at present.

Subacute Bronchitis in Infants and Children.—E. M. Dupaquier says that it is now a clinical aphorism that the influence of infection from the intestine is of practical importance in respiratory disorders. It is, indeed, a common occurrence in pronounced cases of gastro-enteritis that the intestinal germs pass into the blood and

invade the lungs. Such cases are usually acute infections of violent and rapid course, too often fatal. In a less degree we remark the relation of abnormal digestion and chronic bronchial troubles; yet Comby insists upon classing improper alimentation among the most prevalent causes of chronic bronchitis. In a still much less degree the relation between the intestine and bronchi can be suspected when the digestive disorders are, so to say, latent, for in such cases autointoxication is a slow process, and the dyspeptic symptoms are hardly noticed and reported by children. One of the general symptoms and distant manifestations of this slow process of autointoxication is repeated, and stubborn attacks of bronchial catarrh. There is a low fever and coughing, with secretions, lasting several weeks at a time. It is not like an attack of acute bronchitis, lasting at most one week and vanishing spontaneously. It is not like chronic bronchitis, where fever is absent, and where cod-liver oil, iodides, and arsenic will bring about improvement. In the subacute form alluded to any such medication may aggravate rather than ameliorate the symptoms. This class of patients has inherited a gastro-intestinal taint. Nearly the entire management of these cases of bronchitis is contained in the vital question of feeding. For infants medication is limited to antiseptics of the mouth (boric acid), stomach, and intestines (calomel, and enteroclysis with boracic solutions), and antiseptics of the bronchi, as Robin says, by vomiting (ipecac., hot water, and salt). The milk supply must be seen to be perfect, and open air and bathing, with other hygienic measures, must be prescribed. In older children strychnine and cinchona may be used in addition to the antiseptics used in the case of infants.

New Orleans Med. Journ., December;
American Journ. of Obstetrics, January, 1899.

A Positive Case of Sounding of the Fallopian Tubes.—By Dr. F. C. Floeckinger (Galveston) ('Centralbl. f. Gynäk.', 1898, No. 34). Having occasion to perform a laparotomy for tumour on a young woman, the author experimentally passed a sound into the uterine cavity and beyond a depth down to the handle of the instrument. On opening the abdomen he was able to convince himself by sight and touch that the sound had passed into the left Fallopian tube. He was able to pass the sound into the opposite tube.

Post-Graduate.

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* Specially reported for The Clinical Journal. Revised by the Author.

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TWO CLINICAL LECTURES

ON

SOME PRACTICAL POINTS IN THE DIAGNOSIS AND TREATMENT OF SKIN DISEASES.

LECTURE I.

Delivered at the West London Hospital, Hammersmith, January 23rd, 1899,

By PHINEAS S. ABRAHAM, M.A., M.D.,
B.Sc., F.R.C.S.I., &c.,

Dermatologist to the Hospital, and Assistant Surgeon to the Hospital for Diseases of the Skin, Blackfriars.

GENTLEMEN,—It is my intention to-day to show you several cases of disease of the skin, and to direct your attention especially to certain points as regards their practical diagnosis and methods of examination. I shall not go very much into theoretical matters, because I think post-graduates have opportunities of reading these things for themselves, and can thus acquire a knowledge of them much better than when spoken. I think the chief use of these lectures is to enable you to see cases, and to have little practical points demonstrated to you from the point of view of both diagnosis and treatment.

The first case is that of a boy who came here on Friday last with a very bad pustular and crustate eruption on the side of his head and ear. The case was obviously one of impetigo contagiosa, a disease which is extremely common in this part of London; the small houses of Hammersmith seem impregnated with the staphylococcus aureus and albus, which are the cause of this disease, and there is an immense amount of it about. I think we may safely say that 35 or 40 per cent. of the cases which attend the out-patient Skin Department of this hospital turn out to be instances of impetigo contagiosa. But that is not the point of interest in this boy. If I had been a bit careless or hurried in my examination I should have

been content with that diagnosis, but it is important in every skin case to find out whether there is anything else wrong with the patient beyond what is at first seen. I inquired whether he had spots elsewhere, thinking perhaps he would have impetigo in other places, and then I found that the boy had got typical psoriasis on his body, of which he had been the subject since three years of age, though the mother did not at first mention the fact to me; she simply brought him because of the sore on his ear, which, by the by, is now very much better as a result of the treatment. This illustrates also one of the practical points in examining skin cases. It is always well to see as much of the skin as you can. On the Continent this fact is thoroughly well recognised, and in some clinics the practice is to bring both male and female patients up for inspection in a perfectly naked condition. Although this is often unnecessary and inadvisable, we must admit that by so doing one can sometimes see points which the patient himself or herself was not aware of. The psoriasis in this case is of the crusted limpet-shell variety. The patient also illustrates the efficacy of the treatment for impetigo, because in a few days the crusts have all disappeared. I may mention here that this method of treating impetigo results in a perfectly certain cure, and though it is very old-fashioned it does not seem to be as much in vogue as it should be. It consists in the ammoniochloride of mercury ointment, forty grains to the ounce. This cures it in a comparatively short time. So as to ensure asepsis and cleanliness as much as possible I generally order some antiseptic lotion, and that which is very largely used by me in this hospital is creolin lotion (3ss to 3x of water). Creolin is a tar preparation which, mixed with water, forms a milky fluid. It is an excellent antiseptic preparation for all these conditions; it also has an antipyretic action, and stops the itching. I generally instruct the patients to use this lotion mixed with an equal part of hot water, applying this first, and then putting on the ointment. The warm lotion has the effect of softening the crusts, and allowing the ointment to act on the skin itself more readily. The old-fashioned way of treating these crusted impetiginous cases was to poultice. Occasionally that may be proper, but I think very strongly that the warmth and moisture of a poultice only help the growth of the

micro-organisms; but by the method I have mentioned you can absolutely cure these cases in at most a week or two. It is a point well worth remembering, especially in vaccination cases. Where impetigo happens to be prevalent it is not an uncommon thing to have a vaccination wound infected with impetigo, and the impetigo in such cases often is put down to the vaccination itself. I think if medical men would only just watch cases, and if they see any purulent collection, or anything which looks like impetigo, remaining after the vaccine pustule should have dried up, and apply the ointment I have referred to, they would cure the condition, and thus dispose of half the opposition to vaccination, on this account at any rate.

The next is a case of erythematous lupus on the nose of a young woman. At one time it involved the greater extent of the nose, and a large portion of it was erythematous and superficially ulcerated, leaving scar tissue with a somewhat serpiginous outline. From a diagnostic point of view the case is interesting, because in its present form I do not think anyone would suspect it to be erythematous lupus. Now it looks more like a syphilide than anything else. It shows the importance of going well into the case; we have to look elsewhere, and on examining her ears we find she still has the typical erythematous lupus condition there, and the orange-peel appearance inside the conchæ, which Mr. Hutchinson first pointed out, and also scarring of the lobes of the ears. Apart from that she has obviously had scrofulous glands in the neck in early youth. Mr. Hutchinson, I believe, thinks that erythematous lupus is more frequent in tuberculous patients than even the common lupus. That view is not absolutely accepted; still there is a good deal in it, for in my experience many of them do give a history of tubercle in the family, if not in themselves. As there is not another case of lupus coming to-day, I want to draw your attention to a little instrument* which embodies a method devised by Dr. Unna of Hamburg. One must remember that the typical tissue in ordinary lupus is a yellowish new growth, which Mr. Hutchinson calls the "apple-jelly" growth. This is sometimes very obvious. It may form a tubercle, as it were, sticking out, or it may be a very small mass in the skin itself. And if it

* Made for me by Mayer and Meltzer.

happens to be crusted over, and there is some erythema and induration around, it may not be seen properly, but by pressing the blood away from the part by a flat piece of glass the yellow apple-jelly tissue is seen remaining behind. Dr. Unna uses a plain piece of glass, which he holds between the finger and thumb, and it occurred to me to use the same thing with a handle attached to it, so that the examiner and others can see without the hand getting in the way. I hope you will examine the ears, because that is very important in the diagnosis of lupus erythematosus. She is getting on very well under applications of pure carbolic acid and pure camphor—the so-called pheno-camph—applied locally occasionally. She is using an ointment consisting of ichthyol and some mercuric preparation, and is also taking iron and quinine in five-grain doses internally.

The next patient is a man who presents a very rare condition, namely, the "dermatitis herpetiformis" of Duhring. It is in a comparatively early condition, and one does not often get such a typical instance as this. It seems that this man was in very good health until quite recently. Some nine months ago he became ill, and his kidneys seemed to be affected, resulting in retention of urine for some time, and this retention caused the man a good deal of anxiety. After a short time groups of erythematous patches appeared in a sort of herpetiform manner, that is to say in clusters, and these clusters are quite symmetrical—they occur on each shoulder, on the backs of each elbow, the fronts of the axillæ, and about the buttocks and anus. When I saw him first, about two weeks ago, these were chiefly papular, and some of them were vesicular. Then the vesicles enlarged, and they became bullæ about a quarter to half an inch across, on an erythematous base. I show you the case to-day because the lesions are disappearing so rapidly that I could not perhaps demonstrate them later on. He is to be exhibited on Wednesday at the Dermatological Society, and I am afraid some of the dermatologists will ask me where are the bullæ, as they will be then practically gone. Dr. Duhring, who is one of the ablest dermatologists in America, and I may say in the world, placed cases such as this in a class, that is to say, he separated them from pemphigus and hydroa, &c., and his views are now being accepted, dermatitis

herpetiformis being now looked upon by many other authorities as a distinct disease, *sui generis*. In many cases the lesions extend, and may become quite generalised. It is an affection often most rebellious to treatment. I have not mentioned one important symptom, a subjective one, namely, an intolerable burning and itching from which the patient suffers. This man, as he will tell you himself, was in a terrible state a fortnight or so ago, and Dr. Taylor, of Richmond, who kindly sent the case to me, also wrote about his condition. He is taking arsenic, which is the proper thing to give, and it does seem to do good sometimes. Dr. Duhring has found that tar applications are also useful; so also is sulphur. But they are very obstinate cases as a rule. I am giving him strychnine with the arsenic internally, and he is using creolin in baths, lotion, and ointment (see fig., p. 263).

The next patient is a boy, and his case is also interesting from the diagnostic point of view. You will see two raw lesions about an inch in diameter on the upper part of the right thigh, and will probably admit that they are now a pair of healthy healing ulcers. When he came to me there was a punched-out appearance about the sores, which were very much deeper than they are now, and with overhanging edges. The history was that he had had the sores two months, and that previously to that there had been "sores" in his family—his father and brothers had all had them. In my mind it was probable that it was impetigo contagiosa running through the house, but that this boy scratched his lesions and inoculated them. They certainly presented the appearance of tuberculous ulcerations, and they somewhat reminded one also of a very rare affection, which we do not often see in this country—Alleppo evil, or Oriental sore, which occurs in India and other tropical parts of the world. The ulcers are irregular and "punched out," resembling in some measure a tertiary syphilide. This boy is dressing the ulcers with iodoform and a mercurial ointment, and is taking phosphate of iron. The interest of the case is in the fact that without care one could easily make a mistake in the diagnosis.

The next patient is a girl, who says that she had a spot on her hand some time ago, and as she thought it was ringworm, she went to the chemist and put on what he sold to her, and ink and various

other things as well, with the result that she has now got this extensive vesicating erythema, looking like erythema iris, but with outlying vesications. The first question one naturally asks is, What has she been putting on? Then the history becomes unfolded. To satisfy my conscience, this afternoon I took a scraping and looked for mycelium, but, as I expected, could not find any. I believe it has nothing to do with ringworm, but that it is simply a case of some eczema which has been irritated until it became vesicating. I have seen many factitious rashes like that. There is no suspicion in this case that it has been intentionally produced; it is the result of some application applied with the idea of relieving the original affection. Why patients should wilfully produce these artificial eruptions, as they do sometimes, nobody knows. I remember one case which occurred in a hospital when I was in Dublin, in which the young woman developed great bullæ and blisters in various parts of her body and limbs, always on spots where her hands could reach. An artificial production was suspected, and she was watched. She was put into a ward and kept in bed; nevertheless, every now and then fresh blisters would appear. Ultimately Dr. Walter Smith—I think it was,—who had the case, determined to find out what it was. He knew it was artificial, and he gave orders that she was to be moved from that ward into another. Her clothes were to be taken from her, and nothing belonging to her was to go, as he expected she was hiding something. When she was being moved, the last thing she said was, "I must take my Prayer-book with me;" and with that she made a clutch at it. The nurse tried to get it at the same time, and succeeded, and thereupon out of the leaves fell some blistering paper, which appeared to have been quietly kept in this Prayer-book for the mystification of the doctors. Of course the rash soon disappeared after this discovery.

The next case is a child the subject of comedones. When she was brought here the other day she had black comedo points on the back and chest, from which positions they have now disappeared. Comedones in children are by no means common. They occur, however, sometimes on their foreheads, and more rarely on the chest and back. I have here an illustration of a case which we had in this hospital some time ago, in an

infant one year old, in which the comedones were very well marked, and developed into acne. I have only come across three or four cases of the kind. This child has a slight xerodermic condition of the skin, the roughness being apparent to the touch.

Next we have two children with impetigo of the scalp and pediculosis. With the lens you can see the ova some distance from the roots of the hair in various places. This child has also got impetigo in the neck and various other parts. This is the most common disease in this part of London, as I have said, and I am sorry to say that I think the board schools are somewhat responsible for it. Some few years ago Dr. Aldersmith and myself were asked to examine two Board schools in the East end of London for a supposed epidemic of ringworm which had broken out. We did not find much ringworm, but we found an enormous amount of impetigo and pediculosis capitis; in fact, only two per cent. of the girls in the two schools were free from pediculi. That is some few years ago. In spite of our report little seems to have been done; but at length, I hope, the present School Board will take the matter into serious consideration, and try to do something to mitigate the evil. In the first place you see by these examples how fond poor people are of keeping their children with long hair, even the boys. On the Continent the children in public schools often have short hair, and that of the small girls is cropped quite short; they are much healthier in every way in consequence, and their heads can be kept clean. Fortunately, as in the case of some other loathsome skin diseases, we can cure the condition quickly. I recommend these children to have their heads washed every night with some antiseptic lotion mixed with hot water and lathered on with soft soap. As a matter of fact, the lotion is of no consequence; it is simply to ensure the thorough use of the soft soap. After this process, ammonio-chloride of mercury ointment is rubbed in, and that will cure the disease. I try to make them do this for a week or two after the disease appears to be cured, so as to catch the young pediculi which may subsequently emerge from any ova which may be left behind. Of course, have the hair cropped short, especially at the back.

Here we have a boy who is a very old case of ringworm. It illustrates a sort of negative fact.

According to the text-books, when you get a case of kerion it means that the ringworm is being cured, but this boy came here nearly a year ago with a very severe kerion,—in fact, you can see the scarring which was produced. The top of the head was one mass of boggy inflammation, yet the ringworm was not cured till long afterwards. Now there are no short hairs to be seen I think the tinea has now practically gone. I shall reserve the treatment of ringworm for the next lecture.

This middle-aged woman came here with several patches of a dry scaly character on each leg, and the question was whether it was a simple psoriasis or a patch of seborrhœic dry eczema, or a syphilide. The presence of these other scars at once led me to believe that it was probably specific, and, in consequence, I am putting her upon iodide internally and mercuric applications externally. She appears to have had something wrong with the leg when she was quite young, and the scarring makes me think that it was a case of specific trouble. The patches itch, and that fact is somewhat against its specific nature; but pruritus is sometimes associated with syphilis, and in this case it may also be connected with her varicose veins. You must always remember that it is quite possible to have a combination of skin diseases. It is not fair to assume that because a person has had a syphilitic affection he or she cannot subsequently have eczema for instance; often one's diagnosis may be complicated by that fact.

The next woman presents a typical case of psoriasis, and it is very much crusted, the hypertrophied epidermis being hard and white, and piled up. The fact of having a dry eruption on the knee is not quite sufficient to fix the diagnosis of psoriasis; you may have eczema there, and there may also be ichthyosis and many other things. The condition has not appeared on the elbows. When you have a reddish eruption with not much epidermic thickening, only thin scaling coming off in flakes, and situated on the flexor surface of the forearm, one is apt to suspect that the trouble is specific. When I first saw this case I was going to ask questions about sore throat, &c., thinking it might be a secondary syphilide. However, she volunteered a statement about other parts being implicated, and that she was here before for a disease some time ago, and cured by tar applications, and so on. It is really a simple

case of psoriasis, which will soon be got right. She is using a tar ointment now, and a tar lotion.

This young girl was the subject of pityriasis rosea of Gibert. She does not show much evidence of it now because it has nearly disappeared. It is an affection which begins with a slightly scaly patch which reminds one almost of a patch of tinea circinata, a slight yellowish or brownish furfureous desquamation in the centre, with a reddish ring around. Such a patch may appear on the neck or chest or arms, and remain for some few days, and then suddenly similar but smaller patches appear on the chest and back, and spread all the way down the body. The condition may last from a fortnight to several months, and then spontaneously disappear. This disease, in some of its manifestations, is often mistaken for secondary syphilis, and therein lies, to my mind, the great importance of being able to diagnose it. I have seen it mistaken several times for that disease. In one case a gentleman's wife had an attack, and the doctor, very rightly from the description in the books, thought it must be a secondary syphilide, and started at once to treat the lady. The husband knew something about medicine, and seeing that the treatment was mercurial, at once suspected there was something wrong with his wife in the direction of syphilis. The doctor was a friend of mine, and a consultation soon cleared up matters. In another case the disease appeared in a friend of mine, a medical man, who had never seen or heard of pityriasis rosea, because it is not very common, and it was not mentioned in the old text-books on skin disease in this country, although it was described by Gibert some thirty or forty years ago, and was well understood in France. This doctor, from his reading, thought his trouble must be secondary syphilis, and although he was sure that he could not have contracted that disease he was in a terrible state of mind and got quite ill over it. As soon as his mind was cleared on the point that it was not syphilis the affection cleared up also. It is generally associated with some disturbance of digestion. This girl had for a long time suffered from indigestion in various ways. At the present time the eruption has all disappeared with the exception of a little on the face, and there is a small recurrence on the thigh. It has been all over the chest and abdomen. It forms somewhat annular patches, and has a rosy ring around.

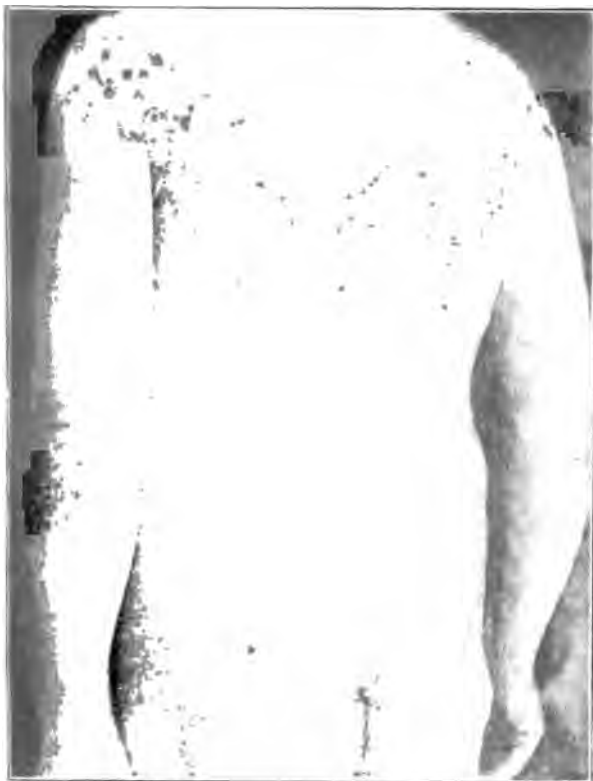
Some practical points in the diagnosis of diseases of the skin you have been able to see to-day. It is, of course, important to get as much history as you possibly can from a patient, though we do not depend entirely upon histories. The dermatologist or the experienced practitioner is able, from the appearance of the lesions themselves in most cases of skin diseases, to determine their actual nature, and in this respect I think we are more fortunate than the general physician in having a superficial part of the body to deal with, where the affections are so obvious. If you depended on histories alone you would often go wrong. For instance, in syphilitic cases. There was a syphilitic case which should have come here this afternoon; it is a girl with well-marked secondary syphilis, absolutely certain from its physical features. She has a brownish papular eruption on her face and neck and flexor surfaces, and she is losing her hair; there are also swollen glands. But she denied absolutely any primary affection whatever. In spite of that fact, when she came three weeks ago I put her upon mercury at once, and made her use a chlorate of potash gargle and rub mercurial ointment into her face. The result is that the rash is disappearing rapidly and improvement is general. It was without doubt a case of secondary syphilis. When a case of syphilis turns up in a woman it is often hardly necessary to question her; the symptoms are either there or they are not. If the patient swore positively that it was impossible that she could have the disease, it would make no difference in the treatment if the manifestations were present. Of course, very often in the case of women they simply do not know; it may not be intentional dishonesty in their case, for they may really have no idea when they have got the primary affection. We constantly come across middle-aged women with tertiary manifestations of syphilis; they have had children and they have had miscarriages, and so on, but they have not the least notion they ever had syphilis. Nevertheless, if you put them on iodide of potassium and other suitable treatment, you find the lesions disappear. If you do not put them on antisyphilitic treatment they go from bad to worse, and other syphilitic manifestations may supervene. And even in men, in whom there is often no attempt to hide the facts, even they sometimes do not know that they have had syphilis, though they admit having

been in the way of getting it. I have had several such cases in my experience, and most practitioners, I believe, are aware of similar facts. You have to be certain of your diagnosis from what you see, and you must treat accordingly.

Of course, in the diagnosis of skin complaints we have to keep in mind what we have to look for. We ought to have a fair knowledge of the so-called elementary lesions of the skin; that is to say, what is meant by "macula," "papule," "vesicle," "pustule," "bleb," "wheal," "cicatrix," "tubercle," "tumour," and so on, because some diseases appear with these special lesions, and in many cases the particular elementary lesion is typical of the affection right through. But it is not always so. Several skin affections are characterised by the multiformity of the lesions, several of them being present at the same time, or the one may develop into the other. For instance, it is not an infrequent thing for a vesicle to turn into a pustule, or for a macula to be raised and papular. One practical point, which we have not got in this room, is to view your cases in a good light, because often small differences in tint make an immense difference in the diagnosis of the case. But tint is not everything. For instance, in syphilis; it used to be the custom to suspect syphilis whenever there was an affection taking on a raw-hammy colour. We now know that there are several diseases which have that colour; even simple psoriasis may have a brownish-red tint when the scales are removed. Lichen planus has a red and brownish tint. One has to examine these things very carefully, and it is a useful thing to carry a lens with you. I find an ordinary watchmaker's glass, which you can put in your eye, very serviceable; they are inexpensive and very convenient. I also carry a compound platyscopic lens with me, which gives a big field and will magnify up to twenty-five diameters; that is to say, it is strong enough to show the acari of itch, and thus saving one the trouble of putting objects under the microscope. One should always have a microscope with one in skin work, because the best man cannot always be certain of his diagnosis without that instrument. Cases of ringworm sometimes come before you in which the typical short hairs are not to be seen by the naked eye or the ordinary lens; there may be so much crust and so much pityriasis or seborrhœa present that

nothing but a careful microscopical examination of a scraping will enable you to determine their true character. I asked some cases to come up this afternoon, so that I might demonstrate the way of putting hairs under the microscope and ascertaining whether the fungus was present, but they have not come.

On Thursday next we will see some more cases, including one of very severe pemphigus in one of the wards.



Dr. Abraham's case of Dermatitis Herpetiformis of Duhring (see page 259).

NOTE.—It is right to state that these lectures were not delivered with a view to publication, and were not reported at Dr. Abraham's request. The lectures were taken down *verbatim*, and are printed with verbal corrections only.

ON THE APPLICATIONS OF ELECTRICITY IN MEDICINE.

A Clinical Lecture delivered at the West London Hospital, Hammersmith, November 18th, 1898, by

W. ALDREN TURNER, M.D., F.R.C.P.,

Assistant Physician to the Hospital.

II.

AFTER recapitulating the chief points of Lecture I, Dr. Turner proceeded :

GENTLEMEN,—It is now necessary to explain why the alterations in electrical irritability already detailed occur in peripheral nerve palsy and in certain forms of spinal disease, and why they are not obtained in palsies arising from brain disease. For this purpose an idea of the architecture of the central and peripheral nervous systems is necessary—at all events of the efferent portions of the nervous system, which commence in the cerebral cortex. There is a neuron system extending from the cerebral cortex to the bulb and the spinal cord, the neuron consisting of a cortical cell with its axis-cylinder process or fibre. From the bulb and spinal cord to the periphery there is another neuron, consisting of a multipolar spinal cell and its axis-cylinder process or fibre. So that the motor or efferent nervous system consists of two neuron systems, one from the cortex to the anterior horns of the spinal cord and their analogues in the bulb, and the other from the bulb and spinal cord to the muscles. The point I wish to emphasise is, that lesions of the first or primary system of neurons, consisting of the pyramidal fibres from the cortex to the spinal cord, do not give rise to electrical alterations, but that lesions of the secondary efferent neuron extending from the anterior horns of the spinal cord to the periphery, give rise to the characteristic electrical changes which have been described. So that when confronted with a case presenting the reaction of degeneration, one may say with perfect confidence that there exists a lesion of the secondary or peripheral neuron system.

Common diseases of the primary neuron system are such affections as hemiplegia and lateral sclerosis. In those conditions there is no

qualitatively altered electrical excitability. The diseases of the secondary or peripheral neuron system are peripheral neuritis in any locality and acute anterior poliomyelitis (acute inflammatory conditions of the anterior horns of the spinal cord). It is in those two diseases that one finds the characteristic reaction of degeneration. But there is a third set of cases, namely, the chronic degenerative conditions, diseases which are not uncommon in neurological practice, examples of which are progressive muscular atrophy and bulbar paralysis, in which the primary, or at all events the chief lesion is an atrophic degeneration of the cells of the secondary neurons. In those chronic degenerative conditions you do not find the characteristic reaction of degeneration, but merely a quantitative diminution both to faradic and galvanic excitability.

Prognosis.—We now come to the important subject of prognosis. Let us take, for instance, a case of ordinary peripheral facial paralysis electrically examined on the tenth day. Of this, three varieties or types may be met—

(a) In one series of cases there is paralysis *plus* normal faradic irritability of the paralysed muscles. In cases of this nature one may say with certainty that recovery will take place in from two to three weeks. This is a common enough condition in slight degrees of facial paralysis, but it is not met with in the severer neurites resulting from alcoholic or lead poisoning.

(b) The second type of case is paralysis *plus* diminished faradic irritability, *plus* the qualitative galvanic reactions already described as the reaction of degeneration. This is a common type of case, for it is only in severe cases of facial palsy that the faradic reaction is entirely lost. One may, however, find loss of faradic excitability in the orofacial muscles, while it is partly retained in the oculo-facial group. Recovery in these cases takes place in from six weeks to two months.

(c) The third class of case consists in paralysis *plus* loss of faradic irritability, and *plus* the qualitative galvanic changes of the R.D. Recovery in these cases takes longer—from three, six, to nine months. In this variety it is necessary to periodically test the faradic irritability. If at the end of six weeks the faradic reaction is beginning to come back, you may say recovery will ensue in about three months. If, however, at the end of three

months the faradic reaction has not yet returned, it is necessary to give a guarded prognosis as regards time or ultimate complete recovery. Bearing these facts in mind, you should be able to form a prognosis with respect to most of the cases which you are likely to meet.

Treatment.—Electricity is used in clinical medicine as a therapeutic agent with the following objects:—First, to maintain paralysed muscles in a state of nutrition while the injured or inflamed nerve is undergoing repair. Secondly, to relieve pain and spasm. Thirdly, to promote the general nutrition of the body, and more especially the neuro-muscular system.

As regards the first indication for treatment. The question was asked at the end of the last lecture, when galvanism or when faradism should be used. It is clear that in paralysis that form of electricity should be used to which the muscles respond.

Referring for one moment to the three types of case described under the section on prognosis, it is evident that in Types I and II, should the application of electricity be deemed necessary, faradism is that form which should be applied; and in Type III, galvanism. But there is a useful method of applying electricity known as the combined galvano-faradic method. By means of a simple arrangement, the de Watteville table, both faradism and galvanism may be applied at the same time. In the cases under Type II, above described, this would probably be the most satisfactory form to apply. It may be asked, How often should electricity be applied in a case of muscular paralysis? It should be applied once a day, and preferably combined with massage. The best nutritive effects are obtained by a combination of these two methods. In a case of alcoholic paralysis affecting the legs, for instance, electricity may be given in the morning, massage in the evening. This is easily carried out, as the patient is of necessity confined to bed. It is also of importance to know how long the application of electricity should be continued. If a case is doing well, when are you to stop applying electricity? That must be a matter of individual judgment. It is well to continue the electricity until the muscles present a reaction equal or almost equal to that of the muscles in a state of health.

Now with regard to the second point—the relief

of pain and spasm. In severe cases of trigeminal neuralgia, intercostal neuralgia, brachial neuralgia, and other forms of pain, electricity has been found of great service. Galvanism is the form to be used, the anode or positive pole being applied over the painful area, and the other electrode being put at the back of the neck. If the pain is severe, the patient should be provided with a battery of his own, and allowed to apply the electricity when the pain comes on. You will readily see that if he has paroxysms occurring every half-hour there is not much benefit to be obtained by applying the electricity only night and morning, especially if those times happen to synchronise with the intervals between the pains. The electricity should be applied when the pain comes on, even though it may be necessary to do it twenty times a day.

There are two conditions in which faradism should be avoided, namely, in spasm, such as facial or histrionic spasm; and in all conditions of rigidity and spasticity, such as are seen in hemiplegia and after myelitis. Faradism in such cases only increases the rigidity. The same remark holds good in lateral sclerosis in which there is a tendency to rigidity of the extensors and adductors; use galvanism in such cases, for we know that galvanism tends to reduce spasm.

The third use of electricity in treatment is to increase the general nutrition of the neuro-muscular systems. For this purpose it should be combined with massage. For instance, in neurasthenia faradisation in the ordinary way is not of much service, but there is a method of applying faradism which is of service, namely, the faradic bath; that is to say, the water of the bath becomes one of the electrodes, the electricity affecting the whole cutaneous surface. It is soothing and at the same time invigorating to the patient. But in all conditions of this nature massage should be carried out as well.

Another method of increasing the general nutritive condition of the body is by galvano-faradism, which has been previously mentioned. It is applied to the general skin surface; but this, again, should be combined with massage.

Finally, there are two practical points which should be mentioned regarding the application of electricity:

First, always test the strength of the current on

yourself. If you can bear the current, the chances are that your patient can bear it also.

The next point is to use the weakest current which will produce the required effect, whether the current you are using be the faradic or the galvanic.

V. NIESEN (Wiesbaden) has demonstrated the germ of syphilis discovered by him. He was dealing with cocci, which, in the preparations shown, were ranged in the form of chain links, and were very prettily stained with carbo-fuchsin, according to Gram. It may be mentioned that v. Niesen discovered this syphilococcus in pure cultures, in the bone marrow of children suffering with hereditary syphilis. Pure cultures of his cocci were injected into pigs and rabbits, and indurated plaques were formed at the point of injection. He then allowed these rabbits to pair, and out of ten young ones which resulted, three were altogether rotten, and showed all the symptoms of hereditary syphilis. In a paper by Moritz Mayer (Simmern): "Chemical Suppuration, as applied to the combating of Infectious Suppuration and Local Tuberculous Processes," Mayer had obtained very favourable cures in actinomycosis and in various tubercular processes; in cold abscess; in glandular suppuration; in fungous processes of the bones and joints; in fungous ulcerations, from the application by means of tampons of sterilised diluted pyogenic remedies to a large surface; in artificially made clean wounds of the tissues, as well as on abscess-membranes. Among these pyogenic remedies may be mentioned compound tincture of benzoine, balsam of copaiba, and especially balsam of Peru, and oil of turpentine. Mayer explained the manner of healing, which was elucidated by photographic exhibits, by assuming that a chemical suppuration was produced by these drugs, which successfully combated the infectious suppurative and local tuberculous processes. To this chemical suppuration Mayer ascribed the successful internal use of the balsams, and their use by inhalations.

Der Kinderarzt. Pediatrics, January, 1899.

Neuralgia of the Trigemini.—J. Hirsch-kron recommends three cachets a day of the following:—Extract of cannabis indica 50 gm., salicylic acid 5 gms. For ten cachets.

Semaine Méd., Dec. 14th.

MEETING OF THE SOCIETY OF ANÆSTHETISTS,

At 20 Hanover Square, January 20th, 1899:

The President, Dr. DUDLEY BUXTON,
in the Chair.

Resumed Discussion on "The Choice of an Anæsthetic."

(Continued from p. 256.)

SIR FELIX SEMON said if he ventured to take part in this discussion, it was first of all on account of the President's kind invitation, and because he found in the discussion at the Society's last meeting an observation by Dr. Silk, in which he said he imagined that the great object of this discussion was not the question of the general utility of chloroform or ether, but to help the Society to arrive at some conclusions as to whether there were any conditions which might determine the use of one anæsthetic in preference to the other under certain circumstances. He would take the liberty of making a few observations with regard to that point, but only with regard to those operations with which he was familiar, namely, on the upper air-passages more particularly: first, with regard to operations for chronic stenosis of the upper air-passages, particularly in the larynx; and secondly, concerning adenoid vegetations.

With regard to chronic stenosis in the upper air-passages, he thought there was a tolerable amount of agreement amongst surgeons as well as amongst anæsthetists that chloroform was, *par excellence*, the anæsthetic to be used. He was very glad to hear this confirmed by Mr. Haward. He thought ether under such circumstances should never be used, for the reason that it produced irritation of the respiratory mucous membrane. It was very well known that the great danger of these operations was not so much the danger at the time of the operation itself as of pulmonary complications afterwards. The nature of these pulmonary complications was twofold; they might be either septic, or they might be the production or aggravation of a pre-existing catarrhal process in the lower air-passages. With regard to the occurrence of septic complications, of course there

could be no question as to the selection of an anæsthetic. Neither one nor the other would guard against that; but with regard to the aggravation or even production of fresh catarrhal complications leading to serious respiratory and pulmonary or bronchitic troubles there was an almost universal conviction amongst them that chloroform was the proper drug to avoid that. He was only once in his life induced to use ether in a case of this sort, and although he had in some other connection communicated the case, he thought it was so interesting with regard to the discussion now proceeding that he might be allowed to briefly communicate it to the Society. It was the case of a gentleman, æt. about fifty-eight, who suffered from malignant disease of the larynx, and it was obvious that only a partial extirpation of the larynx could cure this condition. The patient consented to the operation, and only made one stipulation. He had been told that he had a weak heart, and he had been warned against ever allowing the administration of chloroform. He (Sir Felix Semon) very carefully examined the patient, but could not find any signs of valvular weakness or degeneration of the heart, but as the patient insisted on this point ether was administered. Unfortunately he was persuaded, not having any personal experience on that topic, to allow administration of ether by the rectum. It was administered by a well-known first-rate anæsthetist. It took more than half an hour to get the patient under its influence sufficiently to enable the operation to be begun, and it had to be repeatedly interrupted completely, because the patient showed signs of returning consciousness. A very large quantity had to be given; and although the operation from a surgical point of view offered no complication, it took one and a half hours to perform. Finally, the patient looked very white; his pulse was very irregular and unsatisfactory. During the last part of the operation something occurred which he had never seen previously or since; the patient coughed almost incessantly, and brought up not ordinary bronchial secretion and small quantities of coagulated blood, such as were occasionally seen when there had been some entry of blood into the lower air-passages, but what he expectorated was simply a thin watery blood-stained fluid looking like diluted blood, and this continued uninterruptedly after

the operation. Additionally, a few hours after the completion of the operation he was taken with very violent and never-ceasing diarrhoea, and the fluid which was evacuated by the rectum looked exactly like that from the bronchi, so that from the mere appearance the two fluids could not be distinguished; the only difference was that the intestinal fluid had a very horrible smell. The patient got more and more drowsy, his temperature began to rise, the drowsiness deepened into coma, and he died twenty-four hours after the operation, the temperature at the time of death being 107° .

Sir Felix Semon then read the post-mortem report, which was to the effect that the whole of the duodenum and to a minor extent the other parts of the intestinal tract were affected by hæmorrhage—in part submucous, in part on the surface. What if possible was more important than that, however, was that the bronchi were in exactly the same condition as the duodenum, and that the hæmorrhagic tendency extended into even the very smallest bronchi. There could, therefore, be not the slightest doubt but that the patient died from the ill effects of ether.

Of course it might be urged against this case that it was an extreme one, and that the event was not likely to be repeated. He trusted not, but he thought that what was shown here in its extremest degree should be a warning with regard to the probability of irritation being actually produced by ether in the larger air-passages, even if administered, as it was in this case, very skilfully. Patients should be warned against having ether in any case in which a pre-existing bronchial affection was likely to be aggravated in the performance of one of these operations for stenosis of the upper air-passages.

With regard to operations for removal of adenoid vegetations, one felt almost that this subject had been thrashed out so frequently that one hardly dared to return to it; yet it seemed necessary in view of a paper which was read last year before the American Laryngological Society by Dr. Hinkel, in which the author arrived at the sweeping conclusion that in view of the statistical and pathological data, the general use of chloroform was inadmissible for removal of naso-pharyngeal adenoids and of hypertrophied tonsils.

Dr. Hinkel based his conclusion on a collection of cases in which fatal results followed the opera-

tions for adenoids performed under chloroform. He brought forward eighteen cases in which this lamentable result had followed. Of these eighteen cases, he said that in four of them death occurred before the operation was begun; in three from a few moments to an hour after the operation was completed. The proportion of deaths from chloroform in all kinds of operations had been placed at 1 in 4000.

If there was no special susceptibility in the subjects of adenoid operations, this death-roll of eighteen would imply that 72,000 of these operations had been performed under chloroform since 1892—a preposterous conclusion. When it was remembered that a large majority of the reported deaths had occurred in England alone this conclusion was still more untenable; and the inference was plain that the risks from chloroform anæsthesia in these cases were for some reason particularly great. We were fully justified in inferring also that not all deaths under chloroform in this operation had been reported, and that the proportion of fatalities from this cause was greater than the statistics showed.

Granting this last sentence, the question might be well asked, Which was the particular reason why in these cases the mortality from chloroform should be so great? as of the occurrence of these eighteen deaths there could unfortunately be no doubt. Dr. Hinkel in his paper traced the cause to a hypothetical "*habitus lymphaticus*," to a tendency to degeneration of the heart, to spasm of the glottis, and so on. But it appeared to him (Sir Felix) that he had not detected the *real* reason, although another paragraph in his paper seemed to point to it very forcibly. Dr. Hinkel had stated that in 1893 several brief communications appeared in the medical press of Great Britain calling attention to the large mortality attending these operations, but these communications had received surprisingly little notice. In 1896 Dr. W. G. Holloway, Registrar of the Central London Throat and Ear Hospital, had tabulated fourteen deaths under chloroform in nose and throat operations that had been reported in England up to April, 1895.

Of these fourteen deaths under chloroform, eleven were in operations on the tonsils and naso-pharyngeal adenoids reported since 1892. He (Sir Felix) wished to draw the attention of the Society particularly to this last sentence. Here

were within three and a quarter years—1892 to April, 1895—eleven cases of death in consequence of operations for adenoids performed under chloroform, and they all had occurred in this country alone. Now surely it would be a very extraordinary thing if the "habitus lymphaticus" or the tendency to degeneration of the heart should all of a sudden have made its appearance in an almost epidemic form, and that it additionally should have selected England only for its appearance. It was not a very pleasant thing perhaps to give what he (Sir Felix) considered as the true reason of this excessive mortality. He would be corrected if he was wrong, but his opinion was that the reason of this increased mortality was that all of a sudden, after the operation had been limited for many years to the hands of the comparatively few, the operation became fashionable,—anybody thought he could not only perform it, but that he could administer the anæsthetic. He did not wish to offend anybody, but he had at the time followed the cases out with a very great deal of interest, though he was not prepared to give absolutely accurate numbers this very moment. So far, however, as his memory served him, only one of these fatal cases had happened in the hands of a good and well-known operator, and in this solitary case it was not a skilled anæsthetist who gave the chloroform. As far as his knowledge went, not one of all the fatal cases has occurred in the hands of a *skilled anæsthetist*, and where the operator was a skilled operator. One witnessed in those days what one knew had so often obtained in medicine; an operation was at first pooh-poohed, and was said to have been invented for the special benefit of a few specialists. When, however, the results of these operations, as performed in suitable cases, became gradually known, and could no longer be denied or ridiculed, the operation fell from one extreme into the other; from having been done very little or not at all it came to be performed with such frequency that not only were cases operated on where the operation was needed, but also where it was not! And, as he had said, everybody commenced to operate upon and to give an anæsthetic in these cases. And then all of a sudden came this terrible mortality, which since the note of alarm had been sounded had receded very much. Whilst there had been eleven fatal cases in Great Britain alone

between 1892 and April, 1895—*i.e.* within 2½ years,—since then two fatal cases only had been reported according to Dr. Hinkel's own statistics from Great Britain between April, 1895, and the present time, *i.e.* within 3½ years, in operations under chloroform for the removal of adenoids. With regard to the selection of an anæsthetic, he (Sir Felix) had enjoyed the co-operation in his own cases of one of their foremost anæsthetists, and he was convinced that in these cases if chloroform was quietly and slowly administered, and never beyond the point at which the reflex irritability of the conjunctiva ceased, and if the patient was in that position which he considered was physiological and really the only proper one, namely, with the head hanging over the table, there was no particular danger in that operation at all. But of course all these demands ought to be fulfilled if one wished to perform the operation under chloroform. And if he were asked why not select the safer anæsthetic in the first place, the objection he had mentioned, namely, the irritation of the respiratory mucous membrane, held good. But there was another reason. They heard from Mr. Davis that evening of operations for adenoids which occupied a few seconds, or perhaps half a minute, to perform. His (Sir Felix's) own operations took what Mr. Davis would probably consider an unconscionable long time, namely, five minutes, and often a good deal longer than that. The reason he selected the anæsthetic which gave him the longest anæsthesia and the better quiet was that he thought there could be hardly anybody in the room who, if nowadays he told a mother or father about removal of adenoids, was not immediately met with the objection, "But do not they always return?" The belief in the return of this hyperplasia seemed to be a general one amongst the public; no one could deny that there were genuine recurrences, and that such recurrences might happen in the hands of the most careful and thorough operator. But these "genuine" recurrences were extremely rare as far as his experience went, and what was usually called "recurrence" represented as a rule, he thought, *continuity of growth owing to incomplete operation!* Such incompleteness in turn was a consequence of the operation being made by some a sort of exhibition performance! He was present not long since at a discussion when there was some sort of sweepstakes as to who could

perform this operation in the minimum number of seconds, and it seemed to be thought a very desirable performance if it could be done in forty-five seconds. He disagreed with that entirely. Their duty was to perform the operation as *thoroughly* as possible, so as to reduce the chances of recurrence to a minimum. For that reason he would, while he had the help of a skilled anæsthetist, use chloroform in preference to any other anæsthetic in these operations.

Dr. G. H. SAVAGE said as an invited guest he might perhaps be allowed to make a few remarks upon the relationship of the selection of an anæsthetic to cases of mental disorder. When first asked to come to the meeting he could hardly see in what possible relationship he could speak on the subject; but on thinking over the matter he found there were many points of common interest. However, as he had received a courteous request to read a paper on some of the relationships between anæsthetics and insanity, he would not trench far upon what he might have to say on that occasion. He would only now say that in his experience the insane, as a majority, took either the one or the other very readily; that there were comparatively few difficulties with operations performed on the insane under the influence of either ether or chloroform. He was afraid he could not say that he knew chloroform to be better than ether, or that ether was better than chloroform. But there was another side of the question altogether, namely, the effect of chloroform or ether on people who were insane as a curative measure. In some cases one had known people suffering from the most violent mania kept under chloroform for many hours. He could not speak so clearly of the influence of ether in these cases. Nearly all the cases he referred to had been those of extreme violence, with destructive tendencies, where the patient had been in some private house, or where it had been impossible, for social, legal, or other reason, to move him at once; and it was felt that he must be kept as quiet as possible;—such, for instance, as where there was a parturient woman in the house. He had seen an insane patient kept for hours under an anæsthetic, and without any injurious effect. On the other hand, one would have to point out that there were certain risks, and he would say that the majority of cases of mental disorder that he had had the

opportunity of seeing depending upon the administration of an anæsthetic had followed the administration of chloroform rather than of ether or gas; but, as he would point out on a later occasion, he had not found any one of them to be free from possible danger. To sum up, he would say that the insane, whether they were suffering from maniacal excitement or from depression—whether they were suffering from organic disease, such as general paralysis of the insane, they would take either chloroform or ether, or they would take gas without really suffering in any way; that, in fact, they did not suffer more than ordinary persons. But, on the other hand, one had to remember that certain unstable people might be disturbed by one or the other of these anæsthetics; some unstable people had been upset by one, and others had been upset by another.

Dr. MACNAUGHTON-JONES said he found himself taking part in the discussion through the courtesy of an invitation the President had given him to join in it, an invitation founded, he doubted not, on an association extending over a number of years, during which he (Dr. Macnaughton-Jones) had been many times deeply indebted to the President for his patience and great skill in administration, his resource in emergency, and tact with patients. The action, he thought, and, to a certain extent, the after effects of any anæsthetic depended upon the mode of administration. Volumes had been written on the relative dangers of anæsthetics, comparatively little on the necessity for careful discrimination in the selection of the anæsthetic, and the skill demanded in administering it. Every one, for example, considered himself a capable chloroformist. Certainly nothing could be more simple than holding a piece of lint over the face, or a cone over the nose and mouth, and that most dangerous expression "just a whiff of chloroform" is one of the commonest we hear. It is just that "whiff" that kills, when there is reflex shock or sudden severe pain inflicted. Witness death from chloroform in dental operations. To his mind, skill in administering chloroform depended upon the ability to recognise the signs of impending danger and in meeting and dealing with these promptly.

He thought that the reasons why he considered administration by any form of cone dangerous were too obvious to need recapitulation. Junker's

apparatus, or the ordinary small mouth and nose flannel mask, and the modern drop-bottle, were cheap enough for every practitioner to be possessed of. On the other hand, there was not the same familiarity with ether. It required a more expensive appliance, more skill and experience in its use so as to produce rapid and satisfactory anæsthesia. He would go so far as to say that in 90 per cent. of cases in which the practitioner required to administer an anæsthetic, ether was the one which should be chosen on the grounds of safety and rapidity of action. He was speaking in regard to its proper administration with a Clover's apparatus. Chloroform given under certain conditions might be dangerous in the best hands, whereas a man had to go a little out of his way in order to succeed in killing a patient with ether. He had little faith, save for the sake of appearance, in the placing of the stethoscope for a second or two over the heart before administration, while the lungs, the most important organs, were overlooked. The temperature of the room, a most important factor, especially in chest cases, was often neglected. The danger of talking while the anæsthetic was being administered went without saying. The only death from an anæsthetic which he had ever seen arose from the distraction of a house surgeon who was spoken to while administering chloroform.

As a surgeon, he might say give him an absolutely fearless, but not reckless, anæsthetist. It gave him that confidence which is insured by deep anæsthesia during operation, also absence of movement and freedom from reflex shock. Surgeons were apt to attribute collapse to anæsthesia. The anæsthetist in nine cases out of ten might with perfect truth affirm that collapse, both during and after operation, was purely surgical, caused by the performance of an operation which the vital powers of the patient could not sustain, or by some rashness or bungling during its performance, thus demanding increased length of administration. "She never recovered from the anæsthetic" might be read as "she never survived the hæmorrhage" or (through an imperfectly secured vessel) as "she bled to death."

As to the age, broadly speaking he agreed with what had been said by previous speakers. Though he believed that ether could be given at all ages, he preferred chloroform for very young children.

Not but that he recognised its special dangers, which could not be too forcibly insisted on, of the suddenness of the occurrence of its full physiological effect. There was the disadvantage of having to stop and of an interruption to the operation. Much depended, however, on the administration; they should avoid the fright of the cap and the struggling with the ether. In people of advanced life with high tension and atheromatous conditions and in old alcoholics, chloroform, he thought, was the best anæsthetic to use. He preferred chloroform in hysterical or nervous patients, at least, chloroform succeeded by ether or by a mixture.

He was not oblivious of the fact that on the Continent, in all gynæcological operations, chloroform or some compound mixture with chloroform was, he might say universally, used. An anæsthetic mixture of chloroform two parts, petroleum ether one part, sulphuric ether one part, he had seen freely administered in a large number of abdominal sections this year in Professor Schauta's klinik in Vienna; it was given freely poured on the usual cap from a drop bottle. It appeared to him to be an undesirable anæsthetic.

In all cases of organic lung mischief he should prefer chloroform, as also in cases of insane people or those who had shown symptoms of alienation. The only two cases of insanity which he had seen arose after the administration of ether for eye operations. There had been no history of previous mental aberration. Both these cases had to be removed from hospital under certificates, and both recovered.

In diabetes, on chemico-physiological grounds he would prefer ether; on the pathological, if there were arterial changes and nutritive degenerations, decidedly chloroform. In the case of a woman of advanced age, on whom he operated for mammary cancer on the day after her admission to a home (the fact that diabetes existed having been purposely concealed from him), though the aseptic wound healed perfectly, the elimination of sugar rapidly increased, and the patient died within a week of diabetic coma. Ether was the anæsthetic given.

In ophthalmic surgery, at a time when he had daily operations in hospital or otherwise, his rule from the time he discarded methylene was to use ether in all cases requiring an anæsthetic, save under those other exceptional conditions which he

had already mentioned. There was one other exception, and that was in all operations in which glaucomatous states existed. Cocaine he had then no knowledge of, but from his experience of it since it came into use his feeling was that when an anæsthetic was necessary in ophthalmic surgery, general anæsthesia was to be preferred. He would pass over the surgery of the mouth, because others more competent in that matter had expressed their opinions; but he would say that, from his use of chloroform for most of the gravest operations, he considered it to be *par excellence* the best anæsthetic for such. There was a time when he had often done palate operations, always under chloroform, the head of the child being raised and partly thrown back and supported by a nurse.

Now as to gynecological surgery. His rule was to leave himself in the hands of the anæsthetist. He never expressed a wish, and he found that they all gave gas and ether, judiciously supplemented by oxygen as occasion demanded. When he said that this afforded him full satisfaction, he referred to the general absence of such unpleasant complications as vomiting, abdominal movements, and collapse. Sometimes a patient would beg not to be given ether, having had some previous unpleasant experience of it. Then (which was the rarest occurrence) he would ask the anæsthetist as a favour to administer chloroform. In an abdominal operation likely to be considerably prolonged, say from inflammatory conditions and adhesions, he would certainly prefer ether to chloroform. Hysteria and a neurotic temperament would, as he had said, make him lean to chloroform, and possibly other general considerations might occasionally come in, influencing him to venture a suggestion to an anæsthetist. His strong feeling, however, was that the anæsthetist, having once had placed before him any exceptional conditions in a case, and knowing the nature and probabilities of an operation, was best left to his own discretion.

With regard to tonsils and adenoids. For removal of the former he always preferred gas, or gas and ether, when any anæsthetic was necessary, which in a large proportion of cases ought not to be. As to adenoids, perhaps there is no operation in surgery to which the old saying more applies—that attitude is everything—than to this. It was to his mind most fallacious and dangerous to talk of the absolute safety of adenoid operations.

There had been deaths from adenoids in the best hands, and possibly more than we are aware of. He thought that there was but one position for the head to be in during the operation, and that was well thrown back, and here again he would not dream of making a suggestion to a skilled anæsthetist, though if he asked him for his choice he should say gas and ether. In turbinotomy and nasal operations gas and ether had been invariably given for him. He was doubtful whether they did not get more bleeding after ether than with chloroform, and seeing that many times such operations had to be completed with chloroform given by the mouth, he was not so sure that it might not be as well to commence with it. But here again he much preferred to leave himself in the hands of the anæsthetist. In minor nasal operations cocaine was most useful, as in some cases of polypus, for cauterisation or partial turbinotomy. But for the great majority general anæsthesia was required to do an operation well. Not long since he had operated on a lady of sixty-eight, with a large aortic aneurysm under ether.

In rectal operations he had not for over twenty years had any anæsthetic administered save ether, and he had never had a death. He certainly considered that in some women blanched from repeated bleeding, chloroform would be a dangerous anæsthetic as compared with ether.

Finally, a word on obstetric surgery. Obstetric tolerance of chloroform was a commonly used expression, and physiologically they understood how such tolerance during parturition was explained; he had seen reliance on such immunity bring the reckless or over-confident administrator, or inexperienced nurse, with minds abstracted in watching the labour, to the brink of a precipice over which mother and child were both very near falling. Analgesia, not anæsthesia, was what should be aimed at during labour. The borderland, save in cases of operations, should not be crossed. Not one, but two lives, were in their hands. Some time since he delivered a highly neurotic patient who had been operated upon for laceration of the cervix some years previously with a consequent cicatricial os uteri. She suffered from severe attacks of angina pectoris, constantly inhaling nitrite of amyl and having morphia injections. Her heart-sounds were extremely feeble. She would have chloroform, which was given from a Junker. The result was

so alarming that he had to desist. Ether was then administered by Mr. Bailey, and he, Mr. Macnaughton-Jones, safely delivered her under that. The abolition of reflex action, the safety of the child, and post-partum hæmorrhage, were the points for the obstetric anæsthetist to remember. He was sure that ether would be the best anæsthetic to select for certain cases when deep anæsthesia was called for.

(To be continued.)

A STUDY OF CEREBRO-SPINAL MENINGITIS.

DR. L. A. CONNER lately read a paper based on sixty cases. Every effort had been made to exclude those in which the meningitis was secondary to other processes. The youngest patient was a child of two months, and the oldest a woman of sixty-five years. In the very acute cases there was marked congestion of the meningeal vessels. The exudate was often so slight as to be scarcely perceptible, except in cases that had lasted two weeks or more. The lateral ventricles were often distended with purulent fluid. The greatest involvement of the spinal cord was in the dorsal and lumbar portions, and always on the posterior surface. Microscopical examination showed constant involvement of the subjacent brain tissue. The common involvement of the cranial nerves was also noted. Councilman and his associates were convinced that the specific cause of epidemic cerebro-spinal meningitis was the *Diplococcus intra-cellularis*. In the thirty-five cases examined by them the organism was found in all but four, almost exclusively in the pus cells. It was an aerobic bacterium, which grew best on blood-serum agar. All the usual types described were represented in this series. All the fulminant cases were marked by early delirium and coma. There were two of the intermittent type. Thirteen lasted more than a month, and in these cases eleven patients recovered. The longest course was three months and a half. The onset was sudden in thirty-nine cases. The temperature was decidedly irregular, but there were several distinct types. In a number the temperature was normal or subnormal throughout the disease or up to a short time before death. In a second type the temperature was moderate most of the time. In

others the temperature rose gradually and then fell, whether or not recovery took place. These cases resembled typhoid fever somewhat, but not infrequently the highest temperature was in the morning. In the fulminant cases the temperature was sometimes nearly normal for the first few hours. The pulse also varied greatly independently of the variations in temperature. Where the symptoms were active an almost constant symptom was a marked increase in the respiration rate. Vomiting was present at some time in over half; in twenty-five it appeared at the onset, and in fifteen only at that time. In fifteen it remained a prominent feature throughout the disease. In no case was the vomiting "projectile." There was more or less pain in all cases, and it was usually in the form of an intense occipital headache; but there was not infrequently pain in the back, limbs, and chest, sometimes especially marked on one side. The reflexes presented no constant feature. More or less rigidity of the muscles of the head, neck, and back was present in almost every case. Rigidity of all the extremities was also common. There was no uniformity about the appearance of the pupils. In a large proportion delirium and stupor alternated. Some of them were marked by an unusual form of delirium simulating hysteria. Skin manifestations were observed in 25 per cent., but the hæmorrhagic eruption was seen in only three of the cases. A distinct leucocytosis was present in most of the cases in which the blood was examined. The spleen was enlarged in 23 per cent. In two there was bilateral deafness. In several there were involvements of the joints resembling those seen in rheumatism. In seven pneumonia occurred, and in all of them it developed after the meningitis. Albuminuria was generally an accompaniment of the disease.

N. Y. Med. Journ., Jan. 21st, 1899.

Local Applications of Castor Oil.—Beloll asserts that warm castor oil thoroughly applied to the abdomen is often as effective in the case of a child as if taken internally.

N. Y. Med. Journ., Oct. 22nd, 1898.

Heat in Treatment of Syphilis.—Storoscheff reports the favourable results of local warm baths, compresses, &c., in the treatment of syphilis, according to the experience of Ussass, Kaleschni-koff, and Stepanoff.

Derm. Cbl., December, 1898.

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THE INFLUENCE OF VARICOCELE ON THE SEXUAL POWER.

BY

CAMPBELL WILLIAMS, F.R.C.S.

MATTERS relating to the sexual aspect of our being do not lend themselves palatably to medical discussion, and for the same reason are either omitted, or so briefly touched upon in text-books, that often one must glean by experience and clinical observation what one is unable to learn by other methods. The influence of varicocele on the sexual power falls within this category, and perchance for that cause it has received but brief mention. Undoubtedly in some subjects it produces a very distressing physical and mental state. Since the bearing of a varicocele to the train of symptoms for which they often seek relief, and which they attribute to other causes, is not always recognised, I venture to dilate upon the subject. It is generally admitted that varicocele usually involves the veins of left pampiniform plexus, and although the right ones may be similarly affected they are as a rule but slightly implicated. A large right-sided varicocele with practically normal left veins led me to discover that the patient was the subject of "transposed viscera," which accounted for the abnormality. A certain number of patients who suffer from a varicose pampiniform plexus seek surgical advice on account of the *local* symptoms that arise from its presence, such as a sense of testicular weight, pain, neuralgia, or hyperæsthesia. Others invoke the surgeon's aid to rid them of a condition which proves a bar to their entrance into certain branches of the public service. Amongst the latter class there are many who were unaware of the existence of anything abnormal in their venous condition until they had been refused by the examining medical officer "for varicocele" in that it had not given rise to any *scrotal* symptoms to attract their attention to its presence. But in both of the above classes one

frequently meets with what may be termed "a debilitated sexual power," which is seldom recognised as being intimately connected with the state of the varicose spermatic veins. It may be that a varicocele is noted for the first time when a surgeon is consulted for purely sexual symptoms, which are really dependent upon the pathological condition of the intra-scrotal veins.

The *practical point* in connection with this is that the quickest and most efficient method of relieving the *sexual symptoms* for which they seek relief consists in direct attention to the varicocele rather than in the prescribing of nerve stimulatory drugs for the genital debility. The usual history recited by a varicocele patient with subnormal sexual power is that the procreative act is either a prolonged, exhausting, and perchance unsuccessful effort on the one hand, or that the orgasm is almost synchronous with contact on the other. For these diametrically opposed states one must seek a causation, and whilst the latter condition seems to be due to prostatic derangement, the former must be attributed to a faulty testicular nourishment. For although one would imagine that physiologically the erotic functions were contributed to in equal proportions by the right and left testicle, and therefore that the activity of the right one would be independent and uninfluenced by the condition of the left,—for we know that a monorchid may have sexual passions and be fertile,—nevertheless it would appear that the virility of the apparently healthy right testicle co-ordinates itself to that of its impaired fellow-organ, and that the possessor does not receive from it that half-share of vigour that one would expect.

In both of the above-mentioned categories there are certain symptoms which they complain of in common, namely, a copious emission of thin seminal fluid at the orgasm, a feeling of exhaustion or even prostration after ejaculation, and inability to repeat the act until some considerable period has subsequently elapsed. There is a marked tendency to nocturnal emissions, which have a very depressing effect upon their spirits. The virility of the procreative power is not in keeping with the subject's years, notwithstanding that he is muscularly well developed and otherwise apparently healthy. Marital duties may become a source of dread to the sufferer, in that the act of coitus, through the rapidity of the climax, may not

allow time for penetration; or that the exhaustion that ensues, together with the pronounced feeling of incapacity to *complete* or *repeat* the function, breeds a fear of impending *total* impotency, and renders him hypochondriacal. Auto-suggestion is undoubtedly a marked adjunctive feature in those cases where "delay" is complained of, but a state of imperfect testicular circulation, and consequent impaired nutrition of the organ, afford the most probable elucidation of this inhibitory phenomenon. For the abnormally "quick dissolution" one must inquire into the prostatic state of the patient, and a direct bearing between the congestive or hyperæmic state of this gland can be connected with the intra-scrotal condition.

A consideration of the anatomical communication between the *deferential* veins and the *prostatic venous plexus* will show that a retardation of the heartward flow of blood through the pampiniform plexus may react upon the prostate gland in that it tends to create a state of testicular stasis. From this hæmal gravitation there results an imperfect oxygenation of the tissue of the orchis, which is consequently not receiving its full amount of nourishment. For although the organ contains more blood than normal its interstitial tissues do not receive their full complement of oxygen, owing to the impeded circulation. Years ago W. G. Spencer pointed out that the veins of the spermatic cord consisted of an anterior or *spermatic* set, and a posterior or *deferential* group, and that it was by the latter channel that the blood returned from the testicle subsequent to ligation and excision of a portion of the pampiniform plexus. After an operation for varicocele, such as advocated by W. H. Bennett, the return of testicular venous blood must necessarily be limited to the deferential route, and it may be said how can the return of this blood be assisted or improved by excision and obliteration of the pampiniform channel, when the arterial supply from the spermatic, deferential, and cremasteric arteries is unaltered, and the carrying capacity of the deferential veins remains the same? One must remember that by ligation of the anterior veins the weight of the column of venous blood that formerly extended from the left renal vein downwards to the testicle is interrupted by the upper ligature on the pampiniform plexus, and that a hæmostasis of the testicle is thereby relieved. Moreover the enfeebled cremaster muscle is

assisted in its endeavours to support the dependent organ in that the weight of the testicle is taken off by knotting together the upper and lower silk ligatures on the divided veins. This also relieves a more or less continuous strain from the *deferential veins* and also from the *vas deferens* itself, which, extending as it does as the continuation upwards of the convoluted canal of the epididymis to the hinder surface of the bladder for junction with the seminal vesicles, and thence passing forward to the base of the prostate, must exert a dragging and irritative effect upon that gland, and likewise upon the seminal reservoir. Though a varicocele may certainly influence the activity of the sexual power through a decreased oxygenation of the testicular substance, it causes no further mischief in the organ. I have never seen nor do I know anyone who has met with a case of atrophy of the testicle from this cause, and I therefore doubt the accuracy of this text-book statement. From what has already been said it will be gleaned that the relief of this state of "debilitated sexual power" rests in the *treatment of the varicose pampiniform plexus*. Treatment by means of a suspensory bag, whilst it may succeed in relieving the patient to a certain degree, does not have the same permanent result that follows an excision of a portion of the veins. It may alleviate a prostatic gleet which is so frequently met with in the subjects of varicocele, but it does not effect a renovation of the sexual functions in the same way the operation does. The following fact has been communicated to me by patients spontaneously. They have noted how much their virile powers have increased after an excision of the varicose veins, and how they had been enabled to fulfil marital duties in a manner that formerly was impossible. In the case of a large varicocele with sexual debility in which palliative means had failed to relieve other than the symptoms of weight and pain, I operated in the hope that excision might, whilst curing the former, benefit the generative state as it had done in other instances. The result was that the patient was sexually rehabilitated. One feels a certain amount of trepidation in advocating an operation for a sexual condition alone, for fear that it might descend to the domains of charlatanism, and perchance be needlessly foisted upon a patient whose powers of imagination or fear had been unscrupulously worked upon. But at the same

time I am convinced by experience, observation, and *facts* that it does restore the sexual power when all other means have failed, and that in certain cases it is the best course to pursue in the interest of the sufferer. The operation that I prefer is that known as "Bennett's modification." This consists of exposing the pampiniform plexus through a vertical incision, measuring about one and a half inches, at the upper part of a previously shaved and surgically cleansed scrotum. The *vas deferens* and its accompanying veins are held out of the reach of injury, and the testicle fixed by being drawn downwards by an assistant. The pampiniform veins together with the immediately surrounding fascia are separated *en masse*, and such a length of varicose veins as the nature of the case demands is excised between an upper and a lower ligature. The stumps of the divided veins are approximated and fixed by firmly knotting together the free ends of the silk ligatures which are then cut off close to the knot. Sometimes, to get even greater apposition of the raw surfaces, I sew together the stumps of the cut and ligated veins by means of fine silk thread. The external wound is closed with a few sterilised horsehair sutures, and dressed with mercurio-zinc cyanide gauze. For fear of the dressing shifting through movements of the patient it is well to fix the immediate layer or two by means of collodion applied to the edge of the gauze. More gauze and wool, applied so as to support the testicle, and kept in position by a double spica bandage, complete the dressing. This is kept from being wetted by urine by means of a square of jaconette (through which a hole is cut for the penis to be drawn), and fixed above and below to the bandage by safety pins. The dressing is left undisturbed until the seventh or eighth day, by which time the wound should have healed by first intention, when the horsehair stitches are taken out and the part re-dressed. After the twelfth day following the operation the patient may be moved on to a couch, and it is safe as regards embolism to allow him to walk and resume his normal life after the eighteenth day. It is well to employ a suspender for six weeks after this, in order to support the parts, when it may be discarded.

There may be retention of urine after the operation, or if the testicle or *vas deferens* have been roughly handled orchitis or epididymitis may

supervene. But this latter complication may sometimes occur when the greatest care against its development has been taken. The patient usually feels the testicle to be tender for a few days following the operation.

The Intra-cerebral Injection of Tetanus Antitoxin.—The daring procedure first successfully practised by Quenu and Chauffard, has been performed by Dr. Rambaud, of the New York Pasteur Institute, in the case of a patient in Passaic, N. J., who developed tetanus after a lacerated wound of the calf of the leg due to falling through a skylight. A large portion of the calf of one leg was torn completely away. "Antitoxic serum was injected hypodermically, but without effect. As a final effort it was decided to resort to the heroic measure of trephining the skull on both sides and injecting the antitoxin directly into the anterior lobes of the brain tissue. This was accordingly done, with the result that almost immediately there was a relaxation of the muscles of the jaw. Since then improvement has been steady and constant." Dr. Rambaud was present at the first successful operation performed by Quenu and Chauffard, in which Roux injected the antitoxin. The period of incubation of the disease is not stated. It is to be hoped that a fuller report of the case, which is one of great interest and importance, will be forthcoming.

Du Hamel ('La Médecine Moderne,' August 16th, 1898) also reports a case of tetanus occurring in a boy aged fifteen, produced by a pistol-shot wound between the thumb and index finger. Later symptoms of tetanus developed, and he was taken to the Pasteur Institute, where, in addition to hypodermic injections of antitetanic serum, three cubic centimetres of a similar fluid was injected in each side of the brain near the fissure of Rolando. At the time the injections were made the patient was in a condition of strong opisthotonos. The contractions gradually diminished, though they did not entirely disappear for some days. In three weeks he was out of the house.—*Boston Medical and Surgical Journal*, October 27th, 1898; *Therapeutic Gazette*.

TWO CLINICAL LECTURES ON SOME PRACTICAL POINTS IN THE DIAGNOSIS AND TREATMENT OF SKIN DISEASES.

LECTURE II.

Delivered at the West London Hospital, Hammersmith,
January 26th, 1899,

By PHINEAS S. ABRAHAM, M.A., M.D.,
B.Sc., F.R.C.S.I., &c.,

Dermatologist to the Hospital, and Assistant Surgeon to
the Hospital for Diseases of the Skin, Blackfriars.*

GENTLEMEN,—I will first of all show you the girl you saw on Monday with vesicating erythema. You will notice that the condition is now decidedly worse, and illustrates the degree of vesication you may get in erythema, which no doubt was factitiously caused in the first instance by applying an irritating ointment to a patch of eczema.

The next patient, also a girl, shows a patch of eczema which she has had on the hand for a long time. No doubt it was irritated through her occupation, that of a housemaid. A day or two ago she developed erythema on the face and eyelids, which have been considerably swollen. It is important to remember that this condition is sometimes mistaken for erysipelas; but in the latter disease there is an abrupt margin which you do not see in this case, and of course pyrexia. In such cases as this there is also generally a history of eczema elsewhere, and this girl has had it before. She is improving under the application of a mild ointment and lotion.

I have asked this next young woman to come to-day to show you the effects of treatment. She came to me at Blackfriars on Tuesday week with a chronic eczema on the palms of the hands which she had had for eighteen months. It was very much inflamed, swollen and desquamating, and itched a good deal. The improvement in a week has been very marked, although the skin is

* It is right to state that these lectures were not delivered with a view to publication, and were not reported at Dr. Abraham's request. They were taken down *verbatim*, and are printed with verbal corrections only.

still a trifle rough. I at once instructed her to stop using soap, and told her to keep her hands from water as much as possible. She has been using a very weak tar solution, creolin half a drachm to 10 oz. of water, and a bag of bran instead of soap. She has been also ordered an ointment containing a very small amount of liquor carbonis detergens, half a drachm to the ounce. Under this treatment her skin should get quite well. You can cure a good many skin diseases by external treatment only. The German school of dermatologists is apt to go too far, and say that there is no need for internal treatment in most cases. But in this country we believe that the skin is so intimately connected with the other organs of the body that internal remedies are often of distinct benefit. Still, in many cases of eczema and psoriasis you can cure the condition by external applications only. A most useful ointment for these cases, especially of dry eczema, and where there is not much redness and inflammation, is—

Liq. carbonis detergens	3ss.
Hyd. ammon.	gr. x.
Vaseline	ad 3j.

If there is much inflammation and moisture—that is to say, if it is a weeping eczema—we use at Blackfriars an ointment containing—

R. Plumbi subacetatis	gr. x.
Hydrarg. subchlor.	gr. x.
Zinci oxidi	gr. xx.
Ung. hydrarg. nitratis	gr. xx.
Vaseline	3j.

If the eczema still hangs fire after the application of the foregoing, then I find a very useful thing is to employ a modification of Lassar's paste. This is of a complicated formula. The modification I am fond of using contains two drachms of oxide of zinc, two drachms of starch, and ten grains of salicylic acid added to half an ounce of vaseline. All this is mixed up into a paste, and you may add to it a little liquor carbonis detergens, or half a drachm of ichthyol. That is a most useful application for weeping and vesicating eczemas.

The next girl presents alopecia areata. If you examine the scalp with a glass you will be able to see some hairs beginning to grow. She will certainly get back her hair. She is using the same ointment which I order for ringworm. It

contains one drachm of carbolic acid and one drachm of salicylic to one ounce of vaseline. That is well rubbed into the scalp every night. Then about once in ten days or a fortnight I rub in with a stiff brush some strong blistering fluid. What we have been accustomed to use at Blackfriars is called Burt's fluid. It is, I believe, stronger than the liquor epispaeticus of the Pharmacopœia. The brush is put into the fluid, and then squeezed against the side of the bottle, so that before applying it is almost dry. My theory is to get as little as possible of the liquid into the hair-follicles, and in that way to cause a deeper desquamation, and so remove the microbes at the mouth of the follicle. The method is, as a rule, efficacious, and you may sometimes see at Blackfriars as many as fifteen or more cases of alopecia on one afternoon, the place evidently having a reputation for the cure of this affection. But under the best treatment the hair does not come back quickly, and I tell the patients they must not expect it back for at least four to six months, and, indeed, not to give up hope for a much longer time. If the follicles are not destroyed, there is always reason to hope that the hair may come back.

The next female patient presents an ordinary case of acne vulgaris. I asked her to come so that I might show you the method by which the condition can be cured comparatively quickly. Acne consists in an inflammation of the sebaceous follicles. In the majority of cases the pustule forms at the site of a comedo, and you will usually find evidences of the latter in cases of acne. It is a good plan to press out these comedones, for if there are no comedones the inflammation should not take place. If staphylococci happen to get into the follicles pustules form. There are many ways of removing the comedo. The old-fashioned method of pressing them out with a watch-key, or



with machines made on that principle, is very painful to the patient. The little apparatus* I show you serves the purpose much better. By pressing the blades together a little hole like that

* Dr. Abraham's "comedo forceps" is made by Messrs. Mayer and Metzler, Arnold and Sons, and others.

in the watch-key is formed, and one can see exactly where it is pressing. The sharp points enable you to clear and divaricate the mouth of the follicle, which is often plugged up by hypertrophied epithelium, sebaceous *débris*, and an abortive hair or two. One cannot ask hospital patients to use such an instrument, but in private practice it is very useful. After this I use an ointment containing a small quantity of sulphur and some mercurial preparation; such as half a drachm of sulphur, five grains each of ammoniochloride of mercury and of the red sulphide of mercury (*hydrarg. bisulphatum*), and vaseline to one ounce. That should be applied to each acne pustule every night after bathing with hot water and soap. A non-irritant soap which I often recommend is Midgley's 10 per cent. ichthyol soap. If the skin is very delicate and not very oily, a superfatted soap is best; but if the integument is greasy, as it is in many of these cases, a soap with an alkaline base does better. In this matter one has to modify the soap according to circumstances; of course if there is much inflammation you cannot use strong soaps. If you do use soap, tell the patient to use it thoroughly, and leave it on in the form of a lather; for instance, while dressing in the morning, and also at night, before applying the ointment to each spot. The worse the case of acne the more obvious the benefit derived from this treatment, combined with the application I shall now speak of. I have here pure phenol with a very little water added; I take a little on a glass brush and apply it to the pustule, that will abort the young ones and cure the old ones. If the disease is acne indurata, and the inflammation deep in the corium, you cannot get at it in this way; you must then make a puncture with the acne lancet, the lancet having been dipped into the pure phenol. In some cases obstinate deep subcutaneous purulent collections form, and they are most troublesome to deal with. I find the best way of treating these is to wash out every individual abscess with a syringe. I use an ordinary hypodermic syringe with a blunt needle. I make an opening in the pustule first, then inject into it some antiseptic fluid, such as carbolised water, sublimate solution, or chinisol solution, and thoroughly wash it out.

A young lady came to me a year ago with quite a plum-pudding looking face, but she is now

absolutely cured of her acne, which had been a terrible disfiguration to her for fourteen years. I had to wash out every individual pustule, and to treat her as above for many months.

This next patient has come for the same condition. She has had very severe acne from time to time, and had a large subcutaneous abscess. Here again I made punctures, and washed out with the syringe. You will see that all the places on the cheek are practically healed. This method leaves slight redness for a time, but that disappears. Pitting may result in acne very much as in smallpox, *i. e.* if the corium be involved in the inflammation.

The next patient is, or rather was, also an example of severe acne, and he is very much improved since he came a short time ago. He had a lot of the lesions on his neck, and you will see smallpox-like scars in some parts; when he came the other day these were large pustules. Two weeks ago the neck was practically a mass of pustules. I ought to say that the application of phenol in these cases was first recommended by Dr. Walter Smith, of Dublin. He showed it to me as long ago as 1879, and I am surprised that it is not more in use in this country. I introduced it at Blackfriars in 1885, but there is little or no mention of it in the text-books. It is very rarely that the method fails if properly used.

The next man presents also a very severe case of acne. He came to Blackfriars only last Tuesday, and I gave him some ointment to use. In many of these cases there is some indication of dyspepsia. We generally give one grain of sulphate of iron and a little sulphuric acid, about half a drachm of sulphate of magnesia, and some bitters or other stomachic,—indeed, the "*mistura ferri aperiens*" of many hospital pharmacopœias. If there is much erythema in these cases of acne rosacea it is sometimes advisable to give bismuth and alkalies before meals. Of course a good many of these rosaceous cases do not depend upon alcohol at all. Occasionally, as in this one, we get cases in which some such cause can be traced, but as a rule dyspepsia and feeble circulation are the cause of a red nose, not alcohol. One of the worst cases of rosaceous acne I have seen was in a teetotaler. If a person, however, has a tendency to acne, no doubt intemperance might increase it. Ichthyol internally is sometimes useful in hyperæmic cases, and recently I have found great benefit from

menthol $\frac{1}{4}$ -grain pills. It is important always to keep the bowels right.

I now want to show you a method which I find efficacious in treating ringworm. These two children have ringworm of the scalp, and they had it for some time, I dare say without the knowledge of the mother, as she says. I asked them to have the heads shaved before she brought the children here, so that I might apply my machine. As the heads are shaved I shall not be able to show you the typical short hairs. But it is difficult to see very small patches in other parts of the scalp unless the head be shaved. You may cure one area, and afterwards find the ringworm break out in another which you had not seen before. I give them one drachm of carbolic acid and one drachm of salicylic acid to one ounce of vaseline, to be rubbed in thoroughly every night with a stiff brush, and to smear a little on elsewhere, so that there may be less chance of the fungus spreading on the child's own scalp and to other children. The whole scalp, indeed, must be kept greasy with the ointment. I recommend the patients to wear calico caps constantly, burning them every day or two and substituting fresh ones. Before using my machine I have the head thoroughly washed, but it does not matter what sort of soap is used, still I often recommend, about once a week, a soft soap containing boracic acid. The theory of the machine is as follows. The fungus gets down at the side of the hair and deep into the hair-follicles. If the fungus remains superficial you can get at it with any substance you like—mercurial preparations, carbolic acid, &c. It is in such superficial cases, and in tinea circinata on the face or body, that you can cure the affection by tincture of iodine, common ink, or turpentine, which are favourite remedies in some homes. But if the fungus has got deep down into the follicles it is very difficult to cure by the application of liquids or ointments. You may treat it very effectively, however, by some such method as that adopted in creasoting logs of wood. The wood is put into an iron tube, from which the air is subsequently pumped, and creasote is then let in under the atmospheric pressure; the creasote thus replaces the air in the crevices of the wood, which thereby becomes saturated with the liquid. In ringworm I epilate as much as possible about the patch, and take out all the diseased hairs as far as I can.

The head is previously washed with soft soap and hot water, and dabbed on with alcohol and ether to remove the grease. In this pumping apparatus which I show you I generally use creasote in hospital, but for private patients I often substitute guaiacol mixed with ozonic ether, or some other penetrating germicide fluid. Guaiacol and ozonic ether mixture seems to penetrate into the follicles better than the creasote, and certainly it has not the objectionable smell of the latter. I put about half a drachm of liquid into the machine, place the cup over the scalp, and then pump, taking care not to do it with much force,—that is to say, I only create a partial vacuum. This operation may be repeated several times, until we may consider the area of scalp under the cup thoroughly saturated. Sometimes one can see the bubbles of air coming from the hair-follicles into the liquid above. No



(Block supplied by Messrs. Arnold and Son.)

fungus will grow where the liquid has penetrated. I have cured in this manner obstinate cases of ringworm which have lasted for years; I know one of twelve years' standing which was cured thus. One point you will have to be careful about, and that is to get the excess of liquid off the scalp afterwards by a wad of absorbent cotton, before it can run on to the face or other parts, because there it would produce much irritation and perhaps some inflammation. After the instrument has been used

the patch is sometimes considerably reddened. The cups are of varying size to suit different parts of the head. The machine is made for me by Messrs. Mayer and Metzler, as well as Messrs. Arnold and Sons.

I next show you a very severe case of psoriasis in a woman of about 26 years of age. I saw her on Tuesday last for the first time, that is two days ago, and she wanted to come into the hospital. She has had psoriasis ever since she was thirteen. She says she has taken a lot of medicine for it, and no doubt she has been fairly well saturated with arsenic, for that is the usual treatment for this affection. In its present stage if you saw portions of it alone you might think it was a case of pityriasis rubra. There is great inflammation and redness, and a tendency for large flakes to come off. But on carefully looking at other places you see there typical patches of psoriasis, and she gives the history usually associated with that disease. I had one case of psoriasis in an old person in whom the whole body was like this, and became inflamed in a terrible manner. This patient has the disease chiefly on the arms, legs, perinæum, and back. In a case of this severity the skin gets in time so inflamed and indurated that it is difficult for the patient to walk. She has also severe itching and burning pain. I shall order for her a bath containing creolin, if possible twice a day; that is to say, a warm bath for ten minutes, not longer, because we have to be careful not to weaken the patient in any way. Then she will have an ointment containing half a drachm of creolin, and she will have internally probably cod-liver oil and phosphate of iron. You will notice that she also has an enlarged joint and evidences of rheumatism. Not infrequently in psoriasis there are some evidences of rheumatism, or a history of rheumatism or gout. It is an old view which has been discarded to a great extent among dermatologists, and I think it is a mistake to discard it altogether, because one does get cases every now and then where there are evidences of such a diathesis. On that account I shall probably give her some salicylate, the mixture containing something as follows: 15 grs. of salicylate of sodium, 4 mins. tincture of perchloride of iron, and 3 mins. of liquor strychnine. The baths will have half a drachm of creolin to every six gallons of water. Nearly every book says you must use arsenic for

psoriasis, and I believe there are very few dermatologists besides myself who do not give it in these cases. But I have seen so many cases which have been almost poisoned by arsenic without any apparent improvement; in fact, where there has been any benefit from arsenic in the cases I have seen, I believe it has been because of the illness produced by the drug. The treatment I have indicated generally proves very satisfactory. I have seen several cases of psoriasis in which some intercurrent disease has appeared. The old woman whom I referred to just now had the whole of her body covered with confluent psoriasis to the same extent as this person's legs. I thought she would die through the want of sleep, and so on. She got some gastric attack which made her very ill, and a month or two afterwards she turned up again at the skin department with her skin nearly clear. She said she had been doing nothing, but had been very ill with inflammation of the stomach, and that her skin had got right. I have seen the same thing in women with psoriasis who have acquired syphilis. There was a case of Mr. Tay's, a woman who acquired a chancre on her lip, and she developed the usual secondary symptoms of syphilis. While she had the specific manifestations upon her, her old-standing psoriasis disappeared entirely. The same thing I have observed to occur in influenza and other fevers. My view about arsenic is that if you give enough of it to make the person very ill it influences the nutrition of the skin as of other parts, and the psoriasis may for a time disappear. It is the same with thyroid gland. If you use thyroid gland in some cases you will cure psoriasis, but the question is whether it is not at the expense of other organs. At any rate, I will undertake to say that this patient will go out of hospital perfectly clear without either arsenic or thyroid. I know that is a bold thing to say, but I have generally succeeded, even in very bad cases of psoriasis, if the baths and ointment have been kept up and properly used and other precautions taken, in curing such a case in from four to six weeks. I shall be glad if you will watch the case. If the tar treatment hangs fire, I shall paint on chrysarobin in traumaticin.

The next patient is a man, the subject of very severe pemphigus. I show you a photograph of him which was taken a short time after he came in. I am glad to say he is doing very well, and

the bullæ are disappearing quickly. You can see some of them on the arm. He is seventy-two years of age. Six weeks ago he was suddenly taken with intense irritation of the skin over the abdomen, accompanied by redness. This spread over the whole of the upper part of the body and upper limbs, spreading from place to place. Five weeks ago he first noticed small blisters containing clear fluid on the upper limbs and trunk; these increased rapidly until the whole body was affected with these large blisters, and some were to be seen



Case of Pemphigus.

on his face. When I first saw him at Blackfriars there was a tendency for the lesions to be grouped, and as they were not so very large I was inclined to think it was possibly a case of dermatitis herpetiformis, but very soon the bullæ increased enormously in size, as you can see in the photograph. One bulla extended completely round the ankle, measuring about ten inches in length. One of the early symptoms, and at the time I saw him the chief one, was fearful neuralgic pains in the arms. That is interesting as showing the con-

nection which has been often recognised between pemphigus and the nervous system. A good many cases of pemphigus give some history of nerve trouble. I am sorry to say that in this, as in many other diseases, the ætiology and pathology have not been completely worked out. A good many of the bullæ were first hæmorrhagic, and some of them became cloudy and purulent. The patient has had a few febrile attacks, his temperature having reached 103° . He has had a few attacks of diarrhœa, which gave us some little concern, as indicating the formation of bullæ in the alimentary canal, but fortunately we have been able to combat all those symptoms, and his condition is much better in every way. He is using a mild ointment consisting of oxide of zinc and boracic acid to dress the wounds. The bullæ are snipped when they get large, and are dressed antiseptically. Day and night he is bathed with antiseptic lotion. It is most important to treat these cases as aseptically as possible, because absorption of septic products is serious.

WE have received from Wright & Co., of Bristol, three neatly bound little books of a size convenient for the waistcoat pocket, being the first of the "Golden Rules" Series. Mr. Hurry Fenwick's 'Golden Rules of Surgical Practice' is now in its fourth edition, and is too favourably known to require notice; but Dr. Aaron's gynæcological rules and Dr. Fothergill's medical rules are most admirable examples of what can be done in the way of condensation. Clearly and concisely these little books crystallise into a few words much that we all know, and in thus accurately expressing commonplace notions render our working knowledge of more immediate value. The same firm are also publishing a most serviceable note-book arranged alphabetically for the practitioner's private formulæ, and being identical in size with the "Golden Rules" series its utility is thereby enhanced.

Orthoform in Intra-muscular Injections.—

Loeb and Raff state that the addition of 5 to 10 per cent. orthoform to the 10 per cent. Hg-salicyl. suspension renders the injection painless for eight to ten hours afterwards.

Derm. Cbl., December, 1898.

MEETING OF THE SOCIETY OF ANÆSTHETISTS,

At 20 Hanover Square, January 20th, 1899;

The President, Dr. DUDLEY BUXTON,
in the Chair.

Resumed Discussion on "The Choice of an Anæsthetic."

(Concluded from p. 272.)

MR. STANLEY BOYD said he came with the intention of saying a few words on the question before the Society, but most of what he had intended to state had been said much better by Mr. Haward. He agreed practically with everything Mr. Haward said. With regard to the anæsthetic he (Mr. Boyd) favoured, as a rule he would say most decidedly it was ether. With regard to chloroform in children, he had quite lately, at the Children's Hospital with which he was connected, asked that an anæsthetist should be appointed. Hitherto the anæsthetic was given by the residents, and almost invariably his experience was that at every new appointment he (Mr. Boyd) had to be at both ends of the body of the patient at once. The accidents which had happened almost all took place in the early stage of anæsthesia with chloroform; the child struggled, and the inexperienced anæsthetist did not realise the fact that when the child was crying and breathing deeply it was taking more chloroform than was good for it. Then came a sudden stoppage, which the surgeon had to deal with himself. The objections with regard to ether seemed to be chiefly from congestion, and the violence of movements, which interfered with some operations. There were difficult cases in which they were almost compelled to give chloroform. The difficulties in connection with administration varied very much, it seemed to him, with the particular administrator. With some anæsthetists the movements of the abdomen were very much less than with others. Still it was necessarily difficult to judge, because no two patients were exactly alike. At the same time he thought he had seen enough to be able to say that one was more comfortable with some anæsthetists than with others; and he thought one thing which was responsible for a good deal of the difficulty was the haste with which the anæsthesia was produced.

If a patient was pushed very deeply under gas and ether, the full pressure of the ether or chloroform being applied at once, the patient got greater congestion and respiratory obstruction than under other conditions. He had no doubt the surgeon was largely to blame for the hurry; when he had got six or seven operations he naturally wanted to get on; but if that was the case he must pay for it, even if the patient did not, for the inconvenience the anæsthetist was put to. Another thing which seemed to add very much to this violent abdominal movement, was that the air-way did not seem to be quite free. He had of late suggested to young anæsthetists that it would be worth while to try to use the gag; gagging the mouth open and extending the head had in many cases resulted in the breathing becoming at once very much improved. It seemed to him a point which juniors might very well try when they had difficulty owing to violent movements of abdominal muscles.

With regard to cleft palate operations, it seemed to him it was well worth while to paint the area with cocaine, and if possible abolish the swallowing reflex in that way. Chloroform was thought to be the anæsthetic most suitable in this operation, for reasons which had already been stated several times, but to paint the part with cocaine to abolish the swallowing reflex was a very great help in the operation.

There seemed to be a very strong feeling in the Society that the best position for removal of adenoids, and for jaw operations, was with the head hanging down. Personally it was not the position he liked best: he liked the patient to be on his side, a pillow up behind the patient's back, and the corner of it placed under the patient's cheek, depending at the angle of the mouth so that everything would run out of that angle. He stood on the right side of the patient for both jaws, turning the head from one side to the other. He thought those who helped him by giving the anæsthetic would say that they practically never had any trouble with blood entering the air-passages; it was exceptional to hear any gurgling, and there was not the oozing which was found when the head was hanging down. Of late he had been using what he saw Mr. White spoke rather against in his opening paper, namely morphia. If morphia and atropine were given before the

administration of the general anæsthetic, he believed that less chloroform was necessary, and although he had not had very much experience of it he was satisfied with it; he thought anæsthetists also were.

Regarding those bad abdominal cases referred to of greatly hampered heart and lungs, to give any anæsthetic and to push it must be fraught with danger, and under those circumstances the view of Mr. Haward seemed to be the right one, namely that they should use cocaine freely. They ought to do so before trying to find the cause of the obstruction, and then the general anæsthetic could be given under more favourable circumstances.

Mr. EASTES said that so much of what he had intended to say had been already mentioned, that there was little left for him to talk about. The general anæsthetic which he preferred was ether, preceded by gas; but there were certain cases in which he thought ether should always be given, and in which chloroform should not take its place; for instance, in shock, and intercostal and diaphragmatic paralyses, even though there was reason to expect some bronchitis might ensue. Again, he would not choose chloroform in fatty degeneration of the heart, nor in cases of low blood tension. Neither would he give it in cases of abdominal and pelvic surgery if it could be avoided. In adenoids he would not add gas to the ether, because it made so much venous congestion that the hæmorrhage which resulted was extreme. In all cases in which the sitting-up posture was required ether should be given. For men ether seemed altogether to be the preferable anæsthetic; women seemed to take chloroform better than men, and there had generally been a larger mortality amongst men with chloroform than amongst women. So far as chloroform was concerned, he thought it should be given in extremes of life, in post-apoplectic states, in acute and spasmodic conditions of the respiratory organs, in cyanosis with cases of high tension, and also in cases of renal disease. In diabetes he used both chloroform and ether, and he thought with equal success. In gall-stone colic he had kept a patient anæsthetised for a great many hours with chloroform, although he had not pushed it beyond the point up to which one used it in midwifery. In cleft palate he used chloroform.

He might refer to one peculiar case in which there was stenosis of the upper air-passages. A child had had tracheotomy performed two years before; the surgeon tried to open the passage again. For nearly two hours he (Mr. Eastes) gave chloroform through the tracheotomy wound, with the result that the child kept under fairly well. He was very sorry to see that Mr. Owen made such a dead onset against the A.C.E. mixture. Ether being safer than chloroform on the average, if there was only a small mixture of ether with chloroform it was better than none at all; and with regard to not knowing how much ether and how much chloroform there might be in the bottle, he thought the A.C.E. mixture should only be made just before it was required for use, being poured from a drop-bottle, which could only be opened by the finger of the anæsthetist depressing the spring. He thought also it was the best anæsthetic for practitioners who were not often giving an anæsthetic. In cases of labour he constantly used it himself. And they must remember that although ether might be extremely good in cases of labour, such as the one related by Mr. Macnaughton-Jones, it was not always one could find a second surgeon to assist him, and the operator had to be content with whatever assistance was at hand. With regard to cocaine, he had seen so much faintness from it that he would be extremely reluctant to use it upon children; the depression produced by it was excessive. Again, it was highly desirable that children should not know what was going on, and the higher functions were not annulled by cocaine as with general anæsthetics.

Mr. CARTER BRAINE said that so many points had been touched upon that he would only just make a very few brief remarks. As regarded the suitable anæsthetic for various ages, it had been his practice to administer the A.C.E. mixture to children up to six years of age. Concerning the later age, he thought there was no bar to the administration of ether. That week he had had one patient who was seventy-five years of age, a big, fat individual who had to undergo a lithotomy. It took half an hour, and he consumed two and a half ounces of ether. Four hours afterwards he was smoking a cigar and wanted to go for a walk. With regard to cases in which diabetes existed, he had a very open mind as to what anæsthetic

should be administered. He had many times given ether and also chloroform, and really he could not say that he would have preference for the one over the other. With regard to the operations attended by great shock, he certainly thought ether was the anæsthetic to be administered, and ether only, that is to say without previous nitrous oxide or other anæsthetic. These cases required a very light anæsthesia indeed. With regard to mania, he might mention a case that happened recently. All arrangements were made, but on the morning of the proposed operation the patient became a lunatic, and therefore the operation was put off. If the operation had been fixed for the previous day, he thought most people would have said the anæsthetic had something to do with the loss of reason. Abdominal operations for a condition of distension, or in which distension was present, were some of the worst cases in which an anæsthetic had to be administered. Formerly he gave gas and ether, but he had discarded that for A.C.E., and he had managed to get patients to pass under the anæsthetic better than formerly. Those cases in which there was intestinal obstruction and regurgitation he considered to be some of the most alarming they had; just when they were getting into the surgical stage there seemed to be a relaxation of the œsophageal opening of the stomach, and there was a gush of fluid which was very apt to get into the larynx. He thought in all such cases the stomach should be washed out thoroughly previous to the anæsthetic being administered. In renal disease in which there was albuminuria he did not know which was the best anæsthetic, whether chloroform or ether. In the few cases of which he had taken notes after ether administration albuminuria was present, but on the second or third day it had come down to normal and there had been no bad effects. As to operations on other organs, he might mention cases of goitre. These were very awkward cases to administer to, and he had found that the plan to which Mr. Boyd referred—that of giving morphia before the administration of the anæsthetic—helped them very greatly, because of the smaller quantity of chloroform which was necessary. But he would like to caution against moving the head under such conditions, even from one side to the other. He had known one case in which the head was

moved to one side and the trachea became occluded. Laryngotomy was done, but it had no effect; therefore tracheotomy followed, and by placing a catheter well down to one of the bronchi the patient was resuscitated.

In conclusion he would like to say that this was a society able to do good and lasting work by settling this point in very many instances. Surgeons began to look to the Society for some definite opinion on this subject. All the members held hospital appointments, and a vast amount of valuable material passed through their hands annually. They were all very busy, and could not follow out their cases as they might wish to do. But they might follow out a few of them, and he therefore suggested that each member should send in a minimum of five fully reported cases annually bearing upon a subject or subjects selected by the council. They would then accumulate 300 cases each year. A committee should be appointed to report upon them, and they could then produce a very valuable opinion on the selection of an anæsthetic.

Mr. H. Low said he was not present at the last meeting, and would like to know whether any reference had been made at the last meeting to a paper which appeared in the 'Lancet' on the question of giving the A.C.E. mixture from a Clover's inhaler. He thought the practice in London had been never to put chloroform into a Clover's inhaler, but the writer of the paper said that it had been the practice for fifteen years in the Midland counties. Mr. Tyrrell had pointed out in his two-bottle method the great advantage of a mixture of chloroform and ether without the alcohol, and if, as that paper seemed to prove by a considerable number of cases, it was an advantageous mixture, he thought the Society ought to take up that question while discussing the matters now before it; he thought it was too important a paper to be passed over.

Mrs. DICKINSON BERRY said in reference to Mr. Haward's statement about giving ether, as house surgeon at the Belgrave Hospital for Children, she gave ether as a routine anæsthetic in all cases of children over two years of age. They had no case in which there were any alarming symptoms of any kind. The patients took the anæsthetic quietly, and she did not think there was any more terror with it than with chloroform.

They had no cases of ether bronchitis or anything of that sort.

Mr. TYRRELL said he wished to explain his position. At the last meeting he agreed with Mr. Owen in his efforts to make chloroform more often used; he agreed also with Mr. Allingham in the choice of ether for rectal operations, while for Mrs. Scharlieb's operations of vaginal hysterectomy gas and ether was the best. What he really meant was that he did think there was an extended field for chloroform used more carefully, and especially where a little ether could be added to it by the double-bottle method. Mr. Allingham said he preferred ether for the more common operations, namely for hæmorrhoids. He did not suppose there was an anæsthetist in the Society who did not give ether as a routine for hæmorrhoids. He must say he did deplore that there were a few "etherists" in the Society; by etherists he meant men who vowed openly that they never gave chloroform if they could possibly avoid it; he did not think there was a member of the Society who would be glad to give up ether, but amongst surgeons there were "chloroformists." He once entered a room to give an anæsthetic to a lady, and the operator, who was getting ready for the operation, said, "Chloroform, if you please; no ether on any account whatever." Now that surgeon was going to perform a laparotomy for the removal of both ovaries in a young lady. He said he had given ether to this lady on a previous occasion, but the surgeon replied, "I do not care; I can have nothing but chloroform." This young lady was a hystero-epileptic, and her right heel was making a hole in her buttock. He gave her ether for upwards of two hours to bring the leg straight and put it into plaster of Paris. On another occasion he gave her ether for bringing the other leg straight, which was not quite such a long procedure. But on this third occasion he was told to give her chloroform, and accordingly had to do it. He confessed the patient did very well; both her ovaries were successfully removed. But she did not recover from the hystero-epilepsy, and died some months after the operation, but not as a result of the operation or the anæsthetic.

The PRESIDENT said what had struck him very much throughout the discussion was the personal predilection element, if he might say so, rather than the accumulation of experience. Possibly

perhaps the personal experiences had been purposely given rather than the facts accumulated by experience. Attention had been drawn to the fact that Mr. Owen, in his racy speech, gave them his personal experience and predilection rather than any evidence that would substantiate the position he had adopted. Another point which struck him had been, not only in this discussion, but in the daily intercourse with the surgeons, that they had arrived at their preference for one or another anæsthetic largely from their experience with anæsthetists. They commonly found a gentleman who had worked with those who gave chloroform extremely well, but whose experience perhaps did not extend to ether, had a strong predilection for chloroform. On the other hand, many surgeons who had been accustomed to ether had a very great fear, in many cases an undue fear, of chloroform. And then unquestionably a great deal of damage had been done by labelling men "chloroformists" and "etherists." He was very glad to hear Mr. Tyrrell refer to that point. Many were biassed as to an anæsthetic, more and more by the method in which it had been given, rather than by the essential behaviour of the anæsthetic itself. Unquestionably an anæsthetic given in one particular way might lead to very beneficial results, and in another might not yield quite such a success. He was surprised to find that many of them in dealing with the question had not dwelt upon the variety of methods suitable in any particular conditions in which one anæsthetic might be given. It was certainly a fact that an anæsthetic when given in a certain way gave a satisfactory anæsthesia, which it would not do to such a useful extent if given in another particular manner. Certainly, speaking of ether, it was true that given as it should be given, it did give a much better account of itself than when, perhaps, it was given by those who were not experienced with it. He had no hesitation in saying that the majority of people gave chloroform very much better than they gave ether. There were comparatively few people, except those who had had experience, such as most members of the Society had no doubt had, who could give ether as it should be given, without having achieved many failures, and having worked through their failures to successes. Another important point which perhaps had not been ventilated so fully as it should have been was the evidence

about the after-effects of anæsthetics. They were told generally that ether produced bronchitis and all manner of terrible things, but they were not given chapter and verse for this; and, curiously enough, many surgeons who must have noticed these things had not been able to say, or rather had not chosen to cite certain definite cases. They knew that in London now ether was given many thousands of times in the year; and if, as was stated in one of the medical periodicals, ether pneumonia [occurred once in every 300 administrations, the enormous number of ether pneumonias that occurred in London—the result of ether inhalation—must be at least startling, and yet one never met with them. With regard to chloroform, they knew that the statistics which had been collected abroad, where chloroform was perhaps used more largely than here, showed that untoward after-effects did follow chloroform, and they had been tabulated in such a form that he who ran might read. Still few of those after-effects were ever spoken of by “chloroformists,”—surgical as well as anæsthetic,—but they did hear of the after-effects of ether. With regard to children, he was entirely at one with what had fallen from Mr. Warrington Haward and others with regard to giving ether to children. Many years ago he (the President) commenced giving ether to children, being led to do so by the perusal of Mr. Haward's paper in the ‘Transactions’ of the Royal Medical and Chirurgical Society; and if one accustomed one's self to it, remembering that one was dealing with a delicate organism, and that some amount of caution must be used, it could be given successfully. If children were drenched with ether, they very naturally got ether bronchitis and other ill-effects. But if given as it should be given, he really failed to see the least justification for the statement that ether produced more trouble in children than chloroform did.

Mr. WHITE, in reply, said the President had answered very accurately the question of the personal equation in the giving of an anæsthetic. Each anæsthetist had his own ideas formed upon his practice, and could judge which was the best anæsthetic to be given in each case. As the anæsthetist felt in the matter so he would permit him to give the anæsthetic. Mr. Owen commenced by saying that if he fell down outside and got a dislocated hip, he would say to the man “for

goodness' sake give me chloroform.” He, Mr. White, would not. He would say “give me what you can give best; do not take my view of the case; you know your own personal powers in the matter.” He always had in mind a case somewhat like Mr. Tyrrell's. He anæsthetised a lady twice; once with chloroform, once with ether. She begged him to give her chloroform on the second occasion; he did so, and she died there and then.

With regard to the bad after-effects from ether it must be remembered that it was not given in that large quantity which it was formerly; and with the improved methods of giving the drug, lung trouble seldom occurred—that is to say, compared with ten years ago. Indeed they could now be placed almost out of consideration. In choosing an anæsthetic, of course one naturally tried to get the safest. He could give ether for any age and for any purpose. If owing to mechanical reasons he could not give ether, he gave chloroform.

If the Society could by any means draw up a scheme to assist the profession in the choice of an anæsthetic they would do a great deal of good; but he took it that it would be difficult and almost impossible to prepare such a document.

THE DIAGNOSIS OF THE CONDITION OF EACH KIDNEY BY INOCULATION OF THE SEPARATED SEDIMENTS INTO GUINEA-PIGS IN SUSPECTED RENAL TUBERCULOSIS.

LITTLE is yet known of the natural history of renal tuberculosis; indeed, it is for comparatively few years that we have known that tuberculosis can be primary in the kidney, and the great mass of the profession has not yet realised that this disease is often localised for many years in one kidney before invading the rest of the urinary tract.

It is apparent that the medical descriptions refer to what we from a surgical standpoint should call advanced disease. Our surgical speciality has already led us to the possibility of establishing the diagnosis at a very much earlier stage than this.

Though such a diagnosis can be made only by physical examination, a suspicion of tubercular or other renal disease will often be excited by the symptomatology, and this must therefore be briefly reviewed. The early symptoms of the disease will

vary greatly both in character and in intensity in different cases, and from time to time in the same case, but will consist typically of pain and tenderness over the abdomen on the affected side, frequency of urination, and sometimes hæmaturia. These symptoms are, however, equally characteristic of simple pyelitis, renal calculus, and new growth in the pelvis of the kidney. There are, indeed, no differences of kind in the symptomatology of these diseases in their early stages, though there are differences in the degree in which the several symptoms are likely to be present.

All these diseases are characterised by dull pains over the kidney and along the course of the ureter on the affected side. The pain is often bearing-down in character, and therefore a uterine origin is usually assigned to it; is usually increased by standing; and is always more or less associated with frequency of micturition, occurring at night as well as in the day-time. In all of them the call to micturate is a very urgent one; the pain on micturition is usually referred to the meatus; and all are liable to more or less tenesmus at the end of micturition. In all the pain may be increased by jarring or jolting (as in driving over rough roads). This group of symptoms is, however, in reality symptomatic of the amount of inflammation present, and therefore varies with the subsidiary feature of the case. They are often though not always more marked in renal stone than in the other diseases.

All the diseases enumerated may be accompanied by hæmaturia, but this is rare in simple pyelitis; it is more likely to be profuse in tuberculosis or a new growth than in stone.

In all, the patients are almost equally liable to attacks of mild renal colic, due usually to inflammatory obstruction in the ureter. They differ from the pains excited by the passage of a calculus in being less severe and not followed by the appearance of the stone. These attacks are perhaps less common in new growths than in the other diseases under consideration. Not infrequently, at intervals in the course of these chronic renal diseases (and especially in tuberculosis and simple pyelitis), the inflammatory symptoms will be found to be most marked on the sound side. This is probably because each exacerbation of the disease in the affected side leads to an increased elimination of toxic materials from the functionally

more active kidney; and this excites a transitory and somewhat acute inflammation in the mucous membrane of the urinary apparatus on the sound side.

This transposition of symptoms I have seen so frequently (I may say almost constantly) that I am sure it must always be guarded against. The side on which the patient tells us that the symptoms were first noticed is usually the diseased side.

In all these lesions we find, on palpation, a tenderness which may extend from top to bottom of the urinary tract on the affected, and even on both sides, but which is usually most marked at one or more of three points: namely, over the kidney; at the spot where the ureter crosses the brim of the pelvis, in which case it is often limited to a spot the size of the finger-tip, midway between the umbilicus and the anterior superior spine of the ilium (McBurney's point or its fellow); and, finally, over the vesical end of the ureter at the side of the cervix, which examination may even detect an enlargement or induration of the ureter. These tender points are again symptomatic of the amount of inflammation present, and therefore usually vary with the amount of pain.

A study of the history and the results of the gynaecological examination usually enables us, then, to suspect, and sometimes permits us to postulate, a diagnosis of some renal disease of a surgical nature, but it does not enable us to say what, nor always on which side it is. The special examination now steps in, and the real diagnosis begins here.

If a visual examination of the bladder shows that the vesical mucous membrane is substantially normal except in the inter-ureteral region, and that a strongly localised inflammation is present there, the source of that inflammation is probably to be found in the passage of a vicious urine from one or the other ureter. If one ureteral orifice is abnormal in appearance, this probably marks the diseased side. The next and most important step is the catheterisation of the ureters and a microscopical and chemical examination of the urine secreted by each kidney. But here the results must be interpreted with the greatest care, as recent advances have made it certain that most of our past opinions on the results of urinalysis must be revised in the light of the new knowledge. This is especially true of renal tuberculosis; the

large amounts of degenerated pus and bits of necrotic material which are commonly described as characteristic of the disease being, in fact, found only in locally advanced cases, while the urinary signs of early tuberculosis are usually limited to the detection of pus and the bacilli by the microscope.

In the more advanced of the class of cases which are still quite operable we are indeed almost sure to find more or less degenerated pus, but the amount of it varies greatly from time to time, and is not infrequently insignificant. The detection of tubercle bacilli in the sediment of the urine is of some positive value (it is absolute if its confusion with the smegma bacillus is sufficiently carefully excluded; and it should therefore be absolute in the sediment of the urine obtained by ureteral catheterisation). Its absence is of no diagnostic value whatever; *e. g.* I have had a highly trained expert make repeated negative reports on the urine from a bladder in which tubercular ulcerations were actually visible, and in which a subsequent report was positive.

It is probable that the discharge of the bacilli with the urine is not uniform, and no man can expect to search a sediment so thoroughly as to detect the bacillus with certainty if only a few are present; but it is held that if a fresh sediment is injected into the peritoneum of a guinea-pig, the presence of only one or two bacilli will be enough to cause infection in this very sensitive animal. The generally accepted opinion that this is the most delicate test for tuberculosis known, and the great surgical importance of using the most delicate test possible for the determination of the condition of each kidney separately in suspected renal tuberculosis, has, then, been my reason for injecting the sediments obtained from the kidneys by ureteral catheterisation into separate guinea-pigs, in each of three cases of suspected renal tuberculosis.

If this test is to be of real surgical value two points must be determined with regard to it: 1. Will it give us positive evidence in the early stages of the disease? 2. How absolute is the negative evidence obtained by the negative results from the other kidney in the same case? But neither of these questions can be answered by anything but an extended experience.—EDWARD REYNOLDS, M.D., from *Bulletin of the Johns Hopkins Hospital*, November, 1898.

Vaccination.—A very admirable and praiseworthy device, enabling a medical practitioner to expel with ease and cleanliness the contents of a vaccine tube, is to be had from J. H. Montague, 101, New Bond Street. New apparatus and new appliances are always coming forward, and novelty is often the sole reason for the passing interest they may arouse, but in this useful and inexpensive vaccine expeller we have ready to hand the means of effectively completing an otherwise somewhat awkward manœuvre. The objectionable and dangerous method of blowing out the lymph by placing the tube between the lips needs only to be mentioned to be condemned; but one must congratulate Mr. Montague who invented this ingenious arrangement for thus furnishing the profession with a method as satisfactory in its mechanical result as that obtained from the mouth, and equally as satisfactory from the point of view of



absolute asepticity. The tube with sealed ends is first drawn midway through the larger opening as illustrated, the upper end of which is then broken off, and is drawn so far through the hole that the broken end is only about half an inch from the smaller hole. The other end of lymph tube is now broken off, and the mount held between the first and second finger, whilst placing the thumb over the top opening and compressing, the Vaccine is immediately expelled. By this means the Vaccine can also be distributed as desired.

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THE NORMAL TEMPERATURE OF LYING-IN WOMEN.

THE CAUSES AND TREATMENT OF SOME VARIATIONS.*

BY

W. S. A. GRIFFITH, M.D.

THE subject which I have chosen for discussion, viz. the normal temperature of lying-in women, is one of the highest importance to all who practise midwifery, and, like so many others in medicine, our knowledge of it has been rendered more exact by careful observation of normal cases under favourable conditions of antiseptis. It is not many years since the statistics of fever in childbed were statistics of death, but now that the principles of antiseptis have become clear, and in consequence reasonably simple to carry out, such statistics of death have almost given way to statistics of disease.

The opportunities for accurate observation which are afforded by the practice at Queen Charlotte's Hospital enable me to make this contribution as the subject of my paper. They add nothing new to the physiological portion of the subject, but confirm what has been observed by others, that "the temperature of a woman during the natural and physiological puerperium, differs in no important respect from that of a woman in perfect health;" from which follows the important principle that "if the temperature does vary beyond such limits, the cause is pathological and not physiological."

If we accept these fundamental propositions, we cannot but agree that the earlier we are able to recognise all deviations from the normal, and their causes, the better for our patients, and our own success as practitioners.

If this be granted, as I cannot for a moment

* A Paper read before the East London and South Essex District Metropolitan Counties Branch, British Medical Association, at Leyton.

doubt, we are met with the question "Is the accurate and systematic observation and record of the temperature of lying-in women practicable under the ordinary conditions of practice?" Many will say that it is not, and within certain limitations I am sorry to say I must agree with them. Where patients are able to afford themselves a thermometer and the assistance of a nurse who has had some elementary training, there can be no excuse for the omission of such an important detail, but there are large numbers of very poor women to be attended who cannot afford either one or the other.

We have found at Queen Charlotte's Lying-in Hospital that temperatures taken at intervals of six hours during the day-time, namely at 8 a.m., 2 p.m., and 8 p.m., give us sufficiently reliable information, and this is our routine practice. The patients are not disturbed during the night unnecessarily, but more frequent records can be obtained when desired.

A morning and evening record are alone of great value, but the early afternoon one should not be neglected, for it is often found that the temperature begins to rise soon after midday.

It is essential that some such regularity should be observed, for if the temperature be taken at 10 a.m. one day, at 4 p.m. the next, and not at all the following day, the results are of little value and may be entirely misleading.

There is a practice common enough and one which cannot be too strongly condemned, viz. that of waiting for a rigor or some other definite sign of severe illness to occur before taking the temperature. How often do we hear the remark that all went well until a particular day, when a rigor occurred, and the temperature was found to be high. Such a statement is rarely true, for in most cases early signs precede the severe ones, and had they been recognised, treatment might have been adopted which would have saved the patient from the graver illness.

There is a common prejudice against the regular use of the thermometer in midwifery, as it is said to alarm the mothers, or at least to be liable to make them nervous. I agree with those who believe that regular observation is less likely to frighten than the occasional use of the thermometer when the patient feels ill, and I am certain that if we did not act on this principle at Queen

Charlotte's Hospital, where we admit nearly 100 cases each month, and where the nursing is carried out to a large extent by pupil nurses, most of whom have had no previous training, we should have very different results to record and far greater dangers to deal with.

Let us take it then that when practicable the temperature of every lying-in woman *must* be systematically taken and recorded, and at such hours as will disturb her least. It is scarcely necessary to add that care should be taken to keep the chart out of sight of the patient and of too inquisitive friends.

The charts, sixty in number, which I hand round this evening are those I use at Queen Charlotte's Hospital, and have been selected to illustrate some particular point. They contain a great deal of information besides the mere record of the temperature to which I shall only incidentally refer.*

The upper line on the temperature chart is the uterine "involution curve," a paper on which subject by myself and Dr. Stevens is published in vol. xxxvii of the 'Transactions' of the Obstetrical Society of London. At the end of each month all the charts of patients discharged during that period are divided into two groups, those in which the temperature has not at any time exceeded 100° F., and those in which it has risen above this point. The former are considered to be normal, the latter are grouped as cases of pyrexia and are sorted into different classes.

The first group, which contains an average of about 60 per cent. of the patients admitted, will be seen to contain a number of cases where the temperature has not exceeded 99° F., and personally I should prefer to place the limit at 99° rather than at 100°. But the higher limit is the one now most generally adopted, and the one therefore most useful for the comparison of results.

Returning to the consideration of the "normal" charts, there is one fact thereby demonstrated, namely, that the development of the physiological function of lactation proceeds without any rise whatever in the temperature. We are therefore able to insist on the following important principle, that if pyrexia arises during the days in which the

* By the kindness of the Editor of the 'Clinical Journal' I am enabled to select a few of the charts to accompany this paper.

function of lactation is being established, the cause *must* be pathological, not physiological.

If we examine the group of pyrexia cases we notice that a large proportion of them exhibit a rise of temperature on one day only, and very frequently only for a portion of a day.

Taking those cases where the rise of temperature is seen to occur within twenty-four hours of delivery, in some the pyrexia has begun during labour, in others some hours after. The latter, for want of more precise knowledge, we call "reaction" pyrexia. The former are instances of emotional or exhaustion pyrexia, the temperature falling to

pyrexia, and to fully admit the room there is for difference of opinion.

But granting this, can there be a more imperative duty on the part of the practitioner in attendance on a lying-in woman than to recognise the very beginning of fever, and with the least possible delay to diagnose the true cause. We are all very strongly biassed in every such case by the wish to prove to ourselves, as well as to the friends of the patient, that the case is not one of "puerperal fever," which to admit means (or should mean) notification, with perhaps some loss of prestige and practice.

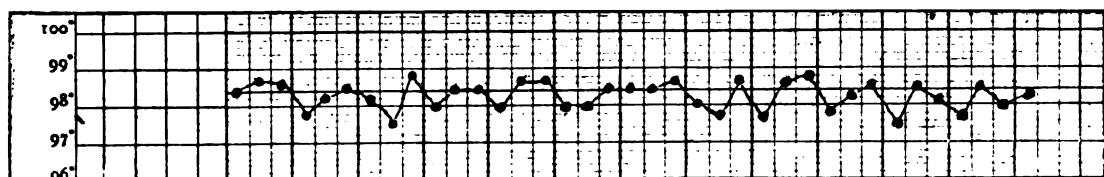


Chart 1.—H. G—, æt. 18. Primipara. Normal temperature chart.

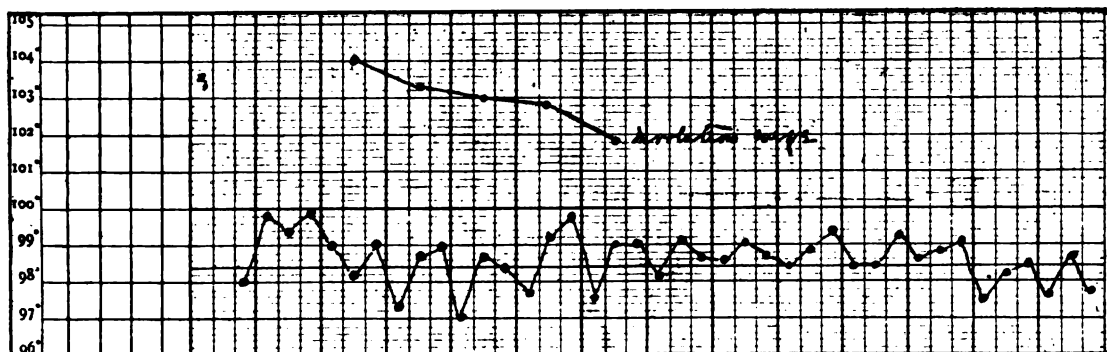


Chart 1a.—H. L—, æt. 20. Primipara. "Normal" chart, but variations too great.

normal, and remaining normal within a few hours of delivery. "Reaction" pyrexia is not accompanied by any increase in the pulse rate.

The same kind of temporary rise may occur on any subsequent day of the puerperium, and is attributable to emotional or other slight disturbance. If pyrexia recurs or continues, even to a slight degree only, for two or three days in succession, the cause is very unlikely to be emotional; for though the anxiety or trouble may continue, the pyrexia will not.

Before proceeding further I should like to refer to the difficulty which exists in some cases, of correctly and immediately assigning a cause to the

It is not to be wondered at that some find it perhaps easy to satisfy themselves that the case is one of influenza, typhoid, or pneumonia, or any disease rather than septicæmia. In a lying-in hospital such mistakes are doubly criminal, for if such concealment is allowed, it can only be with the approval of the seniors, with the result that every subordinate will think it to be to their interest to follow the example, and to conceal from their seniors matters for which they may think themselves responsible. And I know of nothing which so strongly compels every person who has a due sense of responsibility, whether doctor or nurse, to minute attention to systematic antiseptic pre-

cautions on which the safety of every lying-in woman to so large an extent depends, as the occurrence of, or even the threatening of, a case of septic puerperal fever.

We therefore make it the rule in the hospital that every case of doubtful fever is assumed to be of septic origin until we are able to prove that it is not so.

In private practice the reverse is very frequently adopted, namely, to assume that a case is not

in maintaining regular and sufficient relief of the bowels, even when they do not entirely neglect proper attention to these matters. Certainly attention has to be given to this point in a very large number of cases whether there is pyrexia or not. A severe case with intestinal colic is alarming as it somewhat simulates peritonitis; but the comparative softness of the abdomen, the varying position of the pain, with the other well-known differences in physical signs enable us to diagnose the

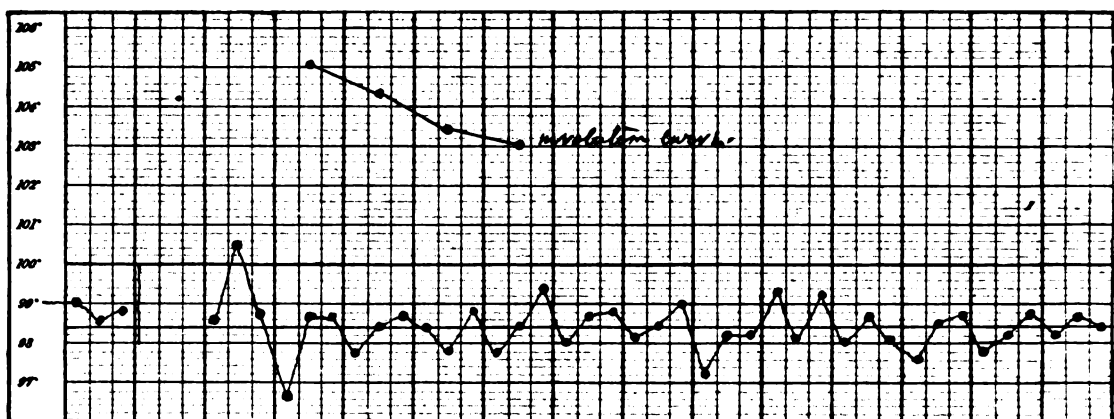


Chart 2.—Primipara. Reaction pyrexia, following delivery.

Days of
Puerperium.
1 2 3 1 2 3 1 2 3

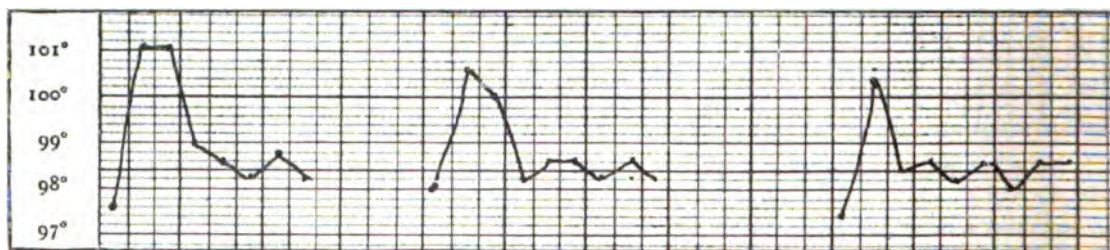


Chart 2a.—Three charts showing reactionary temperatures.

septic until it is no longer possible to do so, and with very injurious if not disastrous results to the patient; a wholly futile treatment being adopted, directed only to the relief of symptoms, while the disease is sometimes allowed to progress until it is too late to do anything effectual.

Another very common cause of pyrexia of comparatively slight severity is fecal accumulation. Most women during the later months of pregnancy find considerable and increasing difficulty

case, and to prescribe two or three grains of calomel, to be followed by small repeated doses of castor oil (with opium or belladonna if the pain is considerable), and if necessary to complete the evacuation with enemata of soap and water.

It is most important not to be tied by our customary notions of waiting until the third day to give the usual aperient, which is often needed much sooner.

The most serious case I have seen belonging to

this group was in my own practice, and was due to gastro-intestinal irritation of the most violent kind due to improper food.

It occurred in a young primipara whose labour and puerperium up to this point had been normal; on the fifth day she was given a quail on buttered toast, followed by stewed fruit and cream. Two or three hours after she had a violent rigor, the temperature rising to 105° F., with abdominal pain and vomiting. A dose of calomel and castor oil emptied the bowel, and after a few hours the temperature fell, and she made an uninterrupted convalescence.

mastitis, a connection which when first pointed out to me I was slow to admit. But of the fact I now have no doubt, and in every case of pyrexia with a blush on the breasts I would urge the importance of examining the interior of the uterus, where frequently we shall find some putrid clot or remains of placenta.

The removal of this, followed by thorough irrigation of the uterus with an efficient antiseptic, leads to a rapid disappearance of the so-called "breast symptoms," though the only direct treatment of the breasts has been the application of cotton wool or an antiseptic fomentation.

Tenth day.

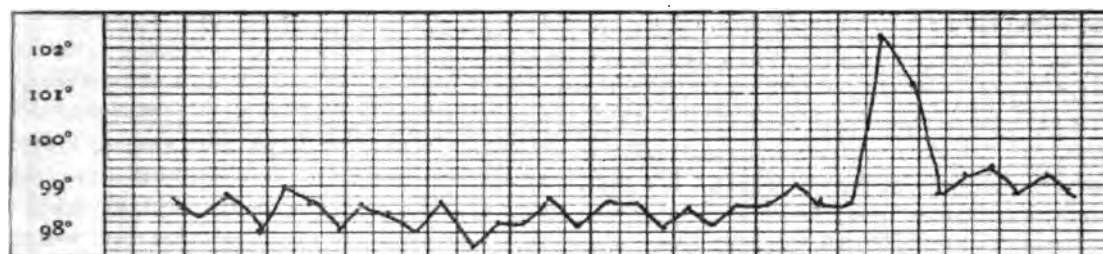


Chart 3.—Emotional pyrexia. Sudden rise of temperature on tenth day, for which no cause could be found except that patient on that day received a letter from home containing bad news.

Third day.

Third day.

Ninth day.



Chart 4.—Three cases in which the breasts became distended and painful. No other cause could be found for the rise of temperature.

Various conditions of the breasts sometimes give rise to pyrexia, though may I repeat once more that the *normal* onset of lactation is never the cause of pyrexia, and that if fever does arise during this process there is certainly a pathological cause to be determined. We must all have seen cases where much valuable time has been lost from attributing the fever to the "milk coming in," whereas the case was really a serious one of infection. There is a direct connection between septic conditions of the uterus and a form of

The common fissure or more extensive erosion of the nipple does not lead to pyrexia if the nipple is protected during suckling by a glass nipple-shield, and during the intervals is protected from direct infection by a large cyanide gauze dressing.

The breasts may be greatly distended without a rise of temperature, but cases are not rare when this does occur. The patient is found lying on her back in considerable distress, hardly able to move her arms because of the heavy, very tender, and distended breasts which hang over into the

axillæ. The infant is unable to draw the milk, and its ineffectual efforts only increase the pain and distension of the breasts.

The means I have found most useful for the relief of this condition are—gently to raise the breasts into the middle line, apply a pad of absorbent wool over each nipple, and then to firmly bandage the breasts with a soft elastic bandage applied in a figure of eight round the whole chest. In exceptional cases a Martin's rubber bandage is still more effectual. The bandage may be removed after a few hours, and the nurse having, if necessary, gently drawn out the nipples with the nipple-shield, the baby may be put to the breasts successfully. Drawing off the milk by any violent mechanical means is to be avoided.

True inflammation of the breasts is probably always septic in origin. Apart from mere differences in site, two classes of cases are to be noted: (1) the acute cases, in which all the characteristic features develop within a few days, terminating quickly in resolution or suppuration; (2) latent cases, in which, perhaps long after regular medical attendance has ceased, we are called to find an abscess already formed, and with so little evidence of its development that the patient has not thought it necessary to send for advice. These late unsuspected passive inflammations of the breast must be considered in connection with the same variety of inflammation in the pelvis, especially affecting the pelvic cellular tissue, and both are found to occur in cases in which during the lying-in the temperature is only slightly raised, oscillating between 99° F. and 101° F. (or a little higher) for several days, and without the formation of any definite lesion to explain it.

We shall do well in all such cases to keep our patients in bed, not only while the temperature presents such oscillation, but if possible for a good many days after.

Considerable distension of the bladder is a very common occurrence during the puerperium, and is easily overlooked unless care is taken to examine for it. This condition appears to be sometimes a cause of pyrexia.

Cystitis in women is most commonly due to infection conveyed by a septic catheter; the only certain way to prevent this is to use an aseptic catheter, to expose the orifice of the urethra, and

to sponge it with an antiseptic, so that the catheter may not be infected during its introduction.

The next group of cases are of very great importance, viz. the septic ones, in which the pelvic generative organs are the seat of infection.

They may be divided into three classes, the types of which are easily distinguishable from each other; these differences depending upon the properties of the different kinds of microbes as well as upon differences in the immunity of the individuals infected.

A piece of placenta or of membrane retained in the uterus, or a laceration of uterus, vagina, or vulva, is not *the* cause of puerperal sepsis, but each of those may be of great importance in facilitating it, the one by interfering with the proper involution of the uterus, and providing a suitable medium for the rapid development of microbes if introduced, the other forming a ready means of passage of such microbes from their comparatively harmless situation in the vagina to the lymph and blood channels, where their presence and rapid multiplication may give rise to a local inflammation, or to a general sepsis which may prove fatal.

The three groups of septic cases are (1) sapræmic; (2) cases of inflammation localised near the seat of infection, *i. e.* perimetritis, parametritis, salpingitis, &c.; (3) cases of true septicæmia.

The second group frequently arises from the first, especially if no effectual treatment has been adopted in good time.

It is not possible within the limits of this paper to discuss properly the importance of gonorrhœa as a cause of these pelvic inflammations.

I am sure that if men had some idea of the risk of permanent injury and invalidism to which their wives are exposed when infected by gonorrhœa, more care would be exercised by them, and there would be fewer cases of suppurating tube and ovaries.

It is to the first group, the cases of sapræmia, that I propose more particularly to refer; they are common, and are for the most part directly "curable" by treatment. We are not able to speak in such unqualified terms of our powers of direct "cure" in many other diseases.

It seems to be a fact that at Queen Charlotte's Hospital the proportion of cases with a raised tem-

perature is lowest when the rate of admission is low, namely, one or two cases a day, and higher when the pressure of cases and work is greater. This is not a matter of surprise, it would be greatly to the advantage of the Staff and patients if our antiquated and very insufficient accommodation were replaced by new buildings, designed and furnished in accordance with our present state of knowledge and modern requirements.

Let us now consider what are essentials for the prevention of sapræmia.

They are—

1. *Strict antiseptis.*
2. *The perfect closure of the uterine cavity.*
3. *The perfect closure of all lacerations.*

The proper discussion of this subject would alone occupy the whole evening, and that I may not occupy too much of the time available I will only briefly refer to each detail, and point out that it is beyond dispute that these same precautions which prove effectual against the occurrence of sapræmia, a disease uniformly curable, are also effectual against septicæmia, a disease usually incurable.

My experience tells me that two out of the three precautions are very generally neglected, not in slight and minute details, but (if I may use such terms) in the most gross and serious manner.

(1) To take the first precaution, *strict but rational antiseptis*, each word of which needs the most careful definition, for if the septic mortality and morbidity of labour and surgical measures of all kinds are to be reduced to the minimum, each practitioner must study this for himself, especially those, who like myself, or whose training was still earlier, were brought up to the full belief in the methods which provided us with poultices for dressings, waxed string or thread for ligatures and sutures, and plenty of laudable pus as the result.

Those of us who have had the great advantages of working with men who have been in various directions developing the great work of Lord Lister, have regarded with pleasure not only the results of the development of antiseptic methods, but at the same time their simplification; and while we cannot sometimes help smiling even in our admiration at the somewhat extravagant proceedings of some operators, we should look closely to our own methods to see that we do not omit some necessary detail which might improve our own results. But what are we to say now to

practitioners whose ideas of antiseptis are represented by the presence of a saucer of Condy's fluid under the patient's bed?

Twice have I been called in to find this the type of the precautionary antiseptic measures resorted to. On the other hand, I do not recollect ever meeting a man who, with extravagant notions of antiseptic methods, omitted the essentials.

As a teacher of mixed classes of monthly nurses and midwives, medical students and practitioners, I have unusual opportunities for studying their relative knowledge and ways of looking at things, and of sometimes giving a note of warning that I understand has been of use to some.

I tell the nurses that they have advantages, in being trained at the present time, very greatly in excess of their predecessors of even a few years, and indeed of some practitioners who have never had the opportunities they are having, and that it will happen to some of them, in the event of matters not going well with the patient, that the husband or friends may ask the direct question, "Is the doctor taking the same precautions as the nurse has been taught to look upon as essential for the patient's safety?"

All such questions put to a nurse are most unfair, and may place her in an exceedingly difficult position, for unless she can unhesitatingly answer yes there will be further questioning, and confidence in the doctor will be shaken. It is obviously necessary that the doctor should in every particular keep up to date with the training given to his nurse.

(2) *The complete closure of the uterine cavity* means something more than the complete removal of the placenta and membranes. There cannot be complete closure unless the placenta, amnion, and chorion are completely expelled; but, on the other hand, a considerable portion of placenta and membranes may be left behind, and be expelled spontaneously some days later without any complication whatever; not that I am prepared to recommend the practice of waiting for spontaneous expulsion of membranes and placenta, for if either hæmorrhage or sepsis do occur, it would not be possible to avoid the conclusions that such might have been prevented by proper evacuation of the uterus.

It must also be borne in mind that in natural labours the uterine decidua remains attached not

only over the placental site, but over considerable areas, especially over the lower third of the cavity, and will be cast off in minute or larger fragments with the lochia during the first few days of the puerperium.

To discuss the causation of incomplete closure of the uterus would occupy too much time, but the occurrence of an actual cavity in the uterus during the puerperium, from whatever cause, leads to an accumulation of the blood and other constituents of lochial discharge, which, even if this does not become infected and lead to sapræmia, pelvic inflammation, or even to septicæmia, will inevitably interfere with the proper involution of the uterus, leaving it large and soft, and easily displaceable, instead of small, rigid, and in natural position. The latter effects of this subinvolution I need not here refer to.

The indications of incomplete closure of the uterine cavity are easily recognised, and such cases are not uncommon.

(a) The lochia, which should cease to consist principally of blood on the third or fourth day of the puerperium, is found to be profuse, and to contain clots. This, whether the discharge is foul or not, is a certain indication, and equally reliable at any period of the puerperium.

(b) The presence of severe (not slight) after-pains, necessitating something more than a little bromide to relieve them.

(c) The uterus is found to be more bulky than it should be; and if there is a piece of placenta left adherent, that part of the uterus will often be found to be distinctly tender, while the remainder is not so.

I need only mention here the necessity for making sure that the bladder is empty in every such case. A full bladder during the first twenty-four hours is a frequent cause of imperfect contraction of the uterus and of profuse lochia, and distension of the bladder itself displaces and conceals the enlarged uterus in the abdomen, which might otherwise have been easily discovered.

The importance of imperfect closure of the uterus in relation to puerperal pyrexia lies in the fact that if the condition is recognised early, as it should be, and the case is promptly and effectually treated, the severer symptoms and a tedious and imperfect convalescence will be avoided.

Before I discuss the third factor, *the perfect closure of all lacerations*, may I refer to a matter of the greatest practical importance, namely, the foetidity of the discharges in relation to diagnosis? We meet with three classes of cases: (1) fatal septicæmia without foetidity of lochia; (2) foetidity of lochia without evidence of sapræmia, there is not even pyrexia; (3) foetidity of lochia with evident sapræmia or septicæmia.

Now is there any likelihood of error in what appears to be the very simple matter, viz. that of determining whether the discharges are foetid or not?

If the discharge on a recently removed diaper is putrid, even in a slight degree, there is no doubt whatever about the matter; if it is not, and vaginal douches are being used, we must not assume that the contents of the uterus are not putrid, for it frequently happens that foetid lochia during its passage through the vagina is partially disinfected by the antiseptic vaginal douche, some of which is very likely to remain in the vagina for some considerable time after the douche has been used. We can, therefore, only make sure by examining the interior of the uterus with the finger, taking care that the vagina is well irrigated first, so that no infected matter may be carried into the uterus.

A detail of importance for ourselves before making any vaginal or rectal examination is to see that the hand as well as the finger is well saturated with soap and water, or with an antiseptic, for in this way a series of examinations can be made without the finger becoming offensive, otherwise the discharges, even when not foetid, soak into the dry surface of the skin, and we all know the difficulty of deodorising and disinfecting the finger under such circumstances.

Laceration of the parts which have to be dilated during labour to permit the passage of the foetus forms a subject sufficient to occupy the whole evening. I will only refer to a few very practical points, the most important of which is the great frequency with which lacerations, even where extensive and external, are overlooked, or if recognised are entirely neglected. Their immediate or later results are sometimes serious.

I have seen a patient die from hæmorrhage from a laceration of small extent of one nymphæ, and I have seen fatal septicæmia in cases in which slough-

ing has followed extensive perineal and vaginal lacerations; a careful examination after death failed to show any evidence of infection of or through the uterus.

There is one reason why some practitioners object to discover lacerations, and indeed in some instances take steps to conceal their occurrence, namely, the fear that the patient or her friends should attribute them to want of skill or want of care on their part.

This idea is very prevalent amongst all classes, and we frequently hear some such remark as "Dr. — attended my friend Mrs. — with all her babies, and he never had to stitch her."

I am satisfied that the most experienced and careful practitioners are unable to prevent the occurrence of many lacerations, and, from no inconsiderable opportunities for observation, that much more injury from bruising and internal laceration follows too persistent efforts to keep back the head and support the perinæum.

I am also satisfied that blame rather attaches to the man who fails to recognise a laceration, or having recognised it omits to close it effectually.

There is no difficulty in doing this if we employ right methods, and there should be no failure in the results; failure means imperfect methods, and should not recur frequently in any one's practice. Having failed myself I can speak on this point without hesitation. But we need not delay to remedy matters when we find we have failed, for there is still no great difficulty in obtaining complete union even as late as ten or twelve days after delivery, and thus save the patient from the necessity of a later operation and more lying up at a subsequent period.

To illustrate this I may be allowed to refer to one of my own cases, the most serious and unpromising one for operation. A. B—, a primipara, whom I saw with her medical attendant on account of post-partum hæmorrhage occurring on the eighth day. I found the uterus nearly to the navel, and the right half of it tender; there were clots in the lochia, which was slightly offensive; the temperature had been raised for the last two days, but not above 101° F.; the perinæum was torn through into the bowel, the tear also extending a considerable distance up the left side of the posterior vaginal wall. The whole torn surface was sloughy, and beginning to discharge pus.

There was also the complication of albuminuria, eclampsia having occurred at delivery.

The patient was placed under chloroform, the vagina was disinfected thoroughly with cotton sponges soaked in biniodide of mercury 1 to 2000, and the sloughy surfaces scraped with a blunt scalpel or removed with scissors.

I then proceeded to empty the uterus of a considerable quantity of offensive clots and membrane, and washed it out thoroughly and sutured the whole length of the lacerated perinæum and vaginal wall. I saw her again seven days later and removed the sutures, the whole was healed without suppuration of a single stitch, the temperature had fallen to normal, and the uterus had shrunk to its normal size.

I have had many other successful results where lacerations have been closed as late as this, though not under the same serious conditions as in the above case.

I would point out the great advantage of placing the patient on her back and in the lithotomy position, both to examine for lacerations and for suturing them. The suturing can be done without difficulty with the assistance of the nurse if a friend gives the chloroform; this of course is quite unnecessary for merely making the examination. I rarely use more than three or four sutures however extensively the perinæum be torn, and have quite given up separate suturing of either the vaginal or rectal mucous membrane. If the sphincter or rectal wall is torn through, I prefer to wait twenty-four hours to enable the bowels to be well cleared with castor oil and enemata; after the operation the bowel should be cleared each morning by an oil enema, so that there is no accumulation.

Lacerations of the labia or of the vagina I generally close with a continuous suture.

Lacerations of the cervix, unless very extensive, I leave alone, but they are not difficult to close if it is thought necessary.

Cases of sapræmia may be divided into two classes: (1) the acute ones, which almost without warning commence with a sudden rigor, the temperature running quickly up to 103° to 105° ; (2) the more common and much less acute cases, which begin about the third or fifth day, according to circumstances, the temperature oscillating at points between 98° and 100° , later to 101° or 102° .

The treatment of both classes of cases is identical, and it is imperative that it should be carried out without delay. The principles of such treatment are to empty and thoroughly wash out the uterus and vagina with an efficient antiseptic, and to close any considerable laceration by sutures.

The procedure is as follows:

A vaginal douche is given, and an anæsthetic if this does not entail undue delay; for though anæ-

sthesia is both safer and better to remove as much as possible by the fingers than to use a curette or any other instrument for the purpose.

The curette opens up the orifices of the great vessels of the placental site, and profuse hæmorrhage may follow, which it is not always easy to check except by plugging the uterus.

In the rare cases of purulent endometritis following labour (I have only seen two such cases)

Third day.

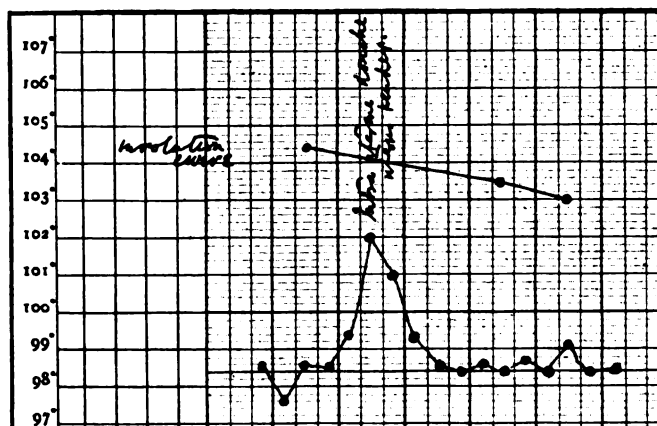


Chart 5.—Sapræmia effectively treated. E. G.—, æt. 20. Primipara. Foul lochia on third day, uterus washed out, foul shreds removed.

Thirteenth day.

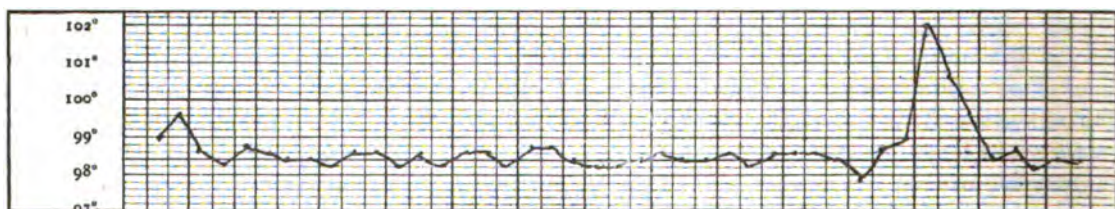


Chart 6.—Late one day pyrexia. Late sapræmia, thirteenth day, cured by intra-uterine douche.

thesia is distinctly advantageous, it is not essential, many patients bearing the necessary manipulations with very little complaint of pain.

The hand being thoroughly cleansed and disinfected, two fingers are passed into the uterus, the other hand on the abdomen depressing it sufficiently to enable the fingers to reach the fundus, without which the examination is imperfect. You can easily recognise blood-clot, pieces of spongy placenta, and partially detached membrane, and remove them; and I am strongly of opinion that

in which the pus pours from the cervix as out of a large abscess cavity, the very careful use of the irrigating curette is essential.

The uterus having been emptied, it is thoroughly douched with two or three pints of water at 115° F., containing 3ij of Tr. Iodi. to the pint, or other efficient antiseptic. During the douches the left hand grasps the body of the uterus, and gently but firmly kneads it; and this, with the stimulus of the hot douche, causes firm uterine contraction.

The effect of this treatment, if the case is not

one of septicæmia, is very satisfactory; and if it has been thoroughly done, no further treatment of this kind will be needed, and I very rarely wash out a uterus twice under such circumstances, and I have quite given up the introduction of iodoform afterwards.

The injection of serum is quite unnecessary in these cases. The use of uterine irrigation alone, without removal of the putrid fragments of placenta and membrane, is insufficient; vaginal irrigation is still less valuable. And what can we say in favour of quinine, morphia, antipyrin, and all such drugs as the sole treatment? though as subordinate measures they are of some value.

It is necessary to point out the usual course of events after the treatment above recommended has been carried out.

If the discharges become offensive after clearing and irrigating the uterus it is doubtful if it has been thoroughly done, and we should at least wash it out again.

In the cases previously referred to, in which a blush over the breast accompanies symptoms of sapræmia, the effects of treatment are as marked in the breast as in the uterus, though only a protective cyanide gauze fomentation has been applied locally.

It should not be forgotten that in every case, after exploring, emptying, and irrigating the uterus and vagina, all considerable lacerations should be closed immediately by suture, as I have previously insisted on.

I have been called to cases where the former treatment has been done thoroughly and the latter

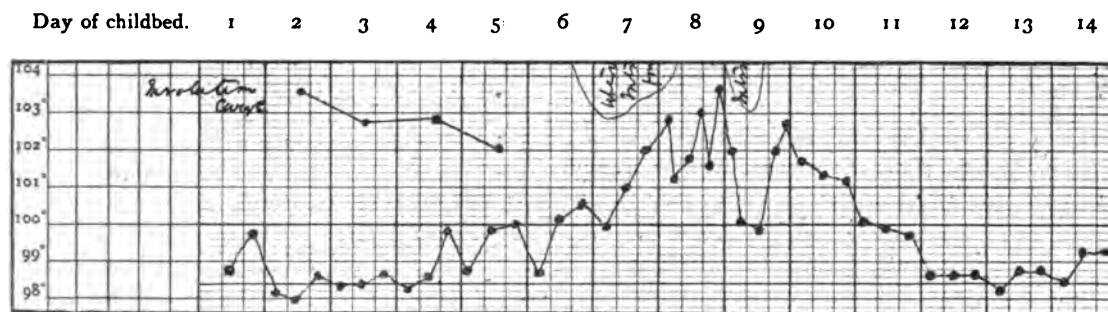


Chart 7.—Sapræmia, usual type. E. C—, æt. 23. Primipara. Torn perinæum; offensive lochia; tender uterus; sapræmia symptoms late. On the seventh day uterus tender; intra-uterine douche; foul clot removed. On the ninth day intra-uterine douche.

In the acute cases—namely, those in which the symptoms are acute and develop suddenly—the usual result of such treatment is a rapid fall of temperature, and little if any further trouble (see Chart).

But in the less acute cases, or in those in which two or three days have passed before the treatment has been carried out, there is no such rapid fall, and sometimes within a few hours the temperature may run up to 105° F., with a violent and alarming rigor and some collapse. I have not seen any unfavourable result under these circumstances. The greatest relief is afforded by tepid sponging and some hot stimulant containing a little opium.

The temperature usually falls steadily at about the rate of one degree each hour, but several days may elapse before the temperature settles to a constant normal.

omitted, to ascertain why the pyrexia continued, and suturing a tear has been not only followed by union, but also by a marked and permanent fall of temperature.

I am afraid I must omit the consideration of the group of cases of local pelvic inflammation and of septicæmia, these forming a subject too extensive to be discussed here.

Lastly, there is a group of cases of pyrexia which occur occasionally and complicate the diagnosis. They are cases of pyrexia due to causes quite apart from pregnancy, labour, and the puerperium, and are only accidentally associated with these.

I refer to such diseases as tubercle, influenza, the specific fevers, and many others.

CHAPTERS FROM THE TEACHING OF DR. G. V. POORE.

No. XV.

GENTLEMEN,—Chronic arsenic poisoning is very apt to occur in workers in arsenic. Persons employed in these trades are very apt to get eczematous eruptions on various parts of the body, especially spots where arsenical particles would be likely to stick, such as between the scrotum and the thigh, in the axilla, and very often around the conjunctiva. Conjunctivitis is not uncommon in those who take arsenic by the stomach only. Conjunctivitis has been observed in cases of both acute and chronic arsenical poisoning. Therefore we must infer that conjunctivitis is not solely due to the sticking of arsenical particles on to the conjunctiva, but to the presence of arsenic in the tissues, or in the secretion of the lachrymal glands or conjunctiva.

There is another point which is of interest in connection with chronic arsenical poisoning, namely, how it is brought about. I have mentioned before that cases are due to arsenical wall-papers, arsenical socks, arsenical dresses, and so forth. Now it has lately been shown that certain moulds growing upon damp wall-papers, such as *Penicillium* and other common moulds, produce a metabolism in the sub-stratum in which they grow, and eliminate arsenical gas. The arsenic enters into the composition of these moulds, and they give off an arsenical gas. That is a matter of very great interest, and opens up a vista of possibilities. It has been observed that an arsenical liver put away for a time gradually loses its arsenic, which disappears presumably in the form of gas.

Now it is said that animal matters impregnated with arsenic and allowed to rot sometimes give an arsenical dripping, and it is said that the aqua tophana was prepared by rubbing arsenic into pigs and collecting the fat, which distilled partly perhaps as a result of heat, and partly as a result of putrefaction.

There are two other conditions observed in chronic arsenical poisoning. One is pigmentation of the skin; the skin gets very brown, and we often have in the hospital cases of this kind in people who have taken arsenic for a long time.

Another trouble is neuritis. Many metals cause peripheral neuritis, arsenic amongst others. It is very common for arsenical workers to complain first of all of numbness, formication, and queer sensations about the limbs, and subsequently to complain of either paresis of the muscles or definite paralysis of groups of muscles, owing to the inflammation of certain nerve branches. A subject of chronic arsenical poisoning often gets pain behind the eyes, he gets dyspeptic, and looks thin and worn. With regard to arsenical poisoning, I think I am right in saying that in the East arsenic has been taken as an aphrodisiac. In the work on Indian Medical Jurisprudence by Chevers, there are allusions to this fact.

There is no poison which has caused so much discussion as arsenic. It has been used for criminal purposes again and again, and there have been several cases of arsenic poisoning which have been worked out with very great care by a number of experts. First of all I will put you in possession of a case of acute arsenic poisoning which came before the law courts in Edinburgh in 1857—the noted case of Madeline Smith. I have been through this case, and have selected certain facts; and I think all the facts which I shall bring before you are worthy of your attention as medical jurists, though they may not be strictly medical facts. Madeline Smith was put upon her trial before the High Court of Justiciary in Edinburgh, charged with the murder of her quondam lover, one L'Angelier, a Frenchman. The indictment charged her with two attempts at murder: (1) on Thursday or Friday, the 19th or 20th of February, 1857, and (2) on the 22nd or 23rd of February (Sunday or Monday), and with actual murder on the 22nd or 23rd of March (Sunday or Monday), and that all three attempts (two successful and one unsuccessful) were made by the administration of arsenic or other poison in cocoa or coffee, or some other article of food. It was never doubted by either side that L'Angelier met his death by poisoning with arsenic; but, there being several missing links in the chain of evidence, the trial ended in a verdict of "Not proven," and the prisoner was discharged.

We have no verdict of not proven in England, and I take it the difference between not guilty and not proven is that if a jury returns a verdict of not proven, the prisoner may be brought up again on

the charge and re-tried when additional evidence is procured. But if a jury brings in a verdict of not guilty, I believe the prisoner cannot be tried again upon that specific indictment. But in England we have a different method of proceeding. Before a case comes into court it is heard in a preliminary way by the grand jury; the grand jury listen to the facts, and they then either find a true bill or ignore the bill against the prisoner. When the grand jury ignores a bill, that is very similar to the Scotch "Not proven"—that is to say, the prisoner may be again charged upon the same indictment when the evidence is sufficient to convict him.

Madeline became engaged to another man, and the theory of the prosecution was, that in order to be rid of L'Angelier (and having failed to do so or to recover her letters in ordinary ways), she determined to poison him with arsenic. It was proved that Madeline had purchased arsenic on several occasions shortly before L'Angelier's illness and death. A packet of cocoa was found in Madeline's bedroom, which was on the ground floor, and it was sought to establish the fact that Madeline handed L'Angelier his fatal draught from the window of her bedroom about ten or eleven o'clock on the evening preceding his death. Madeline's father would not countenance the addresses of L'Angelier to his daughter. All their meetings were clandestine, and he was frequently in the habit of tapping at her bedroom window at night-time, and holding conversation with her. You never can neglect in any of these cases the moral circumstances; and though they may not be precisely medical circumstances, they are most important, and give colour to a case. But moral circumstances of that kind, it always seems to me, cut in two directions. They show that there was reason for wishing to get rid of a certain person, and they give importance to facts which otherwise would be of no importance. On the other hand, you must remember that moral circumstances of this kind might give importance to facts which really ought to be neglected. Moral circumstances generally make against the prisoner, and sometimes unjustly so. "Trifles light as air are, to the jealous, confirmation strong as proofs of Holy Writ."

Now there is the evidence at the trial. The lodging-house woman with whom L'Angelier lived

gave the following account of his illnesses and death. "One night about the middle of February he wished for a pass-key, as he thought he would be out late. I went to bed, and did not hear him come in. I knocked at his door about eight in the morning and got no answer. I knocked again, and was answered 'Come in, if you please.' I went in, and he said 'I have been very unwell; look what I have vomited.' It was a greenish substance; there was a great deal of it. It was thick like gruel. He said that while on the road coming home he was seized with a violent pain in the bowels and stomach, and when he was taking off his clothes he thought he must have died upon the carpet. He was not able, he said, to ring the bell. He went to business (he was a clerk in a merchant's office) that day. He complained of thirst repeatedly during the day. The illness made a great change in his appearance. He looked yellow and dull. Before that his complexion was fresh. He complained of feeling cold."

"On the 23rd of February he had a similar illness, and the witness was called to him about four in the morning. He was vomiting the same kind of stuff as before. He complained much of cold and also of being very thirsty, and he took a quantity of cooling drinks. There was much purging as well as vomiting on both these occasions. At this time he was unable to attend business for a week. He continued very unwell, and was obliged to go into the country for his health."

"On March 1st he returned from the country in better health. He went out that night about nine, asking for the pass-key before he went as he might be late. It was about half past two in the morning when I next saw him. He did not use the pass-key in coming in, but rang the bell with great violence. I rose and asked who was there, and Mr. L'Angelier answered 'I am very bad; I am going to have another vomiting of that bile.' He said he thought he would never have got home, he was so bad on the road. The first thing he asked for was a little water. He began to undress, and then to vomit severely. The vomiting was attended with great pain. He was chilly and cold, and wished a jar of hot water to his feet and another to his stomach. About five o'clock he was much worse, and his bowels became bad. A doctor was

sent for, but did not come [mark how dangerous it is not to obey a medical call]. At seven o'clock he was worse still. He was dark about the eyes. The doctor was sent for again, and came. I took the doctor into the dining-room and asked what was wrong with him, and he asked whether he was a person who tumbled? At nine he was still worse, and he asked that one of his friends might be sent for. He said that if he could get five minutes' sleep he thought he would be better. These were the last words I heard him use. I came back to the room in about five minutes; he was then quiet, and I thought he was asleep. The doctor shortly returned, and discovered he was dead."

Post mortem the body presented nothing extraordinary externally, but a "tawny hue of the surface." The stomach being tied at both extremities was removed from the body. Its contents, consisting of about half a pint of dark fluid resembling coffee, were poured into a clean bottle, and the organ itself was opened along its great curvature. The mucous membrane, except for a slight extent at the lesser curvature, was then seen to be deeply injected with blood, presenting an appearance of dark red mottling, its substance being easily torn by scratching with the finger nail. The appearance of the stomach being suspicious, it was reserved for analysis, and the authorities were informed of the suspicious circumstances. The body was buried, but on the 31st March it was exhumed for further examination. It was observed that the features had lost their pinched appearance, and that the surface of the skin was no longer tawny, but had become florid, "and," the report says, "we all agreed that the evidences of putrefaction were much less marked than they usually are at such a date, the ninth day after death, and the fifth day after burial." Portions of the large and small intestine (the portions being previously ligatured to preserve the contents) were removed, together with some of the liver and brain. The mucous membrane of the duodenum and part of ileum was found redder than natural, and this redness was more marked over several patches, portions of which, when carefully examined, were found to be corroded. Several small whitish and somewhat gritty particles were removed from its surface. A few small ulcers about one sixteenth of an inch in diameter, and having elevated edges, were observed at the upper

part of the duodenum. In the rectum were found two vascular patches about the size of a shilling. The fluid contents of these portions of intestine were poured into a glass vessel, on decanting which there were observed a number of crystals adhering to the sides of the vessel, and at the bottom a whitish sediment. Now, one of L'Angelier's medical attendants gave evidence, and in doing so he said "there was an appearance of jaundice. I have heard of that as a symptom of irritant poison. It is in Dr. Taylor's work on poison."

Counsel for prisoner: "Show me the passage in Dr. Taylor's work" (handing it to witness).

Doctor: "I cannot find the particular passage. It is in the case of Marshall."

Counsel: "What was the poison in the case of Marshall?"

Doctor: "Arsenic."

Counsel: "Well, see if you can find it."

He could not, and you will remember what I said about quoting authorities. The cross-examining counsel was down upon him at once.

Dr. Penny, professor of chemistry in the Andersonian University, Glasgow, made an examination of the viscera and their contents, the most important fact in connection with which was he found $82\frac{7}{10}$ grains, or very nearly one fifth of an ounce, of white arsenic in the man's inside. In concluding his report, he said, "having carefully considered the results of this investigation, I am clearly of opinion (1) that the matters subjected to examination contained arsenic; (2) that the quantity of arsenic found was considerably more than sufficient to destroy life."

In this case of Madeline Smith there was another point which came out at the trial; the arsenic which Madeline Smith was proved to have purchased was mixed (as the Act directs) in one case with soot, and in the other case with indigo (about 5 per cent. in each). The question arose as to whether an untutored girl could, by manipulating this mixture, get rid of the soot and indigo from the arsenic. Now if Madeline Smith got her verdict of not proven by one circumstance more than another, it was that neither soot nor indigo were found in the intestines, but arsenic was found; and it is possible that by throwing the mixture into a large quantity of water the arsenic would fall to the bottom, and the soot float on the top. The question arose as to whether a girl could do this.

The next point was that the defence set up a plea (which has often been set up) that she used arsenic as a cosmetic. I see at the railway stations illustrations of a girl with a marvellous complexion through using somebody's arsenical soap; I mention that to show that arsenic has a great reputation as a cosmetic. It is given largely for skin diseases, and is used by grooms for improving horses' coats. I think there can be no doubt that it does improve horses' coats. It was said that Madeline Smith bought the arsenic to use as a face wash. Then the question arose as to whether the girl could wash her face in an arsenical solution without getting any harm from the proceeding; whether the arsenic would not get into the eyes, and cause irritation and trouble. Be that as it may, a defence of that kind manipulated by a clever counsel is good enough for a jury.

Then the question of the solubility of arsenic came up, and it was shown that this arsenic had been administered in a thick liquid, namely, cocoa or chocolate. It was in this trial that Dr. (afterwards Sir R.) Christison, the great toxicologist, the expert for the defence, deposed that the appearances found in the intestines were such as might be expected in cases of arsenical poisoning. He adds, "I think all the symptoms in the case might have occurred from malignant cholera; the ordinary time that elapses between the administration of arsenic and death is from eighteen hours to two and a half days. The shortest time is two or two and a half hours." Then he said a very ingenuous thing. He said, "The dose was probably more than double the amount found in the stomach. In the greater proportion of cases of suicide the dose is generally found to be large. *It is in cases of suicide that double-shot pistols are used and large doses given.*" The cosmetic theory, the absence of soot or indigo, and the theory of suicide helped to bring about a "not proven" verdict. Dr. Christison was recalled and examined as to the use of arsenic as a cosmetic. He thought it would be very unsafe to put arsenic into a basin of water and wash the face in it; it might get into the nostrils, the eyes, and the mouth, and cause inflammation. He contended (with all respect to Orfila, who states the contrary) that arsenic has no taste. There could be no doubt that large quantities of arsenic have been swallowed by persons without their noticing it. Some have

said it had an *acid* taste. This was probably an error, the acrimony being confounded (1) with the roughness of the gritty particles in the mouth; and (2) the burning, &c., slowly developed by the poison afterwards. The word *acid* is a professional phrase, but Orfila used the word "*apre*," which means rather *rough*.

Counsel:—"Yes, in his first volume, page 377, he does use the word, but on page 357 of the same volume you will find he says the taste is *acre et corrosive*."

Dr. Christison had come to the conclusion that arsenic had no taste from experiments made on himself. Orfila does not say that he made similar experiments. There you get the danger of quoting authorities. Inglis, the defending counsel, was a man of gigantic ability, and tremendous memory. That is a very instructive case, and it is instructive on account of the many medical questions which first came up at it.

ESSENTIAL RENAL HÆMATURIA.

DEBAIRIEUX ('Annales des Société Belge de Chir. ; Annales des Organes Génito-Urin.,' No. 9, 1898), stimulated thereto by a case of rebellious hæmaturia dependent upon a chronic unilateral nephritis reported by Keersmaecker, contributes the interesting details of a woman twenty years old who in April of 1895 presented herself with a history of having suffered from hæmaturia for six months. This began with an attack of grippe, and was accompanied by pain in the left side of the belly, the iliac fossa, the lumbar region. This pain was habitual, though not constant, and was sometimes severe enough to prevent sleep. Occasionally it presented the features of true nephritic colic, running along the course of the ureter toward the labia majora, being accompanied by nausea and vomiting. At times this was so intense as to cause the patient to faint.

There were not the classical symptoms of stone, there were no pus cells nor tube casts in the urine, nor crystals of any kind. The reaction was faintly acid, and the albumen was exactly proportionate to the quantity of blood. The urine was sterile; there was no polyuria; occasionally perfectly limpid urine would be passed—once it remained so for two days.

The renal origin of the blood was decided upon because of the absence of all vesical symptoms and the presence of pain and tenderness in the region of the kidney. The urine passed in three glasses showed an equal quantity of blood in each. On irrigating the bladder the fluid returned without trace of blood. After having irrigated the bladder and removed the catheter, bloody urine could be drawn five minutes after.

Diagnosis rested between neoplasm and calculus. It was decidedly in favour of the latter. On operation the kidney was found normal in appearance, dimensions, and position. The kidney was split, and nothing abnormal was found. The kidney was sewed, and the wound closed. From that time bleeding ceased, and the patient has been perfectly well ever since.

The second case bled for five months, and was opened by Broca. The kidney was found healthy, and was sutured in position. The bleeding ceased immediately.

The third case, having suffered in 1878 from symptoms of nephritis, passed bloody urine from that epoch until 1886, when Sabbatier, believing a calculus was present, practised nephrectomy. The kidney removed was perfectly healthy. Hæmaturia ceased after operation, and the patient was well more than a year later.

Another patient suffered from hæmaturia in December of 1887. This was followed by apparent cure, but in 1889 the hæmaturia recurred, and was almost continuous. Because of the history Senator diagnosed hæmophilia; Nitze demonstrated that the blood came from the right ureter. In 1890 Sonnenberg practised nephrectomy. Histological examination showed only very small isolated portions of the kidney affected with interstitial nephritis. Hæmorrhage ceased after the second day following operation. Seven months later the patient was still well.

These four cases were all women.

The fifth case, that of a man aged 50, suffered from abundant hæmaturia after taking a cold drink; at first intermittent and relieved by rest, it finally became continuous and exhausting. A slight pain in the left side furnished the indications for operation. Nephrectomy was practised. The left kidney, though anæmic, was perfectly healthy. The patient got well.

The sixth patient was a sailor, who suffered over

twelve years from hæmaturia associated with pain in the left kidney. Lauenstein performed pyelotomy, hoping to find a stone; but in this he was disappointed. The patient recovered, and had no recurrence of bleeding.

The seventh case suffered over five years from nephritic colic of the right side and hæmaturia. For two years it had become continuous. Abbe practised nephrotomy, introduced his finger into the pelvis, and found only the extremity of one pyramid covered by a light gritty deposit, which was scraped away. After the fourth day hæmaturia ceased, and never recurred. Pain was also relieved.

Many other cases are reported of both nephrotomy and nephrectomy, all failing to show any adequate lesion in the kidney.

The bleeding seems to be dependent upon an obscure anatomical condition, one which cannot even be detected by minute microscopic study of sections of the kidney substance. The condition has been termed essential renal hæmaturia or renal hæmophilia. The characteristic of this form of hæmaturia is that it is associated with no demonstrable anatomical lesion, that it is refractory to all medication, and that it disappears either spontaneously or as a sequel of an operation, the efficacy of which cannot be clearly explained.

The theory that the bleeding is due to an angio-neurosis finds its support in the fact that this condition is much more frequent in women than in men; also operations other than those performed on the kidneys are followed by a cure of the affection. Thus Passet and Picquet, having through a mistaken diagnosis performed hypogastric cystotomy, cured the patient.

As to the treatment of this affection, nephrotomy is the operation of choice, followed by nephrectomy if the kidney is markedly diseased.

Therapeutic Gazette, January, 1899.

AN extremely useful preparation of carbolic acid, which will greatly facilitate its use as an antiseptic, has just been brought out by Burroughs, Wellcome & Co. in the form of a "soloid." Sixty grains of carbolic acid are contained in each "soloid," enabling the practitioner to have ready for immediate use the means of at once preparing an antiseptic solution of any desired strength, at whatever moment and in whatever place its prompt application may be desirable.

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SPLENIC ENLARGEMENTS: SPLENITIS AND PERISPLENITIS.

A Clinical Lecture delivered to the London Post-Graduate Class at the Central London Sick Asylum, December 1st, 1898,

By JAMES CANTLIE, M.B., F.R.C.S.

GENTLEMEN,—The patient before us is a woman, æt. 41, who has been an inmate of the hospital since March of this year. Her appearance at the present moment indicates anæmia of a marked character, and the distension of her abdomen with ascitic fluid, combined with the enlarged and tortuous veins over the abdominal wall, suggest visceral disease. Percussion and palpation of the abdomen reveal a contracted liver and a big spleen. Auscultation shows the presence of a systolic bruit over the apex of the heart, and the thrill felt at the pulse is indicative of the state of the circulatory disturbance. The woman confesses to a free use of alcohol, and it seems an easy conclusion to come to that the patient is suffering from cirrhosis of the liver and subsequent enlargement of the spleen due to alcoholic causes.

In dealing with this very ordinary group of clinical signs and symptoms I propose to consider the following points:

1. The normal and pathological relative anatomy of the spleen.
2. The association between enlarged spleen and hepatic disease.
3. The anatomy and meaning of distended surface veins on the abdomen.
4. Perisplenitis, its frequency and anatomical consequences.
5. The treatment of splenic enlargements.

1. *The normal and pathological relative anatomy of the spleen.*—It is the rule when the spleen is being examined to have the patient lying on the back. In this position by palpation and percussion it is no doubt quite easy to find the spleen when it is greatly enlarged; but when the organ is

of normal size, or even moderately big, it is impossible to determine, even approximately, while the patient is supine, its exact position or actual size. We are accustomed to regard the spleen clinically as being an organ presenting relations with the anterior aspect of the abdomen, but a very superficial study of the splenic relations will show that this is a clinical fallacy. The spleen is situated far back in the hollow of the ribs; it is accommodated in the angles of the ninth, tenth, and eleventh ribs. The long axis of the spleen runs parallel with the tenth,—"the splenic rib,"—and the inner (upper) end of the organ reaches to the head of the rib. The outer (lower) end of the spleen comes no farther forward than the level of the posterior axillary line, so that to attempt to percuss out the spleen from the front with the patient supine is, in the case of the normal spleen, an absurdity. Such being the case, it is necessary to enquire in which position is it expedient to place the patient so as to determine the exact limits of the spleen. With the patient in the horizontal position and rolled over on the right side, equally fallacious signs of splenic relations will result as the organ moves forwards with the altered position of the body, rendering correct location impossible. Again, with the patient prone it is impossible to percuss out the spleen from the back, as the whole organ leaves its position (unless adherent owing to old perisplenitis) and rolls forwards to the anterior part of the abdomen. It is therefore only when the body is in the erect position that the spleen can be percussed out properly, for the organ is maintained in its normal position only when the body is so disposed. The spleen does not descend when the erect or sitting position is assumed, as will be readily understood from a study of its anatomical relations.

Relations.—The spleen lies on the cardiac curve of the stomach, inclined towards its posterior and upper part. Externally and above is the diaphragm. Below the spleen is the pleuro-colic fold of peritoneum presenting a cup-shaped pocket for its support. So exactly does the cup fit the spleen that it is impossible for the organ, when the erect position is assumed, to sink downwards; even when considerable enlargement is present this pleuro-colic fold supports the spleen, and renders enlargement downwards impossible. The only direction along which the spleen can travel

during change of position is forwards; and not only so, but it is only in a forward direction that the spleen can expand when it is enlarged. It cannot enlarge downwards until the supporting power of the pleuro-colic fold is overcome; hence it is only when the enlargement is great that the spleen passes downwards towards the left flank. In all splenic enlargements the organ primarily expands forwards. As the growth increases the spleen speedily crosses the middle line of the body, but after a time, and owing to a continued increase—not to a diminution—in size it recedes from the right of the middle line of the body, and falls more towards the left flank. The sole determining cause for these relations is the pleuro-colic fold. At first the power of the fold is sufficient to maintain the spleen even when enlargement is great, but after a time the spleen comes so far forwards that it overlaps the anterior border of the fold, and pushing it back drops down by its own weight towards the left iliac fossa. In the patient under consideration this very circumstance took place. Some two months ago the spleen crossed the middle line of the abdomen, but now it falls short of the middle line by some four inches, not because the organ has diminished, but because it has increased in size, and by its increasing weight overcame the supporting power of the pleuro-colic fold, and dropping down towards the left side receded from the middle line of the body. One therefore must not assume that a recession of the spleen from the middle line always means a decrease in size, for it is evident the opposite condition may obtain. I would advise you, therefore, when percussing out the normal spleen, or even when you suspect enlargement, to do so with the body in the erect or sitting position; also to search for early enlargements of the spleen high up in the abdomen in the epigastric or umbilical region, or, at any rate, above the umbilicus. You will only find the spleen in the left lumbar or iliac regions when the organ has attained large proportions and receded from the middle line of the abdomen, owing to the inefficiency of the pleuro-colic fold of peritoneum.

II. *The association between enlarged spleen and hepatic disease.*—It is regarded well-nigh as an axiom that a cirrhosis, tumour, abscess, or allied disease of the liver necessarily involves a corresponding and consequent enlargement of the

spleen. This assumption is not borne out by clinical experience.

(a) When the liver is cirrhotic, be the disease in the stage of hypertrophy or fibroid shrinking, it might be expected *à priori* that the venous obstruction would lead to distension of the spleen. It can only be by *à priori* argument, however, for clinical experience shows that in only 50 per cent. of the cases are the two conditions affiliated; that is to say, in half the cases of cirrhosis of the liver there is no enlargement of the spleen. This fact, which can be proved by carefully sifting post-mortem records, disturbs the even tenor of our well-assorted beliefs, and calls for explanation. A cirrhotic liver does not evidently always mean one in which the venous circulation is encroached upon; but, seeing that half the evidence is for and half against, it may be naturally asked which is the normal and which the variation; or does splenic enlargement depend on some factor totally unconnected with cirrhosis of the liver, and outside the liver influence? I have no hesitation in saying in this patient the latter is the cause. The patient is suffering from cardiac valvular insufficiency, which has caused at times a pronounced pulmonary congestion accompanied by hæmoptysis. The associated venous fulness of the liver, combined with the unresilient and inexpandable nature of the hepatic vessels, consequent on fibroid thickening of the tissues through which the portal vessels run, is calculated to produce a splenic engorgement and hypertrophy. In this case I can well believe that had there been no cardiac disease there need have been no splenic enlargement. This is of course a mere assumption; but seeing that in half the recorded cases of cirrhosis of the liver no splenic enlargement exists, and that even in many cases where it does obtain cardiac disease exists as well, the number of splenic enlargements directly dependent on contraction of the hepatic tissue is still further reduced.

Hypertrophy of the liver, therefore, in uncomplicated cirrhosis of the liver is the exception and not the rule.

(b) Fatty degeneration of the liver is not a direct cause of enlargement of the spleen. When the liver is fatty, splenic hypertrophy is the exception. Of course, when other organs are similarly affected to the liver, the effect upon the circulation may be such that the spleen does enlarge to some extent,

but such a condition cannot be attributed to the liver alone.

(c) Hydatid disease of the liver, even when the hydatids bulk largely, does not necessarily carry with it a hypertrophy of the spleen. Quite the opposite obtains, for in many cases the spleen is actually reduced in size. The spleen itself may be the seat of hydatids, and yet the splenic tissue is not increased in bulk.

(d) Amyloid disease of the liver is almost invariably associated with a similar condition of the spleen; yet it not infrequently happens that an amyloid spleen is actually small, in spite of being combined with what would seem to be an obstruction of the portal circulation in the liver.

(e) Abscess of the liver is but seldom associated with an enlargement of the spleen. Even when the abscess is huge, and occupying the greater part of one lobe, a splenic enlargement is quite exceptional. In the hepatitis antecedent to abscess, splenic fulness may be found, but when pus has formed the spleen usually returns to its normal size.

(f) Cancerous infiltration of the liver may be unattended by splenic enlargements. It is only when the portal or splenic venous trunks themselves are encroached upon by infiltrated glands, or by direct interference of the cancerous growth, that splenic engorgement is to be expected.

All these statements go to show that passive splenic enlargements dependent on hepatic disease are not the rule but the exception, and that the cause and effect usually accepted as existing between hepatic hypertrophy or cirrhosis and enlarged spleen will not bear clinical tests.

The anastomoses by which the blood can pass from the organs of the abdomen to the general circulation when the portal vein is obstructed, are as follows:

1. At the lower end of the rectum a free anastomosis exists between the hæmorrhoidal veins, which reach the portal circulation by the inferior mesenteric vein, and those which convey the blood to the general circulation through the internal pudic vein.

2. Through the œsophageal orifice in the diaphragm an anastomosis capable of great development takes place between the coronary (portal) vein of the stomach and the œsophageal (systemic) veins which ascend along the œsophagus to reach the *venæ azygos*.

3. The coronary (portal) vein also anastomoses with the phrenic (systemic) veins, which convey the blood to both the inferior vena cava and, through the internal mammary vein, to the innominate veins at the root of the neck.

The above form the most constant anastomoses, but there are several others.

4. Between the hæmorrhoidal (portal) and the vesical (systemic) veins a fairly free anastomosis is met with occasionally.

5. The coronary (portal) vein of the stomach frequently communicates with the left renal (systemic) vein.

6. The vasa brevia (portal) veins of the stomach,

7. The gastro-epiploic (portal) veins, and

8. The superior mesenteric (portal) vein may one and all have anastomoses with the left renal (systemic) vein.

9. The vasa brevia (portal) veins of the stomach anastomose with the left inferior phrenic (systemic) vein.

10. When inflammatory adhesions take place between the liver capsule and the adjacent surfaces of the diaphragm or abdominal wall, vessels pass along the newly formed adhesions from the portal to the systemic veins.

11. An anastomosis is sometimes found between the splenic (portal) vein and the azygos minor (systemic) vein.

12. A free anastomosis of great importance takes place along the pathway of the falciform ligament, between the veins upon the inner wall of the epigastrium and diaphragm (systemic), on the one hand, and the portal vein on the other. The largest branch accompanies the ligamentum teres, and joins the left branch of the portal vein; Sappey found this vein on one occasion as large as the little finger. The systemic veins involved are those in the substance of the rectus muscle, and the blood is conveyed up or down by the internal mammary to the innominate, and the deep epigastric to the external iliac respectively.

When the abdominal veins are enlarged the deep veins are primarily affected, the surface veins being involved only at a late stage; therefore distended superficial abdominal veins indicate a long-standing and pronounced obstruction. Œdema of the feet and of the abdominal wall may precede the ascitic swelling, but with the ascitic accumulation the œdema becomes more marked.

Considering the anatomical and physiological relations and functions of the spleen, one can readily understand the reason for frequent adhesions resulting when any pathological condition supervenes. With well-nigh every movement of the body the spleen alters its position, and with every meal the spleen enlarges, and again subsides when the plethora induced by the meal is over. The peritoneum covering the spleen is constantly moving upon the peritoneal surface of the diaphragm and stomach, and given a splenic congestion, inflammation, or irritation, the proneness to perisplenic adhesions is readily understood.

The spleen is, moreover, not only the physiological burying-place of the normal corpuscles of the blood, and the seat of their regeneration, but, when the corpuscles or blood plasma are charged with septic materials or become germ-laden, the spleen is called upon to deal with the germs freed of their host or carriers. Consequently we would expect, and in fact we find, the spleen becoming affected in almost every infectious or inflammatory fever. In measles, scarlet fever, typhoid, and many other diseases the spleen becomes enlarged, soft, pulpy, and tender. No more common cause of perisplenitis is known than measles; and in fact when, from whatever cause, the temperature of the body is raised, the spleen shows evidence of being involved. These diseases are frequently associated with basal congestion of the lungs, and a pleuritic rub is a common occurrence. This so-called pleuritic rub may be a misnomer; the rub is frequently not pleuritic at all, but is a peritoneal rub, caused by perihepatic or perisplenic movement and inflammation. In many ailments we find at the base of the left lung a congestion, associated with vomiting, hiccup, and friction,¹ and when the region of the spleen is being examined tenderness is elicited. These symptoms all point to the friction being of peritonitic rather than of pleuritic origin; in fact, that it is a perisplenitis and not a pleuritis that is present. In malarial fever we find the spleen very frequently enlarged, and perisplenic adhesions common. The plasmodium malarie is carried to the spleen in quantity,—not in dribblets, but in sudden accessions corresponding with the paroxysms of febrile disturbance of quotidian, tertian, or quartan ague. In continued fever such as typhoid, the case is different; the infiltration of the spleen is less sudden, and the

organ can accommodate itself to the altered state of the blood. Hence permanent enlargements are less common with continued than with the paroxysmal fevers. It is no doubt for the same reason that the spleen is so frequently of normal size in cirrhosis of the liver. The hepatic disease advances slowly and gradually, giving the spleen time to accommodate itself to the altered condition. By its elasticity, its trabeculæ, and by its muscular contractility the spleen can and does overcome venous engorgement when it is not called upon to do so too suddenly; but paroxysmal and sudden flushings of the spleen are apt to cause distensions of such a nature that the elasticity of the organ is destroyed, and the power of resisting blood pressure gone.

Treatment.—There are certain conditions of the spleen which can only be dealt with surgically. Tumours of the spleen, whether simple or malignant, may require the removal of the whole viscus. The spleen is seldom attacked primarily by malignant disease, so that excision for such growths is seldom called for. Simple cystic tumours of the spleen, when of great size and the splenic tissue wasting, are the most satisfactory tumours of the spleen to deal with surgically.

Several tumours of this nature have been removed, and the patients made good and lasting recoveries, and the microscopic examination some twelve months afterwards showed that the blood in no way differed from the normal, compensatory splenic action being no doubt performed by some other organ or organs.

When ascites is present the patient must be tapped, not once, but many times if necessary, and this operation is frequently attended by relief and permanent good. The operation is rational, as it is merely assisting nature to accomplish what she is attempting to do rather awkwardly. But it is useless to be content with one or two tapplings; one is apt to assume because the fluid recurs after the first or second operation that the process is useless. Not so. I tapped a patient (a Chinaman) for cirrhotic liver and enlarged spleen eight times in all. Between the first two some ten days only intervened, but the intervals gradually lengthened until between the seventh and eighth, four months intervened. The fluid never returned, and the patient was able to attend to his business and get about as usual.

When the spleen is ruptured, and signs and symptoms of alarming internal hæmorrhage apparent, the surgeon is justified in cutting down upon the spleen and removing it if all other steps prove unsatisfactory. A wound of the spleen does not bear stitching well, the soft friable tissue breaks away, and futile efforts in that direction may so prolong the treatment that the patient dies from loss of blood. There is no operation much more simple than removing a spleen which is free of adhesions; but if the adhesions are many, and the union between it and the neighbouring parts close, nothing is more difficult. The danger is not peritonitis, but secondary hæmorrhage from the ligatured stump. Provision must be taken to prevent this by applying numerous ligatures to the splenic pedicle during the operation, and securing absolute rest for twenty-four hours after the operation.

Abscess of the spleen is more common than text-books would have us believe. The evidence in support of this is unfortunately generally post mortem, and not clinical. The surgeon finds considerable difficulty in dealing with splenic abscess; the structure of the spleen is such that we cannot freely insert the needle of the aspirator in search of pus, as in the case of the liver. The withdrawal of the needle may be followed by severe hæmorrhage, or the escape of pus into the abdominal cavity. When abscess is suspected the spleen should be exposed, and the abscess dealt with either by evacuation or the removal of the whole organ.

Direct medicinal treatment of enlarged spleen is, to say the least of it, equivocal. When the disease is combined with anæmia and ascites, severe purgation is calculated to tax the patient's strength too severely. Good, however, will result by procuring gentle and regular purgation, and this is best accomplished by salines. Of these sulphate of magnesia is the most reliable and effective, and in the form of magnesia sulphas effervescens, in half-ounce doses, given in a small quantity of water every morning, or more often, is the most agreeable. Should circumstances permit, the determining cause of the splenic enlargement may necessitate constitutional remedies. Syphilitic visceral disease in which the spleen is involved requires appropriate remedies, and in the advanced stage of the disease increasing doses of the iodide

of potassium is our sheet anchor. Iodides are recommended for splenic enlargements other than syphilitic, and in passive enlargements of the spleen their use is rational.

When cardiac disease is the origin of the trouble, appropriate remedies must be applied,—now to relieve the effects of a visceral congestion, now to strengthen the heart muscle, in order to aid the organ to accommodate itself to the disturbed circulation.

Iron has to be used with judgment in splenic anæmia; mineral acids with chloride of ammonium are effective, more especially when the liver is enlarged at the same time as the spleen.

Quinine is not of much direct value in reducing the enlargement of the spleen in chronic malaria. In this condition change of residence to a non-malarial district is the most effective. A prolonged stay in a temperate or cold climate is the only effectual remedy for splenic enlargements dependent upon malaria, and even in a cool climate the ailment will disappear much more readily when the patient dwells at high altitudes. As in malarial so in all splenic enlargements associated with anæmia, be their causes what they may, it is only at high levels that speedy advantages are to be attained.

Local applications over the splenic regions have some arguments in their favour. Counter-irritation by the liniment of iodine or specific unction by the unguentum hydrargyri iodidi rubri or the oleate of mercury have claims to consideration.

Residence in a city, or at the sea level by the seaside, will but tend to aggravate the disease, however salubrious the chosen spot may claim to be. At high altitudes alone can the benefits of a cold or temperate climate be fully gained. "The open-air" treatment is an imperative necessity in all splenic troubles. The diminished and altered corpuscles of the blood show by the anæmic state of the patient their inability to carry sufficient oxygen to nourish the body satisfactorily, and it is essential that the best and purest supply of fresh air should be provided to compensate for their attenuated condition.

A Syringe to overcome any resistance, as in Schleich injections to the palm, has been invented by Dr. Wullenweber. A description can be found in the '*Deutsche med. Woch.*,' December 2nd, 1897.—*Medical Record.*

A CASE OF ACUTE MULTIPLE NON-RHEUMATIC SYNOVITIS.

A Clinical Lecture delivered at St. George's Hospital, London, on February 13th, 1899, by

H. D. ROLLESTON, M.A., M.D. Cantab., F.R.C.P.,

Physician to the Hospital.

A MAN æt. 30 years, who had been ill since December 17th with a sore throat, got very wet a few days after Christmas, and was seized with acute pains in the limbs on December 31st. He was then treated with salicylates for the joint pain, and with morphia for sleeplessness, but without any benefit. On January 5th his nose bled, and he lost, according to his own estimation, as much as four pints.

On admission to the hospital on January 6th, he was anæmic, with dry, hot skin, slightly yellow, suggesting sepsis, but with no icteric tinge of the conjunctiva. His knees and wrists were hot, painful, and swollen, the outline of the synovial membranes being well shown, but there was no redness of the skin over the affected joints. His heart was normal in every way, and it may be said at once that it remained so during the whole of his illness.

His dry skin did not suggest acute rheumatism, and his general appearance was rather that of a septic process. The most probable cause of a septic arthritis, as being perhaps the commonest, seemed to be gonorrhœa; but he had no urethral discharge, and was quite clear that he never had had. With the exception of tonsillitis, to which he had always been subject, he had been remarkably free from illnesses of any kind. He had never had rheumatism, pneumonia, or syphilis.

On admission it was thought well, in spite of the fact that his illness did not closely resemble acute rheumatism, to try him with a combined treatment of salicylate of soda ten grains every four hours, and salicin ten grains three times a day. I was inclined to do this because quite a short time before an anæmic girl was admitted under my care with a dry hot skin, dry tongue, and joint pains; although from her aspect and dry skin I suspected septic and not rheumatic disease, no definite evidence of the former condition was forth-

coming, and she was accordingly given the chance of reacting to salicylates, and did so in a very satisfactory manner. It therefore seems well to adopt this course in a case where, though suspected, septic infection cannot be definitely established. In this man, however, salicylate treatment gave no relief, and did not affect the temperature in the least. From January 7th to January 14th his temperature was between 102° and 100° , for the following week 100° to 101° , and came down by degrees to normal about January 26th.

On January 13th the salicylate was cut off, and quinine substituted; after twelve days of this he was, just as his temperature was becoming normal, put on iodide of potassium and liquor hydrargyri perchloridi. But it cannot be said that any treatment by the mouth relieved him, or made any appreciable difference to his condition.

When salicylate completely failed to modify his condition, it seemed probable that he might be affected by some septic process; though that it must be of a comparatively mild nature was manifest, since no suppuration had occurred. Accordingly Dr. Slater kindly examined the blood; but reported that the cultures only showed a staphylococcus almost certainly derived from the skin. There was therefore no proof of any bacterial blood infection.

After he had been nine days in the hospital his hips became affected, and there was visible and very considerable distension of the synovial membrane in the groins. Some small lymphatic glands were felt in the groins and in the axillæ, but they were not definitely enlarged, and it may here be mentioned that the spleen was never felt to be enlarged. At this time he had pain on breathing on the right side of his chest, but no friction rub could be heard; this pleurodynia readily yielded to poulticing. The constant application of an ice-bag to his hip-joints, and afterwards to his hands and wrists, gave him marked relief. The pain and swelling migrated after this to his elbows, but only for a few days. A recurrence of right pleurodynia later on was relieved entirely by dry-cupping. After his temperature had been normal for some days, he was got up a little, as I was anxious to get him into the fresh air as soon as possible; but this was apparently premature, for a return of pain in the left ankle followed. This again was removed by

rest in bed and the local application of lead lotion. After this he went on well, eating and drinking well, and could move his joints perfectly well and without any stiffness or limitation. He was naturally very weak and pulled down by his illness, which had lasted over four weeks; his weight before was 11 st., and afterwards 8 st. 2 lbs.

To summarise, the most remarkable points about the case were—

(1) The occurrence of multiple painful swellings of the joints, accompanied by fever, but with little perspiration, no cardiac lesions, and not reacting to salicylates.

(2) The absence of any evidence of adhesions in the affected joints—a sign that the inflammation had after all not been very severe.

(3) The ease with which local applications, such as the ice-bag, acted upon the affected joints and gave relief.

As to the nature of the joint affection in this case there was an inflammatory reaction in the joints, which might be called an arthritis. As shown by the readiness with which the articular inflammation and pain yielded to local treatment, and by the fact that no adhesions were left behind, the inflammatory changes in the joints did not progress further than leading to a considerable effusion; the ligaments, cartilages, and bones showed no evidence of having suffered, and the only tissue that with certainty can be said to have been inflamed is the synovial membrane. There was, therefore, an acute multiple synovitis. It is better to reserve the terms arthritis and osteo-arthritis for more severe inflammatory affections of joints that spread to the ligaments, cartilages, and bones.

The cause of this wide-spread and acute synovitis is an interesting subject for discussion, but it must be admitted that it is easier to say what it was not than to definitely say what it actually was.

It was not the synovitis of acute rheumatism, as has been already seen, inasmuch as salicylic treatment had no effect. The late Dr. James Andrew, Physician to St. Bartholomew's Hospital, used to describe as acute osteo-arthritis, cases resembling acute rheumatism generally, but differing from it, just as this man did, in not reacting to salicylates, in not having any cardiac complications, and in not perspiring so freely.

It was not pyæmic in the ordinary sense of the word, since there was no suppuration, and the

cultures from the blood did not prove that there was any septicæmia.

There is a large class of infective affections of joints; their number is increasing year by year with our knowledge, and to it are being added cases which were formerly considered as belonging to the vague class of rheumatic affections, whether acute, subacute, or chronic. We may divide infective joint affections, at any rate theoretically, into two groups: (1) that in which a micro-organism settles down in the joint, multiplies there, and by producing a poison sets up inflammatory changes—to cases of this kind the term septic is applicable; and (2) that in which micro-organisms elsewhere in the body manufacture a toxin. This poison is carried by the circulation to the joints, which, presumably being especially susceptible, become acutely inflamed; to cases of this kind the term toxic may be applied.

As examples of septic arthritis secondary to a primary infection elsewhere in the body, there is the familiar urethral or gonorrhœal arthritis, in which gonococci or other microbes are carried by the blood from the original site of infection to the joints, and there multiply. It is somewhat doubtful how many or what proportion of joint affections secondary to urethritis should be regarded as gonorrhœal of the joints, for the gonococcus is not always found in the joints, and often there appears to be a mixed infection, other micro-organisms having gained an entrance through the inflamed mucous membrane of the urethra into the circulation. But at any rate it is a good example of microbic arthritis; there are different degrees of intensity in urethral arthritis in different cases, and possibly it may be found that these are correlated with differences in the species of the micro-organisms found in the joints.

Gonorrhœal inflammation elsewhere than in the urethra is comparatively infrequent, but primary gonorrhœal conjunctivitis is known, especially in children, to be occasionally followed by joint manifestations exactly like those just mentioned. Mr. R. C. Lucas has recently collected twenty-three cases of arthritis in infants secondary to gonorrhœal ophthalmia.*

In cerebro-spinal meningitis the characteristic micro-organism, the diplococcus intra-cellularis,

may give rise to arthritis, and be found in the joints (Osler).^{*} Transference of the typhoid and pneumonia micro-organisms to the joints is also well established, though very rarely seen in ordinary hospital practice.

Now with regard to the second group of infective joint affections which were spoken of as toxic; here there is microbic activity in some other part of the body, and it may reasonably be supposed that a poison produced by these micro-organisms passes into the blood, and is carried to the joints, while the micro-organisms themselves remain *in loco*, and do not pass into the blood. Before going into the sequence of events any further, it may be well to remember that besides gouty arthritis—where there is undoubtedly a poison, uric acid, responsible for the change,—an excellent example of a purely toxic synovitis is to be found in the swollen and painful joints sometimes seen after the injection of diphtheria antitoxin serum. In this hospital an antitoxin rash is commonly seen seven days after injection, but pain or swelling in the joints do not occur. Elsewhere, however, with different serums articular pain or effusion are occasionally seen. It is probable that they depend on the blood serum, and have nothing to do with its antidiphtheritic properties.

The existence of toxic joint disturbances secondary to septic absorption elsewhere is perhaps difficult to prove absolutely, but cases occur in which suppuration around the teeth, vaginal discharge, &c., are associated with pain and swelling of joints. In hypertrophic pulmonary osteo-arthritis it is very probable, as Marie suggests, that the cause of the bony lesions is a poison absorbed from the lungs and carried to the articular ends of the bones.

In tonsillitis, pains in the joints are common, but they are usually regarded as being rheumatic in origin; and as at present the micro-organism of acute rheumatism, if there be one, remains undiscovered, there is no useful object in speculating as to whether the synovitis in rheumatic fever is due to a poison manufactured in the joints, or whether it is formed elsewhere and then carried to the joints.

In pyæmia, apart from purulent arthritis, the joints may become painful and swollen without

* Lucas, 'Lancet,' 1899, vol. i, p. 230.

* Osler, 'Boston Med. and Surg. Journ.,' vol. cxxxix, p. 641.

anything further taking place; this synovitis, for such it appears to be in a few joints I have examined after death, may pass away. Possibly the synovitis may be due to micro-organisms that have reached the joint in such small quantities as not to succeed in setting up suppuration, and this is perhaps the more probable view; it is conceivable, however, that some of these slight cases are toxic and due to poisons, rather than septic or microbic.

To sum up, it may be said that toxic arthritis certainly occurs (*e. g.* antitoxin synovitis), that it is slight in degree, and thus forms a contrast to septic or microbic arthritis, where the process is more severe and where suppuration is likely (though this is not by any means universally the case, *e. g.* in urethral arthritis) to supervene.

In the case of this man there was no very satisfactory inlet, so to speak, for the poison or micro-organisms; before he came into the hospital he had had a sore throat, and at first I was inclined to think that the arthritic effusions were secondary to a microbic blood infection that had gained an entry through the tonsils, but, as has been already said, cultures taken from his blood did not prove that there was any hæmic infection. It is of course possible that the joints were occupied by micro-organisms of a comparatively low degree of virulence; but as it did not seem desirable to aspirate his joints, this point cannot be absolutely settled. The ease with which the synovitis yielded to simple local measures, and the absence of any manifest damage to the joints, point to the inflammation being comparatively slight, and this point is, as far as it goes, against a microbic invasion, and in favour of a toxic synovitis.

While in the hospital no active inflammatory process was taking place in his throat, or indeed in any part of the body except the joints, and if the synovitis was due to toxins formed elsewhere, it is difficult to imagine where they were formed. The alimentary canal is a possible source, thus in connection with dysentery arthritis has been described, but there was no reason to think that any abnormal processes were taking place in his stomach or intestines; and if there had been, the salicylates should by their antiseptic action have produced some amelioration, which they certainly did not.

Thus, while the synovitis should with perhaps greater probability be regarded as toxic rather than

septic, it cannot be said with certainty that this was so.

Finally, one point with regard to treatment came out forcibly in this case,—salicylates and quinine produced practically no effect on the condition of his joints, while the application of dry cold by means of an ice-bag covered with flannel had a very beneficial effect. No doubt the treatment of the manifestations of a disease, or symptomatic treatment, is to be deprecated if there is any danger of our looking no further, and neglecting the primary disease; but it is important not to forget the relief that the patient may get from simple, even household, local applications and counter-irritants, old-fashioned though they be.

The Treatment of Bronchitis. — Quincke ('Gaillard's Medical Journal,' October, 1898) draws attention to the fact that in bronchitis the object of treatment is to facilitate the draining away of the exudation. This is, however, possible in bronchitis only to a limited extent. Cough, and especially the act of vomiting, assists to this end. Often in the early morning the bronchitic brings up a large quantity of sputum by the help more or less of persistent coughing. Quincke recommends that at this time the patient should lie as flat as possible for a couple of hours, to assist the draining of the secretion into the large bronchi, and hence its expectoration. The patient becomes accustomed to the position, and can expectorate by turning the head to one side. After a few days the foot of the bed may also be raised from eight to twelve inches. In suitable cases, in from two to four weeks there is a considerable diminution in the sputum. This mode of treatment is adapted to cases of chronic bronchitis which have led to a cylindrical or sacculated bronchiectasis in the lower lobes of the lung. It is of no avail in cases of diffuse, and especially recent bronchitis with general secretion, or in cases of abscess cavities communicating laterally or incompletely with the bronchi, or of cavities with irritating contents.

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PYORRHŒA ALVEOLARIS AND ITS RELATIONS TO GENERAL MEDICINE,

BY

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PYORRHŒA ALVEOLARIS in its strictest sense is a term which may be applied to any diseased condition of the gums which is accompanied by the discharge of pus from them into the mouth. As a matter of fact its use is confined to certain well-defined affections of the tissues surrounding the teeth.

It was undoubtedly first described by Dr. J. M. Riggs, of Hartford, Con., U.S.A., and must have been recognised by him as early as 1844, since in a paper read by him at a meeting of the Connecticut Valley Dental Association in June, 1869, he stated that he had treated this condition with success for over twenty-five years. It has been from that time associated with his name, and called Riggs' disease. Subsequently other names have been proposed, such as expulsive gingivitis, alveolar periostitis, and phagedenic pericementitis. The term pyorrhœa alveolaris was proposed by Dr. Rehwinkel at a meeting of the American Dental Association in 1877, but it appears to have been previously used by Wedl in a paper published several years previously.

Pyorrhœa alveolaris is in many ways an unfortunate name, as it only describes one feature of the later stages of the affection. Nevertheless it is now so firmly established that we may usefully retain it as a convenient term. Before studying the pathology of pyorrhœa, it will perhaps be as well to briefly review the manner in which the teeth are fastened in their sockets.

The teeth, as we all know, are received into sockets in the alveolar processes of the maxillary bones. Between the teeth and the walls of the aforesaid sockets is interposed a substance called the periodontal membrane. This is composed of fibrous elastic tissue, and in fact may be looked on as a suspensory ligament of the tooth, as it

allows a limited amount of motion. Besides supporting and fixing the tooth in its position it furnishes nourishment to the bony surfaces between which it is placed—the inner wall of the alveolus, and the outer layer of cementum on the tooth. For this latter office it is well supplied with blood-vessels from two sources. The artery which enters the apex of the tooth gives off a branch which enters it below; whilst above, the gums send vessels to it over the margin of the socket of the tooth. At its upper margin the periodontal membrane forms a firm attachment between the neck of the tooth and the periosteum of the alveolar wall, such attachment being from one eighth to three eighths of an inch from the free margin of the gums. The free margin of the gum is covered on its exposed surface by a very dense squamous epithelium which is able to withstand the abrading action of the food during the act of mastication. Under this is a layer of softer epithelial cells, which cover a series of papillary projections from the subjacent fibrous tissue. The epithelium which covers the inner surface of the gum margin, *i. e.* that portion in contact with the neck of the tooth, consists of very soft round or polygonal cells, and is of considerable depth. Although not of glandular structure, it secretes a profusion of small rounded cells, which are to be found in abundance in the saliva, and are usually called mucous corpuscles. For this reason it has been termed the gingival organ. It is probable that this structure is not the sole source of the mucous cells in the saliva, but it will certainly account for many of them. This can easily be proved by passing the end of a thin instrument under the free margin of the healthy gum, and transferring the secretion thus obtained to a slide for microscopic examination. The knowledge of this organ and its secretion is of the greatest importance to the physician; as the accumulation of these cells mixed with organisms under the free margin of the gum forms soft cheesy masses that so nearly resemble pus, that they may be easily mistaken for it, and thus lead the observer to make an erroneous diagnosis of pyorrhœa.

The production of pyorrhœa depends upon two factors, a predisposing cause and a local irritation.

The predisposing cause may be tubercle, syphilis, scurvy, the exhaustion of acute infectious diseases, or any other source of malnutrition.

The exciting cause may be, and most usually is, a gingivitis produced in one of the ways to be presently described.

There is also a pyorrhœa of gouty origin, in which the local necrosis of the peridental membrane is caused by gouty disease of one of the blood-vessels in its substance.

Pyorrhœa is met with at any age, and it is quite erroneous to consider that we only see it after thirty-three or thirty-six years of age. Of course young people will not suffer from it in the degree that older ones do, because there has not been so long a time for the affection to have developed.

For many years the association between gout and pyorrhœa has been noticed, and a certain relation suspected. The chief advocate of this theory is Professor Pierce, who in a paper published in the 'Dental Practitioner' in April, 1894, enunciated the theory that pyorrhœa was nearly always a local expression of a constitutional diathesis, and that the mechanism of its causation was some morbid constituent of the blood, which exuded and infiltrated the alveolo-cemental membrane, the effect of this being to set up a pericemental irritation commencing at or near the apical extremity of the root; and he considers it proved that this irritating material is of a gouty nature, from the fact that as the result of a series of chemical analyses made by him, the concretions, which in these cases are found on the roots near the apices, were found to consist largely of uric acid and urates.

As against his theory it has been argued by Burchard and others, that the deposit could not take place upon the root of the tooth except from the lining membrane of the alveolus, since this is in contact with the tooth and practically attached to it. Talbot also ('Dental Cosmos,' vol. xxxviii, p. 310) undertook a series of experiments, and only found uric acid upon the deposits of very few teeth out of those which he examined. It is, however, almost certain that Pierce is right in the main, but that the uric acid deposits are the result and not the cause of the gouty inflammation. One has only to bear in mind that the alveolo-cemental membrane is a vascular fibrous tissue, and is the analogue of the ligamentous structures in joints, to understand how likely it is that gouty inflammation of it should take place. We know

that over-use of a joint will often determine an attack of gout in it; and the pericemental membrane being a true suspensory ligament of the teeth and allowing a really considerable amount of movement in them, may be affected in the same way as other joints—for the tooth in its socket may be properly regarded as a joint. Moreover, from defective position of the teeth, mal-occlusions and defects in the bite, the suspensory ligament is constantly undergoing considerable strain, and we may reasonably expect that in a gouty person there may be local inflammation set up. In these cases there will probably be first inflammatory thickening of the vessels supplying the pericemental membrane of a gouty nature. This will be followed by thickening of their walls from swelling of the tunica media in the usual way observed in gouty inflammation. There will then ensue obliteration of its lumen, and death of the portion of tissue to which it is supplied, with a local molecular necrosis and the subsequent deposition of urates.

It appears more than probable that syphilis may account for a certain number of the cases of pyorrhœa which are met with in practice. One observer found that out of *twenty-three syphilitic patients, eleven had true pyorrhœa*. In these cases there are probably two factors at work, first the constitutional weakness, which is the direct result of the disease, and secondly the gingivitis, which it is almost impossible to avoid when administering mercurial preparations. Although the greatest care may be taken to prevent salivation, yet the soreness of the gums which is almost always produced, and which is generally accepted as the indication that the proper dose has been reached, will be quite sufficient to impair the integrity of the peridental membrane and open the way to the invasion of pyogenic organisms.

Disease of the peridental membrane and subsequent pyorrhœa appears to arise in one of the following ways: as the result of a gingivitis, following death of the pulp of the tooth, or from gouty disease in the substance of the membrane itself.

All these lead up to the same end, but by slightly different paths. We will study them in rotation.

THE RESULT OF A GINGIVITIS.

We may conveniently discuss pyorrhœa commencing in this manner under several heads which

form distinct stages in the progress of the affection.

(a) The predisposing cause.

(b) The local irritation which is the immediate cause of the gingivitis.

(c) The gingivitis itself.

(d) The streptococcus invasion, and

(e) The absorption of bony tissue.

(a) *The predisposing cause.*—As predisposing causes of pyorrhœa we may have the causes which predispose to the antecedent gingivitis, or which may render the peridental membrane less resisting. Thus we may have an hereditary tendency to catarrhal affections or chronic nasal obstruction with its induced catarrhal condition of mouth and fauces.

On the other hand, pyorrhœa may well be predisposed to by any condition which will depress the general nutrition, since the circulation will fail first in the remotest capillaries, such as those in the peridental membrane. These tissues will be thus rendered less resistant. We may thus find as factors, lactation, hæmorrhages, syphilis, tubercle, and convalescence from acute specific fevers. It is also extremely probable that in nervous affections such as tabes and rheumatoid arthritis, which produce or are accompanied by trophic changes, there may be an increased tendency to degenerative processes in the peridental membrane.

It is obvious that in anæmia the resisting power of the peridental membrane must be greatly lessened from the defective circulation in the gums.

(b) *The local irritation.*—It appears to be quite certain that the gingival organs possess, in common with some other tissues of the body, the power of selecting and excreting poisonous substances from the blood. It is also well known that some of these cause hyperæmia or even inflammation in their passage. We have notable examples of this in the inflammation of the gums caused by mercury and iodide of potassium. Although this is really and essentially a gingivitis, yet it is usually spoken of as salivation, owing to the fact that the salivary glands are also affected. In its slighter forms, as met with in patients who are undergoing systematic treatment, it is a gingivitis pure and simple. The exciting cause of a gingivitis, then, may be a mercurial course.

Dentists are responsible for many cases of

gingivitis. In some instances they directly inoculate the gums with disease germs by performing dental operations without first *sterilising the mouth*. They confine their attention to disinfecting the instruments, and forget that the mouth itself is full of disease germs waiting to take advantage of any laceration of surface. In other cases, by means of improperly fitting crowns, fillings overhanging at the neck of the tooth, crude and badly finished amalgam stoppings, and badly constructed and ill-fitting plates, they keep up such a continual irritation of the edges of the gums that gingivitis soon occurs.

Gingivitis is very commonly induced by the irritation of lactic acid formed by the fermentation of food residues retained under the margins of the gums. This generally occurs in people who do not brush the teeth at night when retiring to bed. It is much more important to cleanse the teeth at bedtime than in the morning. It is in the long period during the night, when the food débris is undisturbed, that opportunity arises for fermentation to take place.

Gingivitis is also produced by tartar upon the teeth. Every one is aware that tartar is deposited upon the teeth, but very few understand why and how this comes to pass, and wonder why it should accumulate upon some teeth, and not on others. It is now pretty well established that calcareous deposit in any part of the body is effected by the agency of an organic material such as mucin. Without the presence of such a substance the earthy salts cannot be thrown down from the animal fluids which hold them in solution in such a form as to form an adherent deposit. The calcium salts are apparently held in solution in the saliva by means of carbonic acid gas, and are thrown down on coming in contact with the air. If any mucin is present to bind them, calculi will be formed, which will adhere to any suitable adjacent surface, especially if there is also present a local roughness to assist the adhesion. The mystery of the deposition of tartar in the mouth is thus easily explained, and, as will be seen, it is most frequent just where we should *a priori* expect to find it, viz. in the neighbourhood of the ducts of the glands, which pour mucin-containing secretions into the mouth.

One of the most frequent sites for the deposition of tartar is on the buccal surfaces of the back

teeth in the upper jaw. A little reflection will remind us that this is opposite the spot where the parotid glands, by means of Steno's duct, pour their secretion into the mouth. The parotid saliva differs from the secretion of the other glandular tissue in the mouth in containing a relatively small proportion of organic material, and this in the form of a globulin rather than a mucin. The consequence, as we should expect, is that the calculus formed by its aid differs in character from that deposited upon teeth in other situations, and is softer and more easily broken down.

But in order that tartar should be deposited in this situation another factor is essential, and that is that the parotid secretion should have an opportunity of remaining undisturbed in contact with the teeth on which the deposit is to take place. This happens when from defect of the lower molars the upper ones are practically not used in mastication. In such cases the saliva remains, and stagnates in the pocket formed by the reflection of the mucous membrane outside and above these teeth, and this region not as a rule being reached by the tooth-brush, every condition requisite for an abundant deposition of tartar is afforded.

The same combination of circumstances obtains to a great extent under the tongue, and as a consequence a plentiful deposit of tartar takes place on the lingual surfaces of the lower incisors and laterals. It is important to note that whilst in healthy mouths the tendency to the deposit of tartar is comparatively slight, yet the presence of even a slight degree of gingivitis will enormously increase the process. And this is easily understood when we reflect that such an inflammation of the gums will add a relatively large amount of proteid material to the saliva in these situations. The formation of calcareous deposits under the edges of the gums is thus accounted for. The effect of the gingivitis will be to slightly evert and separate the edges of the gums from the neck of the tooth, and in the cavity thus formed the saliva mixed with the albuminous discharge of the gingivitis will cause a deposit limited in extent of very hard and adherent tartar.

After tartar has once been deposited upon the neck of a tooth, a vicious circle is established which is very hard to break. When a slight deposit has once taken place it becomes an irritant which will itself perpetuate the condition. It is a

remarkable fact that all varieties of tartar appear to possess extremely irritating qualities which keep the border of the peridental membrane and the adjacent gum in a continual state of chronic inflammation, with the result that the deposit slowly and continuously increases. The effect of the irritation of the deposit of tartar is to cause a change of a progressive nature in the peridental membrane, which undergoes degeneration, as we shall presently show, with the formation of a cavity, or as it is technically termed, a pocket.

The effect of the tartar in eroding the gum is, I think, due to two factors:

1. Abundance of lactic acid is generated in the food debris which accumulates in the space between the edge of the tartar and the gum. All starchy food contains lactic acid germs, as one can readily demonstrate in the following manner. Take some crumb of bread and mix with warm water and keep at the temperature of the body for a couple of hours or so. Express the liquid from it, and titrate it with a decinormal solution of sodium hydrate, using a few drops of an alcoholic solution of phenol-phthalein to mark the end of the reaction. The total acidity will by this means be found to be considerable. This can be demonstrated to be mainly due to lactic acid by the ordinary method with Uffelmann's test. This lactic acid probably acts upon the gum, making it more vulnerable.

2. The absolute pressure of the tartar will erode the gums. The immediate effect of the pressure of the rough unyielding tartar upon the spongy inflamed gum is, by the irritation set up, to cause it to grow. The swollen gum then presses in its turn against the lower edge of the tartar until the effect that its vitality is destroyed, and a breach of surface takes place.

Besides the tartar deposited from the saliva upon the teeth, we have another variety which occurs under the margins of the gums. This has been termed serimic calculus. This name was given to it because it was supposed to be formed from the serum of the blood. It is more probable that in the case of gums not affected with gingivitis, it is deposited from the secretion of the tubular epithelial glands of the peridental membrane. It may then set up a gingivitis, the immediate result of which will be the deposit of fresh nodules of tartar of a slightly different character derived from

the secretions of the diseased surface. It is an unfortunate fact, tending to obscure an intelligent understanding of these concretions, that most writers on dental subjects apply the term serumic calculus indiscriminately to all nodules of tartar occurring upon the roots of teeth. This is obviously wrong, as deposits may arise in quite a different manner. An example may be found in the scales found on the roots of teeth where the disease has originated in gouty disease of a portion of the periodontal membrane itself. Under these circumstances the deposit of tartar appears to arise in obedience to the general law which obtains in pathological processes in the body, that dead tissue appears to attract in some mysterious way the calcium salts of the blood. In some cases, probably in all, this is a wonderful effort on the part of nature either to repair the damage or to shut the diseased area off from the rest of the body, and to confine the morbid process within the enclosed area. We have an example of this in the calcification which takes place under favourable circumstances in tubercular disease of the lung. But, unfortunately, it is impossible for nature to adjust the mechanism so that it can provide for all contingencies, and so we have the aimless deposit of lime salts in positions where it cannot only be of no possible service, but even do positive damage.

Serumic calculus is dark and extremely hard, and deposited in minute scales or nodules.

(c) *The gingivitis and destruction of periodontal membrane.*—The inflammation of the gums either produced directly by the irritation of the deposits of tartar, or which having preceded it is aggravated by it, is familiar to us all—the tumid, reddened, spongy gums, bleeding at the slightest touch, and on cleaning the teeth. Up to this point there has not of necessity been any pyorrhœa. The gums do not exude pus when pressed. But further changes soon take place. The first effect of inflammation of the gums, as of other tissues, is the accumulation of leucocytes and the proliferation of the connective-tissue elements. The effect of this state of things is mechanically by mere pressure and by blocking of vessels, to cut off the portion of the blood supply of the periodontal membrane which is derived from the gums. At the same time the morbid process extends to the periodontal membrane, following the course of a blood-vessel or a lymph path, and this structure

also becomes swollen by proliferation of leucocytes and connective-tissue corpuscles. As it has been, as we have seen, partially deprived of its vitality and blood-supply by the gingivitis, the effect of the pressure to which it is subjected by its increased bulk between the two unyielding surfaces, tooth and alveolus, is to cause local necrosis. The immediate result is the liquefaction and breaking down of the diseased areas, and the formation of a cavity or pocket between the alveolus and the root of the tooth. This terminates this stage.

(d) *The streptococcus invasion.*—Up to this point, as we have remarked before, there has been no production of pus. But this condition of things does not last long after the pockets have been once formed. There is soon infection of their walls by the pus-forming organisms always present in the oral cavity, and true pyorrhœa is established. At this stage a careful examination with a thin flat blade will show that destruction of the periodontal membrane is taking place, as the instrument will pass up further than it should along the side of the tooth, into a little pocket which has already formed, and contains pus. These pockets are the characteristic feature of the disease; they follow the length of the fibres of the periodontal membrane, and run up alongside the root of the teeth. They may be single or multiple, and may be met with on the lingual, buccal, or proximal sides of the teeth. The tendency eventually is to the destruction of the whole periodontal membrane, as the pockets slowly widen until they encircle the teeth. It not unfrequently happens that the entire apex of a root is denuded, whilst the tooth is held in its place by a strip of membrane on the other side, which has as yet undergone little change.

(e) *The absorption of the bony tissues.*—At the same time that the changes which we have described are taking place in the periodontal membrane, absorption of the alveolar wall is going on. In some cases, whilst the inner portion of the alveolar wall is being destroyed, deposit of new bone is taking place on its outer surface. The result of this is that the alveolar wall becomes lipped, and by its everted edge holds the gum away from the tooth. The destruction of bone appears to be a process of true absorption rather than a molecular necrosis, and is the result of an

absorptive action of the membrane itself, possibly through the agency of osteoclasts. This is due to the irritation produced by the products of inflammation.

The atrophy of the pericemental membrane keeps pace with the absorption of the alveolar process, so that a V-shaped orifice is found on examination, the floor of which is the exposed root of the tooth. If the tooth is extracted early in the disease there will not be found as a rule any nodules of tartar on the root; but if it is examined towards the close of the disease, when the whole of the root is exposed, small nodules will be found scattered over its surface. These are quite distinct from the serumic calculus which forms under the edges of the gums, and correspond to individual necrotic areas in the pericementum, in each of which is a deposit of lime salts.

FROM GOUTY DISEASES IN THE SUBSTANCES OF THE PERIDENTAL MEMBRANE.

In such a case the affection commences as gouty disease of a part of the pericemental membrane. A vessel becomes obliterated, and necrosis of the small area supplied by it takes place. At this stage there will probably be no symptoms except, perhaps, pain in the alveolar process adjacent to the tooth. An examination of the gums will reveal nothing abnormal. There is no gingivitis, and there is no breach of continuity of the peridental ligament. The next stage will be a painful swelling over the root of the tooth. The patient is said to have a gumboil, and there are all the symptoms of acute apical pericementitis. The swelling is either opened with a bistoury, or breaks of its own accord, and a thick glairy fluid escapes. Sometimes the discharge consists of pus, but this is rare except there is also an exposed pulp, through which the pyogenic organisms can get access. On examination with a probe, we shall find that there is a loss of the underlying alveolar wall, and that we can feel at the bottom of the cavity the root of the tooth denuded of its pericementum. Upon this root we shall often find a calculus. This, as was demonstrated by Pierce, will, in the great majority of cases, contain a large proportion of urates. If not properly treated, the disease will progress until the pericemental membrane is perforated towards the gum margin, and a pocket is formed.

In other cases the course of the affection is different. Instead of perforation of the alveolar wall we shall have extension of the gouty necrotic process towards the margin of the peridental membrane, which becomes perforated, and a pocket is formed which undergoes infection with pus-producing organisms.

Another way in which pyorrhœa may arise is from death of the pulp of the tooth. The first event in pyorrhœa from this cause is death of the pulp of the tooth. By infection through this dead pulp we next get inflammation of a limited portion of the peridental membrane near the apex of the tooth. Absorption of the alveolus immediately over it will then take place, and we shall get a painful swelling on the outside of the gum, which will either eventually break or be opened by the surgeon. In either case it will be found that the fang of the tooth is exposed, and upon this will usually be found some nodules of tartar. It has been held by some writers that such tartar is really an attempt by nature to repair the damage done by the deposition of lime salts; but whether such is the case or not, it will be necessary to remove them before a cure can be effected.

The subsequent course of an affection of this kind will resemble other cases of pyorrhœa if it be not arrested by appropriate treatment. The morbid process will extend towards the free edge of the peridental membrane, and a pocket will eventually be formed.

(To be continued.)

SYPHILIS AND TABES.

ERNST KUHN ('Arch. f. Psych. und Nerv.,' 1898) says that the statistical statements of Fournier and Erb regarding the frequency of syphilis in the antecedents of tabes have given rise to much controversy. Among other criticisms it has been contested that the statistical method is most unreliable, and that the results may be interpreted in accordance with whatever theory the investigator may be supporting. Granting the weakness of statistical evidence, it is to be remembered that it constitutes the only available testimony on the ætiologic relation of syphilis to locomotor ataxia. We are not in a position to show by anatomico-pathological methods that syphilis causes tabes,

and the latter disease ordinarily follows the former after such a considerable lapse of time, and is, moreover, so different from it that clinical evidence is entirely worthless.

The author's statistics are based on a study of 214 cases of tabes (136 men and 78 women) from the neurological service of the Charité, in Berlin, in comparison with 600 non-tabetic patients (400 men and 200 women) from the same service. The latter class belonged to the years 1894, 1895, and 1896, while the tabetics covered the period from 1880 to 1896 inclusive. This disproportion in the time of observation has some effect upon the result, tending to raise the proportion of syphilitics in the non-tabetics, for of late years a history of syphilis has been much more carefully sought than was formerly the case.

The results of the inquiry, which are interesting and apparently conclusive, are in brief as follows: Of the tabetic men, 38.2 per cent. gave a conclusive history of syphilis, while of the men with other nervous affections only 7.75 per cent. gave a like history. By conclusive evidence of syphilitic infection is meant a definite history of a primary sore with following secondaries or antispecific treatment in the venereal wards of the Charité. The non-tabetic men gave a history of chancre with symptoms following which were suspicious of syphilis in 1.5 per cent., while the tabetics gave such a history in 8.1 per cent. Of the male patients with tabes 21.3 per cent., while denying the primary lesion, had suffered from such symptoms as would tend to a diagnosis of syphilis; of the patients with other nervous diseases only 4.5 per cent. had had such symptoms. Finally, in only 1.1 per cent. of the male cases of locomotor ataxia was there no evidence whatever of venereal infection, while 67.5 per cent. of the non-tabetics gave this negative history. The results with the women were practically the same. Of the tabes cases, 35.9 per cent. were surely syphilitics, 35.9 per cent. were suspicious, and 28.2 per cent. gave no evidence whatever of previous venereal disease. For the non tabetic women the percentage in these three classes was 6.5, 10.5, and 80 per cent. respectively.

It will be at once apparent that the paper adduces indisputable proof of the important rôle played by syphilis in the ætiology of tabes. With an unmistakable history of syphilis more than five times as frequent in tabetics, and no venereal history what-

ever nearly four times as frequent in the non-tabetics, only one conclusion is admissible.

Medicine, February, 1899.

Malignant Syphilis.—In a discussion of a case of hereditary syphilis which had destroyed the nose, spine, and part of the bone floor of the skull in a child of six years, Besnier recommended pushing mercurial treatment to its utmost limits, and going far beyond the normal doses. Fournier reported cases rebellious to injections of calomel and inunctions, but cured by combining the two, and Galezowsky related the case of an officer dismissed from the service on account of atrophy of the papilla of the optic nerve, who took Hg inunctions every day for two years, and recovered his sight completely. In such obstinate cases potassium iodide detracts from the benefits of the mercurial treatment, and should be omitted.

Munich med. Woch., January 10th.

UNDER what circumstances do pushing down pseudo-membrane and occlusion of the tube take place in O'Dwyer's intubation, and of what importance are these complications? A. Hand, jun. ('Ann. of Gyn. and Ped.', January, 1899), discusses the danger of pushing down membrane before the tube in intubation. Practically this happens very rarely, though the writer always follows O'Dwyer's advice to be prepared for a tracheotomy in every case. The reasons why this seemingly likely complication so seldom occurs are (1) because the edges of the lower end of the tube are rounded, and, with the obdurator properly introduced and retained, present a completely rounded extremity; (2) because thick pseudo-membranes of much extent are extremely rare, while thinner membranes can pass through the tube so easily that their detachment is of little importance; (3) because thick membranes, when formed, generally have their origin below the vocal cords, so that the tube easily penetrates their lumen,—still more is this the case if they happen to be attached to the cords; (4) because the ominous difficulty of breathing is not dependent upon the fibrinous exudate alone, but on the subglottic swelling as well.

Obstruction of the tube is rare (1) because thick membranes are rarely extensive, and thin ones can pass through the tube; (2) because if the croupous process does not rapidly descend, the fibrinous exudate may be dissolved by rational treatment and expectorated in the form of a slimy secretion.

American Gynecological Journ., Feb., 1899.

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THE ANNUAL ADDRESS

DELIVERED BEFORE THE MEMBERS OF THE

NORTH-WEST LONDON CLINICAL SOCIETY,

October 27th, 1898,

ON THE RATIONAL TREATMENT OF GOITRE,

By VICTOR HORSLEY, F.R.S., F.R.C.S.,

President of the Society.

GENTLEMEN,—In thanking the Society for the honour they have done me in electing me as President, I have thought that I could best choose as my subject for the presidential address a question which I, perhaps in the fervour of youth, consider has now received a practical answer—namely, the treatment of goitre. It is quite likely that my general views will receive sharp criticism from many, but they are based at any rate on practical clinical experience.

Now before we speak of the rational treatment of goitre we must make up our minds as to what we mean by the word "goitre," because undoubtedly, when I was a medical student, I was taught that a large number, if not the very great majority of goitres were instances of parenchymatous enlargement; that means to say, a general diffuse enlargement of the whole gland, such as we see in the photograph I now show you. Now-a-days we do not teach this to students; we know from frequent operations that parenchymatous enlargement of the thyroid gland is not the commonest affection. It is true that the thyroid gland enlarges as a whole in young women, in anæmic persons, and in middle-aged women who become pregnant, especially towards the end of the child-bearing period, as well as in persons who inhabit certain well-known districts. Such universal enlargement in the first place pretty generally subsides in response to simple treatment; but if it does not it goes on to become a universal cystic enlarge-

ment, multiple and small cysts developing from the enlarged acini. Now-a-days the majority of cases that present themselves appear to be not cases of parenchymatous enlargement, but of adenomatous enlargement; that is to say, an adenomatous growth, which may start in any one portion of the gland, and by its gradual extension come to resemble clinically the old condition, which was diagnosed as parenchymatous enlargement. Further, such adenomata, when they commence as localised tumours, very often become cystic; and then we have what used to be described as a cyst in the thyroid gland—*i. e.* a single cyst in the thyroid gland as distinguished from the multiple cysts in the cystic or parenchymatous enlargement. Such cysts have thick walls and brownish chocolate-coloured contents, and have a special tendency to contract adhesions to surrounding parts.

The next class of case that I have to allude to is one which has always been clearly recognised—the dreadful condition of malignant goitre. What do we include under the term malignant goitre? When I was a student it was practically considered that there was one form of malignant goitre; that is to say, the epitheliomatous carcinomatous tumour. Now things have a little altered. In the first place, take the ordinary carcinomatous tumour of the thyroid gland. Where does it start from? I was always taught that it began in the epithelium and acini of the gland. We have now gone considerably further. We know that in the development of the thyroid gland there are not only the elementary acini of the gland, but there is also the structure, which is called the parathyroid, the structure which is epithelial in origin and in appearance, which never develops true cysts, but which remains to the end of its days an embryonic epithelial mass.

I will now show you a photograph of the parathyroid in the monkey under a high power, showing the parathyroid and some of the neighbouring thyroidal acini with normal colloidal material. It is a very vascular structure. This slide shows you a preparation of the blood-vessels of the parathyroid in the guinea-pig. It follows from these facts that it is a highly vascular epithelial organ, and therefore just one in which it might be expected that malignant growths would develop, as we now know that they do. Constantly the section of

malignant tumour of the thyroid resembles the parathyroid so closely as to suggest that the growth sprang directly from an embryonic remnant in which the malignant goitre had arisen, rather than, as one used to be taught, it had found origin in the true thyroidal structure. Before I pass from this mere enumeration of the kinds of goitre, I wish to allude to one more form of malignant goitre, because that also has been discovered since I was a student. I refer to the form which is known as general thyroidal malignancy. It is, perhaps, the most interesting neoplasm that we know of.

As an example, imagine a case presenting itself clinically as a single adenoma of the thyroid. You remove it under that belief, and when you examine it microscopically you cannot distinguish it from true adenoma of the thyroid. Nevertheless the patient may present herself again (for they are usually women) with a local recurrence. These are sections from such a case which has lately passed under my care, and they show the local recurrence, the thyroidal growth, with colloidal material in the acini, and have a very characteristic appearance. This poor woman was admitted into the Queen Square Hospital for paraplegia, and the question arose, to what was the spinal disease due. It was obviously a case of compression from disease of the bones, which were the seat of either a new growth or destructive caries. At an exploratory operation I found that the bony tissue of the sixth and seventh cervical vertebræ was permeated by a new growth. On microscopical investigation this proved to be one of these extraordinary instances of general thyroidal malignancy. Thus, what is found in the bone is practically normal or only slightly exaggerated thyroidal tissue, with cubical epithelial lining of the acini and colloidal contents undergoing the customary vacuolation, and in every way resembling the thyroidal tissue. Under a higher power the growth in the bone is seen attacking a piece of the trabecula of the bone, and here is the epithelium lining the acinus and the thyroidal tissue. So that we have to extend our knowledge of malignancy as affecting the thyroid gland by adding firstly the assumption that in some cases a malignant growth starts not from the thyroid gland but from the parathyroid, and secondly, that occasionally we may meet clinically with the extraordinary condition of multiplication of thy-

roidal tissue in other parts of the body, which, though apparently but a reproduction of normal tissue, is a highly malignant condition which must terminate fatally. Since this case I have seen another, namely, at University College Hospital, in which not only were there recurrences in the bones but also in the lungs. These two cases happening in the same year show that this form of malignancy of the thyroid gland is perhaps not so rare as we have hitherto believed it to be.

As regards exophthalmic goitre, I need not say a word about the recognition of it; it stands to-day exactly where it stood twenty years ago, with this exception, that I think now-a-days it is recognised to be a formidable disease, which, however, is more successfully treated than in the past.

This terminates the list of goitres which we meet with.

Next we have to consider the rational treatment, the recommendation of which embodies in the highest degree our responsibility to our patient, and which responsibility was great twenty years ago; but I think now, by the modern surgery of Lister and Kocher, is reduced to a relatively small figure.

If the gland is enlarged in response to some physiological call upon it, as in anæmia and pregnancy, where the hematopoietic function is maintained under unusual circumstances, it is obvious such cases are improved by iron in a very simple way. Further, also, it is obvious that some of these cases may be improved by the administration of thyroïdal substance, because by such a procedure the gland which is having too great a call made upon it can be supplemented. But I cannot pass by the old-fashioned empirical treatment by iodine without a few more words.

Iodine once enjoyed the reputation of diminishing all enlargements of all glands, and therefore was applied to the thyroid gland. But the most recent researches, especially of Baumann, showing that the thyroid gland contains a preparation of iodine in its secretion, suggest that after all the internal treatment by iodine or its preparations is a real treatment, however empirical it may be, and that it acts by supplementation. In speaking of iodine, let me at once dismiss the question of treatment of goitres by injection of iodine. It is quite possible that in the hands of some observers this has been relatively a successful treatment.

I can only say that in my own hands it has not been successful. It is well known that it has been in the past a dangerous treatment. I am glad to say I have never seen an unfortunate result of the injection of iodine into the gland, but I showed some time ago that the dangers of injection of iodine can be fully explained experimentally by the passage of the injection fluid into a large blood-vessel. If the method is still used, then modern experience of the results of injection of goitres with iodine ought to be published. I believe it should be regarded as a barbarous method, and one which should be wholly given up.

I will say, therefore, in speaking of the rational treatment of goitre, no more concerning the injection of iodine, although it may come up again in relation to the question of cystic goitre.

We will turn at once to the question of the treatment of simple adenoma. Adenoma of the thyroid gland of course resembles normal thyroïdal tissue in every particular; we have, in fact, more or less globular acini lined with characteristic epithelium and filled with colloidal material, and their appearance is constant in a simple adenoma, no matter what may be the size. The section I now show you was taken from a large adenoma which reached to the size of a cocoanut, and sections through any part of its structure showed the same appearance as you see upon the screen. Obviously we should say, if we had not melancholy experience of general thyroïdal malignancy to prevent our being too absolute, that it was a very innocent tumour. Such a simple adenoma grows within the gland, and extends at the cost of the surrounding gland, which it compresses, and it is by reason of this condensation of tissue in its neighbourhood that it may always be recognised by the naked eye as sharply distinct from the rest of the gland. It may ultimately cause atrophy of all the rest of the lobe in which it has taken up its growth. Owing to this manner of growth the rational treatment of adenoma is the simplest in surgery, and consists in the operation of shelling out.

A few further minor details in relation to the growth of adenomata are worth drawing your attention to, especially with regard to diagnosis. In the first place, a simple adenoma commencing in the posterior part of the lateral lobe will often give rise to no obvious symptoms, and not present

itself on the neck at all. The patient will only be aware of a slight increase of size in the neck. But one day, on turning the head, it will slip from the edge of the sterno-mastoid, and the patient will then notice that there is a tumour in the neck. This dislocation of adenomata has been taken advantage of surgically to render the operation easier. But there is another movement of adenomata which is of more importance, and which produces serious symptoms, and that is when the adenoma growing from the under surface of the lateral lobe thrusts downwards into the mediastinum, and presses heavily upon the trachea, constituting the "goitre plongeant" of French authors.

Next with regard to the anatomical and physiological conditions of adenomata, I should like to point out that the vascular supply of an adenoma is of very little importance; its arterial supply as a rule comes from small branches of the thyroid artery lying in the depth of the gland, and the veins which issue from it are purely superficial. These anatomical considerations of adenoma render the shelling-out operation easier than the reputation of goitres a few years ago would have led one to believe.

Passing next to the cystic form of goitre, the cystic condition is worth discussing, from a practical standpoint of treatment, under two heads.

In the first place, parenchymatous goitre may be cystic. Under these circumstances we have multiple cysts. The only treatment for a goitre of this kind is excision of the whole mass. This, of course, may lead to the necessity of excising the whole gland—a necessity which would always be regrettable, but may sometimes exist. The constitutional effects of total excision can be readily guarded against by the continual administration of thyroid for the rest of the patient's life.

Fortunately, generalised cystic degeneration of parenchymatous goitre is not nearly so common as the simple cystic degeneration.

In respect of the diagnosis of cystic conditions, I would like to point out that determination of the existence of a cyst by palpation can only be arrived at by palpation with four fingers upon the tumour, unless the tumour is steadied as far as possible from every direction the knowledge gained by slight pressure upon it is of no value, and may be

confused with that derived from a solid tumour in the neck. Having detected it, we must now abandon entirely the former treatment by (1) drainage, which usually results in suppuration, or by (2) injection. Aspiration leads only to refilling, and the operation subjects the patient to annoyance, if not risk of something else. Drainage is a very distinctly dangerous method of treatment, because of the liability to septic infection of the tube, and subsequent development of adhesion of the cyst wall to surrounding parts. I have been called upon, of course, like many other surgeons, to remove cysts which had been drained and not cured, and such operations often are extremely difficult from the numerous adhesions in the neighbourhood of important structures set up entirely by the septic conditions which the cysts were subjected to. Shelling out of such a cyst, on the contrary, is like shelling out the original adenoma from which it sprang, and is an extremely easy and simple operation.

Passing next to the question of malignant disease, there can be only one treatment, in the proper sense of the word, of a malignant growth, and that is early excision. I would only like to impress upon you the necessity of removing the whole gland, although it may appear to you at the time of the operation that only a portion is the seat of the malignant disease. A case of the kind occurred to me two years ago, in which I had to deal with an obviously malignant growth in the right lobe of the thyroid gland in an old lady. The tumour, on examination and free exposure of the parts, was quite distinct lying in the right lobe, well divided off by a natural furrow across the isthmus from the opposite lobe. To all intents and purposes the two lobes were quite as wide apart as they are in some of the ruminants. But following the rule which I believe was originally laid down by Professor Kocher, that the whole gland should be removed in cases of malignancy, I removed the left lateral lobe, although it seemed quite healthy. On making an incision into it afterwards, however, I found a small nodule of malignant growth, which could not have been detected through the outside. By alone adhering to the principle could the case be treated successfully, and the patient is still alive. If I had trusted to ordinary inspection, that patient would now have been dead. Total excision, therefore,

of the gland in a case of a malignant growth is the only treatment that can be advised.

If the patient comes too late for the operation of removal, carcinoma of the gland, as we know, tends to infect the surrounding parts, and to become hopelessly adherent to the trachea and other structures in the neck. And then we are naturally brought face to face with this difficulty, are we or are we not, as a means of palliative treatment, to perform tracheotomy? I think it is a very open question, gentlemen, this question of early tracheotomy in hopeless malignant disease in the thyroid. With true carcinoma, the growth proceeds so rapidly that the tracheal tract becomes encroached upon, and I believe that, certainly in one case that I have seen, the sufferings of the patient after tracheotomy were in no wise diminished, but perhaps increased by the operation. But every case must be treated on its own merits. In another case that I saw at University College Hospital the obstruction could only be relieved by a very long tube, and that proved to be due to the fact of an accessory thyroid gland in the chest becoming the seat of enormous malignant growth pressing upon the bifurcation of the bronchi. You may meet, in fact, with any complications, sometimes of very great severity, in such cases. Therefore no general rules can be laid down with regard to treatment of such conditions, the treatment in each instance must be decided on the prominence and urgency of particular symptoms.

I now come to speak of exophthalmic goitre. I suppose at the present time we are all agreed that an early case of exophthalmic goitre can be perfectly cured by electrical treatment and by internal medication. What are we to do with the severer cases which tend to terminate fatally. Surgically speaking—and these cases are referred to the surgeon now—we are asked to remove a portion of the gland, or we are asked to ligature the vessels. My own experience of ligaturing the vessels is that it produces very little or no effect. Removal of a portion of the gland produces a most striking and remarkable effect; in fact, you may say that in many cases it will produce a cure. But in proposing it to the patient you will have to mention the question of risk. Unquestionably the risk in removing a lobe of an exophthalmic goitre is considerable. What is that risk due to? I will not worry you with a repetition of the discussion on this subject.

I have already drawn attention to it three years ago in connection with a fatal case which I published in the 'British Medical Journal,' and the subject has also been very ably treated by Mr. Paul, of Liverpool, in the same journal. One would simply sum up the prevailing views at the present time by saying that there are two distinct theories, according to the first of which the fatality is due to interference with the function of the remaining portion of the gland, with the result that the patient dies of acute thyroidism; the other explains the death of the patient to be due to absorption of the toxic secretion of the gland.

In this connection it is important to study a microscopical section of an extreme case of exophthalmic goitre. I will show it to you, so that you may compare it with that of the adenoma which I showed you before, because there it is obvious that with the thyroid gland in such a state the patient must be regarded as being in an athyroidal condition, since there is no trace of normal colloidal secretion. The acinous cavities are filled with a watery fluid, which is not normal thyroid secretion; and therefore to all intents and purposes a patient in that condition is practically in the same state as a patient in severe myxœdema, where the normal acini of the thyroid gland have been destroyed by inflammation, at any rate by leucocytic infiltration producing atrophy of the secreting substance and loss of colloidal material. In severe exophthalmic goitre, therefore, the patient is certainly in an athyroidal condition, and it becomes a question whether, before we adopt operative treatment on the gland, we ought not to partly prepare the patient beforehand by treatment with thyroidal substance. The only drawback to that is that these patients are very sensitive to the administration of thyroid, that they suffer from the administration of a very small dose, suggesting that there is some perversion of the secretion of the gland in exophthalmic goitre which we cannot explain. In advising the patient to submit to excision of a portion of the gland as the only rational treatment, we have to put before him that it is possibly a dangerous operation, and we must take into consideration the general constitutional state of the patient, the existence of emaciation, of extreme tachycardia, and so forth.

I wish to conclude by a few observations upon the operation of "shelling out," because this is

the operation which ought to be performed in the majority of cases in the treatment of goitre.

If a patient comes to you with goitre, which you can diagnose to be adenoma or simple cyst, no matter how large it is, you ought to be able to tell that patient that that tumour can be removed, and that he will be perfectly well in a week. I say you ought to be able to tell the patient that because of the success which attends obedience to the rules which have been formulated chiefly by Professor Kocher. The circumstances of every case are practically these. The connective tissue septa of the neck are extremely thin; the muscles, trachea, blood-vessels, and nerves are all compressed together in the normal neck, and they are only separated from each other by thin planes of connective tissue. A goitre is nothing more than a foreign body pushed into one of these planes, and if you extract the goitre you ought, by adequate pressure, to succeed in obliterating all the cavity of your wound. To quote my own method, I carry out the ordinary preparation of the skin by disinfection twenty-four to forty-eight hours beforehand. The anæsthetic I always use is chloroform, and that in very small doses. The patient should be first made completely unconscious with chloroform, and then only just enough chloroform given, at short intervals, to keep the patient unconscious of pain. The object of this is that in all these cases of thyroidal disease there is a perverted and excessive mucous secretion from the mucous membrane of the respiratory tract. I will not go into the physiology or pathology of that, but will ask you to take it as a fact that as a result these patients are sensitive to chloroform, which produces a hyper-secretion of mucus. But with small doses of the anæsthetic there is no trouble. While speaking of anæsthesia, I should like to say that one is often asked by the patient whether there are any special dangers and risks from the effect of the chloroform as the growth presses on the trachea. I have never seen the slightest difficulty or risk in the operation on any goitre, however great the size of the tumour. I believe that if the patient's neck be not thrown back too far, so as to produce undue tension in the neck at the commencement of the operation,—and there is no necessity for having the head thrown far back,—you do not bring about any awkward compression, or what is

more important, any reflex disturbance of the vagus nerve.

In the next place, as regards hæmostasis. When I was a student the operation was looked upon as dangerous owing to the loss of blood. Now-a-days with a large number of Wells forceps there should be no loss of blood. In fact, there is no need to speak of undue hæmorrhage. In clamping the various thyroidal arteries care must be taken not to include the recurrent laryngeal nerve. Clamping of the veins is carried out as they are met with. There should be no extravasation of blood in the connective tissue, every vessel should be clamped as it comes into view and before it is divided.

As regards rapid union of the wound, there comes in a little practical point. With a large goitre you have frequently to cut across the hyoid-dean muscles. If this is done, I urge that they should not be sewn together again, as one is often tempted to do. If you do they become adherent to the scar, producing puckering of the scar, which is unsightly; but leaving them alone, the gap between the cut edges is filled up by flat scar tissue, and abstaining from stitching in nowise interferes with efficiency of the muscles or the cosmetic appearance of the neck afterwards. The larynx rises and falls just as well after as before the operation, however much you may have to divide these muscles. The wound ought to be healed by a week, and the connective-tissue planes should be firmly adhered in three days. This result can be attained easily by the employment of drainage (for twenty-four hours only), by sewing up the external wound—that is to say, the skin only—by passing a temporary stitch through the drainage-tube aperture, and then the application of a moist antiseptic dressing, so as to produce a gentle compression of the field of operation. The next day the drainage-tube should be removed and the temporary suture tied. At the end of a week the stitches can be taken out.

Lastly, there remains the practical question of prognosis to the patient. What is the prognosis of cure? This question is continually asked in relation to goitre, because the unfortunate appearance of the deformity causes the patient to dread its recurrence. I think myself that the patient can be always assured that if it is a single simple adenoma or a simple cyst, which is practically the same thing, there will be no recurrence,

But if there is evidence of multiplicity, as not infrequently happens, the prognosis must be guarded. If during the operation it appears that the condition is not a simple one but a cystic parenchymatous degeneration, the patient should be told it will certainly recur (though sometimes very slowly) unless leave be given to remove the other lobe. However, that is a matter for the patient to decide, but it will be for you to say whether the rest of the thyroid gland will be affected. Personally I believe it always becomes more or less affected, but in the majority of cases it does not call for interference because, for some reason, the removal of one half, as is well known, produces a diminution in the other half, and even if it is the seat of disease the diminution may bring the patient, perhaps, beyond the climacteric for instance, when the function of the gland is of less importance to the whole organism, and there is not the same call upon it or stimulus to physiological activity.

The prognosis in cases of removal of the malignant disease depends entirely upon the possibility of removing the gland without leaving any mischief outside its capsule. If the surgeon is satisfied at the operation that he has been able to remove the whole gland, and that at no spot has the growth escaped from the capsule, he may promise there will be no recurrence. The reason appears to be this, that there is no early infection of the surrounding lymphatic glands,—I mean that the ordinary lymphatic glands become infected only after the growth has got through the capsule. Of course we know that in malignant disease of the breast the glands are affected very early, and some would say that the breast has no proper capsule. As a matter of fact it has virtually as much capsule as the thyroid gland has, but as a matter of practical judgment for the information of the patient, I repeat that if you can tell the patient that there has been no escape of the original growth through the capsule of the gland, you may equally tell him that he is cured of the disease. On the other hand, if there has been the slightest escape, I think you should say that you can give no opinion at all.

I am afraid my remarks have been of necessity somewhat scattered in an address of this kind, but I felt that at any rate they would bear upon clinical difficulties which must have occurred to all of us,

and I hope, therefore, you will receive them from that point of view.

[Mr. Durham proposed a hearty vote of thanks to Mr. Horsley for his address. The subject of the lecture was one which Mr. Horsley had made his own, and they had all learnt very much from the remarks he had made. The motion was carried by acclamation.]

An Enema for Urticaria.—In the 'Clinica moderna' for December 21st we find the statement that in severe cases of urticaria benefit has been found to follow the use, four or five times a day, of an enema having the following composition:

℞ Sodium bicarbonate	...	300 grains.
Laudanum	...	30 drops.
Boiled water	...	7,500 grains.

M. As improvement takes place, the amount of sodium bicarbonate may be reduced gradually to seventy-five grains.

Pastilles for Fœtid Breath.—The 'Journal de Médecine de Paris' gives the following:

℞ Powdered coffee	...	675 grains.
Vegetable charcoal	...	225 "
Powdered sugar	...	225 "
Vanilla	...	225 "
Mucilage of Senegal gum	q. s.	

M. Make into pastilles, each containing fifteen grains. Five or six may be taken daily.

Iodine in the Treatment of Infantile Gastro-enteritis.—Bizine ('Semaine Médicale,' 'Revue des maladies de l'enfance,' January) reports particularly favourable results from the use of this mixture:

℞ Emulsion of castor oil	...	6 ounces.
Oil of peppermint	...	3 drops.
Oil of cloves	...	5 "
Tincture of iodine	...	10 "
Chloroform	...	2 "

M. S.: A teaspoonful every hour. The mixture should be kept on ice, to prevent decomposition. One bottle is usually enough, but if after that has been taken there is still a little diarrhoea, ten grains of iodized starch may be divided into six doses, one to be taken twice a day.

N. Y. Med. Journ., Feb. 18th, 1899.

THE INTRODUCTION OF COCAINE THROUGH THE UNBROKEN SKIN BY MEANS OF AN ELECTRICAL CURRENT.

BY

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THE production of anæsthesia locally by cocaine introduced through the unbroken skin by a process of electrical osmosis has certain useful applications in minor surgery. For example, in the small electrolytic operations necessary for the removal of facial blemishes, such as moles, superfluous hairs, and small capillary nævi, this mode of sparing the patient pain without recourse to general anæsthesia is very useful. The introduction of various drugs into the system by electrical means is by no means new, but for most drugs it presents few advantages over the ordinary method of administration by the mouth. But the discovery of cocaine has given the method a new interest, so that it becomes useful to know how to produce a local anæsthesia by the aid of a battery. Dr. Morton, of New York, who has recently brought out an admirable book * upon the whole subject of the introduction of drugs through the skin by electrical means, has published a formula for a solution of cocaine in guaiacol with which it is much easier to produce a local anæsthesia than it has been hitherto with watery solutions. The solution he recommends consists of cocaine (the alkaloid, not the hydrochlorate) six grains dissolved in a drachm of guaiacol. If a little of this mixture upon a piece of blotting paper be placed on the skin, and a current applied through it, the cocaine quickly penetrates, and an anæsthesia sufficient for the purposes which I have indicated can be produced in about four or five minutes. The positive electrode should be placed on the blotting paper. It should consist of a flat disc of bare metal of suitable size. A platinum surface is the best, but tin or any other metal which does not easily become corroded will do almost as well. Care must be taken that the metal itself does not touch the skin at any point. The current is then turned

on until it reaches about four milliampères for an electrode half an inch in diameter. At first from ten to fifteen cells are necessary to produce this current, for the solution has a high resistance; but soon conduction improves, and the number of cells may be reduced. A slight pricking pain is felt during the first minute of the application, but this gradually passes off, and its disappearance indicates that the drug is commencing to act. It is not necessary to prolong the action more than four or five minutes, nor should the current much exceed four or five milliampères for the above-mentioned size of anode, because the solution itself is slightly caustic, and may produce a superficial irritation if applied for too long a time. The blotting paper being removed, and the part wiped with a tuft of cotton wool, the operation may be commenced. Common sensation may not be entirely done away with, but the perception of pain is so nearly abolished that the patient will bear the introduction of a needle with calmness.

Recently I operated for the removal of a small nævus from the upper eyelid of a lady. The nævus had been treated by myself once previously without cocaine, but owing to the pain having been rather severely felt the operation was prematurely stopped, and the result was not completely successful. On the second occasion I applied the cocaine in the manner just described, and was then easily able to perform the electrolysis without hurry, the patient suffering no pain. She said that the difference in comfort to her between the first operation and the second was most striking. Other cases in which I have used cocaine in this way were for the removal of a mole from the cheek of a little girl who bore the operation without a sign of pain; for the removal of four nævoid spots from the face of a boy; for the destruction of a small nævus by galvano-cautery in an infant; and for an exploratory puncture of the chest-wall in a young child. In this last case the child certainly showed signs of distress, but according to the operator very much less so than on the occasion of a previous puncture without the cocaine. Without claiming more than a little for this method of using cocaine, I would say that in the kind of case which I have indicated it offers a means of saving the patient from a certain amount of pain, that I have found it certainly useful, and that I now employ it regularly.

* Cataphoresis, or Electro-Medicamentary Diffusion.

CHAPTERS FROM THE TEACHING OF DR. G. V. POORE.

No. XVI.

GENTLEMEN,—In the case of Maybrick, who died of arsenical poisoning in Liverpool, there are a few points worthy of attention. Among the symptoms from which Maybrick suffered was a persistent cough, and he complained that he had a hair in his throat, and several witnesses bore testimony thereto. Post-mortem there was found a slight ulcer on the epiglottis, which was caused by a little drop of arsenical solution, or possibly a minute speck of white arsenic lodging on the epiglottis, and causing irritation. Then the state of the stomach was a matter of controversy. It will occur to you that the state of the stomach in a case of arsenical poisoning must be variable. It is not reasonable to suppose that you can take a stomach and say "that is the stomach of arsenical poisoning." If the arsenic has been given in the form of a powder you very often find on the stomach minute specks of powder, and each speck may be the centre of a little inflammatory area. In arsenical poisoning also petechial ecchymoses have been seen. One of the witnesses for the defence said that in every case of arsenical poisoning you were bound to find the condition of petechial ecchymosis in the stomach. Granted you very often do, but to say you are sure to find such a thing is not reasonable. Every witness for the prosecution and for the defence in that trial admitted that the patient died of gastro-enteritis, and every witness for and against in that trial admitted that the post-mortem signs were those of gastro-enteritis. Now gastro-enteritis is not a thing which comes of itself; it is a condition which is caused by some irritant,—it may be an organic irritant, it may be a mineral irritant.

Then there was found evidence of inflammation of the duodenum for a little way down. Then the rest of the intestinal tract was normal, and there was some ulceration of the rectum. I would again insist that the large intestine is probably a very important channel for the elimination of poisons, and that the escape of the great mass of the intestine from injury points to the fact that

whereas absorption takes place in the upper part, elimination takes place very largely in the lower part. I shall have to read to you presently a case in which exactly the same state of things was found in poisoning by antimony. You will find the same thing is found in some forms of mercurial poisoning. You must remember that if a person vomits incessantly, hæmorrhage very often takes place. I remember once going from Dover to Calais in a very bad sea; I have the good luck to be a good sailor. It was a very bad passage, and I made a sort of clinical tour around the ship. I think I am right in saying that quite 10 per cent. of the people on board brought up a little blood. That is a fact well worth knowing. Some of the passengers brought up large quantities of blood. I take it the hæmorrhage in such cases comes from the squeezing and bruising, and that if a person vomits almost incessantly for a couple of hours, as those unfortunate people did, it is not surprising that a little blood should occur; and if the liver should be a little congested because of the supper the previous night, or something of that kind, I think it is reasonable to suppose there would be a little blood. My point is that you may find evidence of hæmorrhage into, and damage of the stomach from the incessant act of vomiting, quite apart from the action of any irritant upon the mucous membrane.

I have here a case reported by Dr. Thomas Oliver, of Newcastle, who gave to a dog a grain of arsenious acid daily for several months. The animal gained considerably in weight, and became sleek and well and covered with hair, which before was stunted and stubby, but now became silken. All who had seen the dog before were struck by its improved appearance; that is a very interesting fact in reference to the use of arsenic for cosmetic purposes. Before its death the dog suffered from vomiting, diarrhoea, and rapid emaciation, and post-mortem the most careful chemical examination of the liver and bones, repeated on three occasions, failed to reveal arsenic.

We now come to the question of the fatal dose. There seems to have been a case in which death took place with as little as two to three grains, but much bigger doses have been recovered from. As to the fatal period, how can we speak on that point? Madeline Smith's lover probably got his fatal dose somewhere between midnight and two

in the morning, and he died at nine or thereabouts. But he got 82 grains at least, probably a great deal more. Then there is a case recorded by Dr. Foster of Huntingdon, in which a child died in two hours after taking arsenic. As I have warned you, you cannot lay down any absolute rule on these matters. Maybrick lived for a week, and he had probably been having poisonous quantities of arsenic from the beginning of April, more or less, until the middle of the second week in May. The meaning of that is that no sudden death and no very prolonged illness would of itself exclude arsenical poisoning. If a person gets a dose of arsenic, and he has a fatty heart, and he vomits and faints, he may die in a minute or two in the first attack of vomiting. On the other hand, we see that he may live for a long time.

The treatment, of course, is to empty the stomach and wash it out; you may give demulcents, and it is said that the hydrated sesquioxide of iron, freshly made by mixing the liquor ferri perchlor. with solution of ammonia, and collecting the precipitate and giving it suspended in water, is one of the best antidotes. In your efforts to get the stomach empty you must take care not to check the action of the bowels. That is a mistake that is often made. I am not meaning to criticise, but it is obvious that if the diarrhoea is due to arsenic, one of the worst things you can do is to stop it. You will do better to run your risk and let the mineral poison be turned out as quickly as may be.

Next, as to the tests for arsenic. I have here some white arsenic, and I will put some into this mortar and pound it up. That white powder might be anything—it might be chalk, starch, or an alkaloid even. In dealing with a white powder, I proceed as usual, and place some on platinum foil and heat it over a spirit lamp. Notice first of all that this powder does not melt; it fumes and volatilises without melting, and it does not inflame nor blacken. It is gone, and the foil remains almost unsoiled. Therefore that white powder is not chalk, nor zinc, nor magnesia, nor tartar emetic, nor acetate of lead, nor an alkaloid. We shall see how tartar emetic behaves shortly. Perhaps, as arsenic and antimony go together very much, I may just take a speck of tartar emetic and volatilise it on a piece of platinum foil. Tartar emetic and arsenic cause symptoms which are very

similar, and we shall find that chemically also we have to carefully consider the diagnosis of the two. You see this tartar emetic flies about, gets red-hot, and blackens, leaving a residue on the foil. Next, to the arsenic we apply some reagent. One of the first reagents I will apply is ammonium sulphide, and you will notice that in that at first there is very little change, but apparently solution takes place, and you will find after a little time that it will dry up and leave a yellow stain. I will put alongside that a little tartar emetic, and immediately I get what looks like black. The books say it is orange, but it is more like blood-orange gone rotten. Still, if you look at the more diluted points it is perhaps orange. You see the antimony has changed instantly. The patch of arsenic is first of all dissolved in the ammonium sulphide, and it leaves a little orange-yellow rim where it is evaporating. The books say antimony is orange and arsenic is yellow. We will now try another test. A solution of potash would dissolve the arsenic and cause no change of colour; and of course with perchloride of mercury you would get a yellow colour, and with the subsalt of mercury you get a blackening on the addition of potash. Putting our arsenic into a tube, you will notice that on adding water and shaking it in the cold the solubility is very slight; and on boiling, the solution is not much increased; but on the addition of an alkali such as potash you get a solution taking place tolerably quickly. You will notice that on boiling, some of the arsenic stops at the bottom, and a good deal of it is floating on the top of the liquid. I put it through a filter-paper and get a clear solution. Here I have a solution of white arsenic in water pure and simple, so that the amount of arsenic is very small. Proceeding in the usual way, I have a liquid without odour or colour; perhaps it gives a slight reddish tinge to the blue litmus paper. I add to this a drop of hydrochloric acid, and get no precipitate. And then adding some sulphuretted hydrogen, I get a yellow precipitate. Adding to that some ammonia, I get it dissolved. Therefore we get a yellow precipitate in an acid solution, which is dissolved on the addition of ammonia. That shows us that we have to deal with arsenic. Persalts of tin give a yellow precipitate, which is insoluble in ammonia.

There are two other special tests for arsenic which are very good. One is ammonio-nitrate of

silver, and the other is the ammonio-sulphate of copper. Now a word of caution. You saw that that yellow sulphide of arsenic dissolved readily in ammonia, and taken as a broad fact, you must remember that any excess of alkali in dealing with arsenic will keep it dissolved. So that in making ammonio-nitrate of silver and the ammonio-sulphate of copper you must be very careful that there is no excess of ammonia present, and on that account the ordinary ammonia solution is too strong to make your ammonio-sulphate of copper or ammonio-nitrate of silver. So I take the ammonia and freely dilute it. Then I take two test-tubes, and into one of them I put a little solution of nitrate of silver, and into the other I put a little solution of sulphate of copper; to each of these solutions I add some diluted ammonia cautiously and slowly. By adding this to the silver solution I get a brown precipitate, which is redissolved on adding more ammonia. I treat the copper solution in the same way. You will see that there is a deep blue colour. Now I take my arsenical solution, which is very weak, and put a little into two tubes. The ammonio-sulphate of copper gives a green precipitate, and the ammonio-nitrate of silver gives a pure yellow precipitate. So that I have a clear solution, without odour or colour, leaving no residue on evaporation, not blackening, giving no precipitate with hydrochloric acid, but a yellow precipitate with sulphuretted hydrogen, soluble in ammonia. And in applying my confirmatory tests I get a green precipitate with the ammonio-sulphate of copper, and a yellow precipitate with ammonio-nitrate of silver.

Two New Symptoms of Chlorosis.—Splenalgia and osteomyelalgia are the names given by M. Golouboff ('La Médecine Moderne, 1898, No. 39) to two symptoms in chlorosis to which he believes he is the first to call attention. The first consists of sharp pains in the left side directly over the spleen, usually thought to be due to the pressure of corsets. That this symptom is associated with the usual hypertrophy of the spleen would appear evident. The other symptom consists of pains in the ends of the long bones, particularly the tibia, which are brought out by percussion or pressure. The pains are similar in character to those found in the infectious diseases.

Medical Record.

PYORRHŒA ALVEOLARIS AND ITS RELATIONS TO GENERAL MEDICINE,

BY

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(Continued from p. 319.)

THE IMPORTANCE OF THE STUDY OF PYORRHŒA ALVEOLARIS TO THE PRACTITIONER OF MEDICINE.

ALTHOUGH, strictly speaking, this condition lies within the province of the dental surgeon, yet it should be of great interest to the physician, since upon it may depend many disturbances of the whole system, and particularly of the digestive organs. So much is this the case, that I venture to assert, without fear of contradiction, that the presence of a pyorrhœa may often afford a valuable clue to the elucidation of departures from health which have baffled our powers of investigation.

Pyorrhœa alveolaris may act in three different ways in the causation of disease.

1. The pus with its multitude of putrefactive organisms and decayed food remnants from the pus pockets may be swallowed and—

(a) Act locally upon the stomach wall.

(b) Set up fermentation of the stomach contents.

2. The toxins generated in the mouth may be absorbed directly into the system through the mucous membrane of the mouth or stomach.

3. The presence of the local condition in the mouth may favour the growth and development there of pathogenic organisms, and thus render the patient more liable to contract certain infectious disorders, notably influenza.

Before studying these effects in detail it will be as well to briefly pass in review what is now known as to the micro-organisms which inhabit the mouth.

That these micro-organisms are very numerous we should naturally expect when the following facts are taken into consideration:—The mouth is

constantly in contact with the atmosphere; it offers the necessary conditions for the cultivation of disease germs, viz. warmth, moisture, and an alkaline reaction; and there is usually present in the mouth plenty of culture material upon which they can grow, in the form of food débris, &c.

Such being the case, it is not surprising that we have the following list of micro-organisms which have been found in the mouth, and specially named by those who have observed them.

Bacillus buccalis maximus, *Bacillus α* and *β* (Galippe), *Bacillus prodigiosus*, *Bacillus violaceus*, *Bacterium cerasinum*, *Bacterium coli commune*, *Bacterium gingivæ pyogenes*, *Bacterium termo*, *Diplococcus Hauserii*, *Iodococcus vaginatus*, *Leptothrix buccalis*, *Leptothrix innominatus*, *Micrococcus gingivæ pyogenes*, *Micrococcus ochraceus*, *Micrococcus Reesii* (Rosenthal), *Pneumococcus* of Friedländer, *Pseudo-diphtheritic bacillus* of Roux and Yersin, *Proteus vulgaris*, *Saccharomyces cerevisiæ*, *Sarcina aurantiaca*, *Sarcina lutea*, *Sarcina viridis flavescens* (Rosenthal), *Spirillum sputigenum*, *Spirochæte dentium*, *Staphylococcus albus*, *Staphylococcus aureus*, *Staphylococcus citreus*, *Streptococcus*, *Vibrio rugula*, and Vignal's bacillus.

In addition to the above lengthy list there are many other varieties present in the mouth which have not yet been sufficiently studied to enable names to be given to them. Although many of the micro-organisms of the mouth are merely temporary visitors, yet others are met with so constantly that they must be looked upon as permanent inhabitants.

According to Miller,* in the healthy mouth we meet with chiefly the *Leptothrix innominatus*, *Bacillus buccalis maximus*, *Iodococcus vaginatus*, *Spirillum sputigenum*, and the *Spirochæte dentium*; whilst in cases of pyorrhœa we find in addition the *Micrococcus* and *Bacterium gingivæ pyogenes*. We shall also find *sarcinæ* and streptococci.

Notwithstanding the number of organisms in the mouth, it is a curious fact that it is usually not very septic. And our knowledge of the reasons for this we owe to the important re-

searches of Dr. Hugenschmidt* which were made in Professor Metchnikoff's laboratory at the Pasteur Institute. It was at one time supposed that the saliva possessed a bactericidal action, and it was by this means that the growth of organisms in the mouth was kept within bounds. But that this was the case only to such a very slight extent as to be practically ineffective for keeping the mouth aseptic is proved by the experiments of Sanarelli. Curiously enough these demonstrate exactly the opposite of what they were intended to do by their author. He published them to show that saliva had the power of destroying micro-organisms. He found that saliva carefully filtered through Pasteur's porcelain candles, so as to entirely free it from germs of all kinds, had the power of killing certain pathogenic organisms, notably the *Staphylococcus pyogenes aureus*. But this occurred only with such small quantities that its action upon the germs of the mouth, which according to Miller frequently amount to 1,140,000,000 in an ordinary unclean mouth, may probably be neglected.

The explanation given by Hugenschmidt of the fact that the mouth is not more septic, is found in the reciprocal action of the bacteria and their secretions upon each other. Just as in the larger creation, nature provides against the undue multiplication of any special animal or insect, so in the microscopic world similar agencies are in operation which effectually limit by mutual destruction the inhabitants of the locality. Moreover, actual invasion of the tissues of the buccal cavity is prevented in the healthy mouth by the activity of the phagocytes in the mucous membrane, and by the property possessed by stratified pavement epithelium of continuously shedding and renewing its superficial layers.

Having now a clear appreciation of the micro-organisms of the mouth, we can proceed to study their evil effects upon the system. In doing so we shall follow the scheme already given.

1. *Pus, micro-organisms, and decayed food from the pus pockets may be swallowed.*

It has been thought for a long time that the gastric juice had the power of destroying pathogenic organisms which are introduced into it so much so that Bunge enunciated the theory that the chief object of the gastric juice was the sterilisa-

* Miller, 'Inaug. Diss.,' Berlin, 1887; 'Centralbl. für Bacteriologie und Parasitenkunde,' i, 47, 87; 'Archiv für exp. Pathol.,' xvi, 291, 1882; 'Deutsch. med. Wochenschr.,' x, 395, 1884; 'Die Micro-organismen der Mundhöhle,' Leipzig, 1889.

* Hugenschmidt, 'Dental Cosmos,' 1896, p. 797.

tion of the stomach. But this is far from being the case, as not only theoretically can there be but a very short time during the digestive period when the gastric juice will contain a sufficient degree of free HCl to destroy these germs, but practically it is shown that many do actually escape destruction, since the mouth bacteria constitute, according to Lucksdorff, at least 3 per cent. of those found in the intestinal contents. This noxious material thus swallowed may—

(a) Act locally upon the stomach wall.

It is extremely probable, although it has not been absolutely proved, that some obscure cases of acute gastritis may be set up in this way. Hemmeter* has the following:

"The direct cause of the rarer idiopathic phlegmonous gastritis is unknown. . . . The direct causes, judging from anatomical specimens, are undoubtedly bacterial invasions of the submucosa, principally from pyogenic cocci, which find portals of entry through lesions in the superficial epithelium of the stomach, such as occur in most gastric diseases. . . ."

What more likely then that under certain conditions, when the other factors are present, we may have an acute gastritis set up directly by the material from decayed teeth and pus pockets which has been swallowed by the patient—the conditions necessary being such a low degree of acidity in the gastric juice that organisms escape destruction, and the presence of some slight local lesion through which they can gain admittance to the submucosa.

As regards chronic gastritis, I think that we take it as proved that it is often caused by swallowed micro-organisms and pus. In these cases it is apparently not the micro-organisms themselves, but their irritating products which set up the gastric irritation. This is especially true of the yeasts, which produce excessively irritating products. But these products of fermentation come under the heading of

(b) Fermentation of stomach contents produced by micro-organisms swallowed.

The most important of these are the fermentations produced by the different species of yeast plants, the lactic acid and the butyric acid fermentations. And since we have seen that the lactic

acid micro-organism is invariably met with in connection with tartar and pus pockets, it is easy to understand how useless it is to attempt to cure a chronic gastritis while the mouth is in an unhealthy condition. In these cases the HCl of the gastric juice is usually in defect, and consequently the antiseptic power of it practically *nil*. What good, then, can we expect to do with the small doses of antiseptic drugs which we introduce by the mouth if we at the same time allow the patient to swallow fresh germs with every meal? In such cases I have seen the happiest results follow the use of a weak solution of tincture of iodine immediately before each meal as a mouth-wash.

The power of pyorrhoea alveolaris to produce aggravation of existing gastric trouble reaches its maximum in cases where there is habitual retention of food residues. This happens when the muscular walls of the stomach are in a state of atony, and also when there is some pyloric obstruction which prevents the organ emptying itself. In both these conditions there is eventually produced a dilatation of the stomach, with the result that it is never completely emptied. In the management of this condition one of the chief problems which confront the physician is to limit the fermentative processes upon which so much of the discomfort of the patient depends. With appropriate diet, lavage, and antiseptics, much can be accomplished; but it is easy to see how much more difficult the task becomes if the patient is continually swallowing bacteria and pus cocci with his food. I am firmly convinced that enough attention has not been given to this aspect of the subject by the practitioner, and I am afraid that numbers of cases of atony and dilatation of the stomach are treated in every-day practice without that careful examination of the mouth which should precede all attempts at systematic treatment by the methods enumerated.

It is not improbable that pyorrhoea alveolaris may be a factor in the perforation of a gastric ulcer. Hemmeter* has the following:—

"In the gastric ulcer, however, there is another kind of bacterial infection, which is not accompanied with the signs of active inflammation, and is termed by some authors bacterial necrosis. The process is characterised by the invasion of bacteria, usually in the lower depths of the mucous mem-

* Hemmeter, 'Diseases of the Stomach,' Philadelphia, 1887, p. 470.

* Hemmeter, *op. cit.*, p. 469.

brane, by their growth and subsequent necrosis of the tissue. Although the secretion of HCl. is germicidal to many bacteria, it must be remembered that their spores are not destroyed by it, and that the invasion may take place during the period of rest of the glands in the intervals of digestion, when no or very little HCl is secreted. There is room for the suggestion that the primary necrosis is due to bacteria, and the ensuing ulceration caused by the action of the gastric juice."

A subject which requires working out is the possible relation between pyorrhoea and empyema of the antrum of Highmore. It is not inconceivable that this affection may sometimes be directly initiated by it. We have seen how one form of pyorrhoea commences by death of the pulp, and how in some way the tissues outside the apex of the tooth become infected. If it should happen that the affected tooth was one which projected into the cavity of the antrum, as is sometimes the case, we might readily get an empyema set up.

Again, it used to be a common practice—and possibly still is with some surgeons—to extract a tooth and perforate into the antrum in cases where there was reason to believe that there was a collection of fluid in that cavity. If that tooth were the subject of pyorrhoea it would be almost impossible to prevent infection of the antrum by bacteria carried up by the instrument, however thoroughly it had been sterilised before the operation.

A lengthened experience has convinced me that a very large proportion of the indigestions met with among the poorer classes such as throng the outpatient rooms of hospitals may be explained by the very common occurrence of pyorrhoea alveolaris in these cases. Such patients very commonly neglect their teeth until they experience pain, and many cases of the severest grades of pyorrhoea may be met with in one afternoon's work at any of our large public institutions. What a farce it must be, then, to treat these unfortunate patients, as I have often seen done, by the routine administration of Mistura Gentian. Alk. or Mistura Bismuthi Sed., without even a cursory glance at the mouth to ascertain the condition of the teeth!

There is another way in which pyorrhoea alveolaris may injuriously affect the system, and that is—

2. *By the direct absorption of pus into the system.*

To what extent this occurs is at present doubtful. Some authorities, notably Herschell, believe that gastric neurasthenia may be thus explained. He says: * "There is good reason to believe that many of the chronic indigestions which come under our notice are due to the continual absorption of pus into the system either from a pyorrhoea alveolaris or from an abraded cervix uteri. . . . The subject is at present undecided, and offers a valuable field for further investigation. In such cases we should expect to find some of the other signs of the absorption of toxines, such as pigment spots on the arms, tachycardia, goitre, neuritis of the sixth left intercostal in women, and peptonuria. Urticaria or some other skin eruption may also be present."

Sufficient attention has not been given to the probability that—

3. *A pyorrhoea in the mouth may favour the growth and development of certain pathogenic organisms, and thus render the patient more liable to contract certain infectious disorders.*

It is quite certain that bacteria vary much in virulence at different times, and under conditions which at present have not been worked out. For example, the *Bacterium coli commune*, which we have shown to be a constant inhabitant of the oral cavity, is apparently usually harmless; but under certain circumstances, such as possibly a pyorrhoea or inflammatory condition of the gums, it may acquire virulence, and produce serious disturbances of the organism, such as colitis, dysentery, and cholera nostras. As regards Asiatic cholera, it has been pointed out by Metschnikoff that the specific organism will multiply much more rapidly in the intestine if at the same time there is a larger amount than normal of yeasts and sarcinae. We may take it, then, that the presence in the mouth of abnormal quantities of these fungi should theoretically increase materially the chance of the individual contracting this disease. The lesson pointed out is obvious. In presence of an epidemic it will be a matter of common precaution to have the mouth set right, and any pyorrhoea cured.

A subject which, however, strikes nearer home is epidemic influenza, which apparently we shall have ever with us. It is now almost proved that the specific germs of this affection gain an entrance

* Herschell, 'Indigestion: an Introduction to the Study of the Diseases of the Stomach,' second edition, p. 54, 1895. London: Baillière, Tindall and Cox.

into the system through the mouth. And it is a subject worthy of the fullest investigation to ascertain what effect, if any, a foul condition of the mouth may have in affording them the necessary conditions for their growth. In the meanwhile we shall be on the safe side if we insist on all our patients putting their mouths in order.

HAS THE TREATMENT OF DIABETES MELLITUS IMPROVED?

THE recent review of diabetes mellitus at the Massachusetts General Hospital from 1824 to 1898 would at first glance appear to answer this question in the negative, for the statistics show no decrease in mortality during this entire period. But such a conclusion can hardly be drawn, since any marked changes which have been made in the treatment of this affection have been of very recent date. Even the latest works in English, not excepting that by Williamson, do them scant justice.

It is, perhaps, as much the change in our point of view of regarding the disease as the variations in treatment which are capable of producing better results. The statement occurs so frequently in text-books that diabetes is incurable, that the practitioner loses all his enthusiasm the moment a patient with this disease presents himself. In despair he attempts a palliative treatment, writes out lists of articles headed "allowed" or "to be avoided," and perhaps adds a recipe for some new diabetic flour; while in giving the prognosis the patient's courage is perceptibly lowered.

In contrast to this gloomy picture is the hopeful view, which is best set forth in Naunyn's treatise. The key-note of this is contained in the following quotation:—"That cases apparently severe at the outset, when subjected to a vigorous treatment, take a proportionately favourable course, while others running a severe course are, as a rule, those subjected late or not at all to careful treatment." Coming from so eminent an authority, this statement is of the greatest value, since it counteracts the tone of the usual text-book. Naunyn, in carrying out this idea, says further:—"I consider it an unfortunate curtailment of the physician's task in the treatment of diabetes where

it is said 'the essential task of the physician is to support the invalid in an endurable condition of life for a long time.' In the writer's opinion, a broader, more definite purpose should be put into the treatment, namely this: the strengthening of the deranged bodily function, or at least the checking of further disintegration of the same." This change of view is our first step toward an improved treatment for diabetes.

That drugs are of little value in this disease is now generally admitted. Notwithstanding this, there are few of us who have not heard from eminent sources that considerable success has been obtained from this or that remedy. Patients like the idea of taking medicine, and come to the doctor for the sake of the prescription, just as they often go to church for the music. The advice as to the diet and hygiene, either of the body or soul, forms too commonly an entirely secondary consideration. But this question of drugs, at least for a host of remedies, is definitely settled for us by the Massachusetts General Hospital report above alluded to. The pancreatic preparations proved unsatisfactory. No drug save opium commended itself, and that in no wise as a specific. It certainly is another step forward, this relegation of the Pharmacopœia in diabetes to a very minor position.

Rollo recommended an animal diet as long ago as 1797, and it would seem as if little improvement in treatment had taken place, or could be hoped for in the future along this line. But the point made by Rollo was the prominence given to an animal diet over carbohydrates. Little was said of the great class of fats, and what was said was quite as much against as for their use. For example, at the Massachusetts General Hospital, as late as the period 1840-55, occurs a record of the following diet:—"Lean meat, with a small quantity of stale, dry, or toasted bread, avoiding all fatty, farinaceous, and saccharine articles." That fifty years ago there was a lack of appreciation of the importance of fats can be understood, but that this idea has been allowed to persist seems incredible. That it does persist can be seen by consulting the most recent books of medicine, or by looking over the proceedings of the meetings of the British Medical Association recently convened in Montreal.

The importance of fats is seen first from the fact that they are the form of food best assimilated by

the diabetic patient. Nearly all the sugar and starch given leaves the body unused, and for every 100 grammes of albumin there is the possibility of 45 grammes of sugar appearing in the urine, whereas from fat little, if any, sugar is formed. But this is not all, and herein lies another great advantage in fats. Each gramme of fat is capable of furnishing nine calories on being oxidised by the natural process of the body metabolism, whereas a gramme of proteid matter yields but four calories, even allowing that none of it is converted into sugar. These are facts, and on these facts a rational treatment of diabetes must rest.

In the light of all this, the books still continue to recommend or countenance skim-milk and butter-milk either as a prominent constituent of the diet or as worthy of trial. The difference between the amount of sugar in ordinary milk and these bi-products is unessential, but the difference in fat is so great that it amounts to more than one half of the total quantity of nourishment therein contained. In every litre of good milk the diabetic patient receives some 300 calories more nourishment (and that of the quality best suited to his needs) than he does in skim-milk.

Following the remarks on skim-milk in the text-books comes the section on diabetic breads—that pitfall over which few pass in safety. Over and over again these have been exposed in Boston many years ago, and again this year. Need we wait until the millennium before it is recognised that a small known quantity of ordinary bread is better than an unlimited amount of bread nearly as rich, or even richer, in carbohydrates?

The commonly accepted mode of treatment of diabetics has been, and still is, of a negative character. This is a much more serious charge against it than to say that skim-milk and diabetic breads are used. Article after article is cut off from the patient's diet, and even what is left him he is only "allowed," until finally he comes to think he eats at all only by sufferance. The secret of the successful dietetic treatment of diabetes lies entirely in the opposite direction. The diet is positive. It is not a question of how little sugar, but rather how much fat. It is not so much the withholding of articles of food as it is the prescribing of those best adapted to the patient's condition. Von Noorden gives, to form a basis

of the diet, what he calls his "Eisernen Bestand," which consists of 60 grammes butter, 2 eggs, 10 grammes olive oil, 30 grammes fat cheese, 1 litre milk, and 30 grammes alcohol. This quantity of food can be taken daily, is sure of assimilation, and furnishes the patient some 1600 calories, or nearly two thirds of all that he needs. For the remaining amount there can be considerable latitude, admitting of variety.

That one may rightly prescribe and the other follow the directions given, both doctor and patient must know the amount of food taken, and have a working knowledge of its three constituents. This can only be obtained on the doctor's part by the study of first principles. This does not mean theoretical but practical study. He must know how much butter can be taken at a meal, how much cream can be mixed with the coffee, how much oil can be used in salads, &c. Only by personally weighing and measuring the kind of food his patient is to eat can he rightly advise him. Then, if it is found that the sugar continues to be excreted, the cause can be located, and a more suitable form of carbohydrates given, or the total amount can be further restricted. Really an interested intelligent cook is of more value to most diabetics than a trained nurse. A Denver doctor recently said of consumptives that if they wished to get cured of phthisis they must make a business of it. In diabetes this applies to doctor and patient alike. Between the two a partnership must be formed, and with a more cheerful way of regarding the disease at the outset, a better appreciation of the value of fats, and a positive diet list, the results will show that there is even in diabetes an improvement in our treatment.—*Boston Medical and Surgical Journal*; *Therapeutic Gazette*, February, 1899.

An Ointment for Chapped Hands.—The 'Journal des Practiciens' for December 31st credits the following formula to Comby.

R	Menthol	1 part,
	Salol...	2 parts;
	Olive oil	10 "
	Lanolin	30 "

M.

N. Y. Med. Journ., Feb. 18th, 1899.

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* Specially reported for *The Clinical Journal*. Revised by the Author.

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WITH MR. STANLEY BOYD IN THE WARDS OF CHARING CROSS HOSPITAL,

November 3rd, 1898.

Intestinal Obstruction—Strangulated Inguinal Hernia found.

GENTLEMEN,—I have nothing to show you in this patient, as I hope everything from which she was suffering has been relieved. Her age is 47, and the history given was that for the past two years she had had a hernia, which she described as descending into the right labium, and which we therefore assumed was inguinal. She had never worn a truss. The hernia was generally down when she was up, and *vice versa*. On Sunday, October 30th, the patient was quite well. On Monday morning her bowels acted at nine o'clock for the last time; after that she passed neither motion nor flatus. Very shortly after her bowels had acted she felt sick, and she accordingly obtained a Seidlitz powder and took it. She vomited the Seidlitz powder and felt sick all through the day, and there was a good deal of rumbling and pain about the umbilicus. She went on like that until the evening, when she took two pills; these she vomited, and thus started sickness, which continued, while the pain increased until it became very considerable, and always in the umbilical region. On Monday night the vomit became very offensive to smell and taste. She did not consult anybody on the Tuesday, but on Wednesday she sent for Dr. Fletcher, of Streatham, who, recognising at once that there was some intestinal obstruction, had her sent to the hospital. The patient was then in very good condition, her pulse was about 100, rather small; her temperature was subnormal. There was absolute constipation. She said that she vomited very copiously four or five times a day, and a great deal more came up than she swallowed. The material she vomited was distinctly *fæculent* and stinking.

After she had been in the hospital two or three hours she vomited fifteen ounces all at once, and that enabled us to judge of this matter for ourselves.

The abdomen was a little bit full, and was quite soft everywhere. On examining the hernial region on the right side, I thought I could feel something above Poupart's ligament a little different from the condition on the other side, but nothing definite. I thought it might be a piece of omentum in the iliac fossa. The fat in this region was very thick. Intestinal obstruction with a history of hernia always leads one to suppose that there has possibly been some band or some trouble about the hernia which has led to the intestinal obstruction. I did not think that this was a case of simple strangulated hernia because there was no visible or clearly palpable swelling in the hernial region, and partly because the patient stated that on the Monday, the first day of her illness, the hernia was down all day, gave no trouble, and went up when she went to bed, just as usual. It was noteworthy however, that from the Monday night she had not seen it, although she had been retching and vomiting a good deal. That drew one's attention to hernia somewhat pointedly, and I thought I should find something in the neighbourhood of the internal ring to account for the obstruction, and that the early *fæculent* vomiting pointed to the involvement of a portion of intestine high up. If I found nothing in the hernial region, I thought that in all probability I should find something high up in the abdomen, not of a very acute kind, possibly a retro-peritoneal hernia between the duodenum and the jejunum.

I could not at the moment operate, so arranged to watch her a little, and have an enema given; it brought away only a few particles of *fæces*. After she had been in three hours she vomited, and the material was *fæculent*. I saw her again late at night; she was still in a very good condition, and there was obviously no need for great hurry, and therefore rather than wake up the hospital in the middle of the night, I kept her till this morning, when she was still in much the same condition—her pulse being 106, of moderate quality. We took care to warm the operating theatre well, and on the table on which she was placed a hot-water cushion was laid. I explored through a median supra-pubic incision, and the

first thing I came upon was a coil of moderately distended purple small intestine. My next step was to pass my hand down to the right inguinal region, and there I found a thick band running to the internal abdominal ring. I put my hand in, and drew it out as carefully as possible, so as to avoid extravasation into the peritoneum, should it prove to be strangulated bowel and give way during its passage through the abdomen. I found it was a piece of small intestine, of which only a portion of the lumen had been nipped in a small, soft, but sharp-edged internal ring. The bowel showed very distinct signs of pressure at both ends where it had been nipped, and its wall was considerably thickened; but the peritoneal membrane had not lost its shiny appearance in any way, and the colour of the bowel was good, so I put it back again into the abdomen without much hesitation. That is the case, and it is a very interesting one. As I operated only this morning I cannot say much about the progress except that she has had a stool since. She has recovered well from the anæsthetic, and her state at present is quite satisfactory.

Note.—This patient made an uninterrupted recovery, and a radical cure of her inguinal hernia was done nine days after the first operation.

Appendicitis.

This young woman is a dressmaker by occupation, and her age is twenty-two. The case is interesting, particularly with regard to a mistake which was made in the diagnosis. At 6 p.m. on October 14th, having previously been quite healthy, and never having had an attack of the same kind before, she was suddenly seized with severe pain in the right iliac region. This history now-a-days always fixes our minds upon one particular little organ. She went to bed, passed a bad night,—partly on account of the pain and partly from constant retching,—and the pain increased and became very severe. After the first attack, however, the vomiting was only occasional throughout the illness. I may say at once that this was a case of appendicitis, and that the history given is quite a common one in such cases—more or less severe and sudden pain, the stomach emptying itself at the onset, and remaining irritable for a few hours; then, as a rule, the vomiting dies away. Her bowels had been confined up to the

onset, which, again, is common enough before appendicitis; but they acted with the onset of the pain, and some purgative medicine which was given subsequently acted. Her period was overdue one week at the time of the onset of pain; it came on soon after the onset of the pain, but it lasted only two days. Then came the point which led the doctor astray. After this period was supposed to be over he examined her urine, and found that she had hæmaturia. Now the question is, was the period over? So far as I can gather, it was not a catheter specimen which was examined. The probability is that the period was not over, and that he got urine mixed with menstrual fluid, and that was the explanation of the hæmaturia he discovered. However, finding hæmaturia set him thinking about the kidney. On the second day of the illness a tender, rapidly increasing swelling was found in the ilio-lumbar region; and although the tumour was rather lower than a renal tumour usually is, its presence, together with constant moderate fever, led him to the diagnosis of pyonephrosis. There was nothing in the previous history, nor in the history of the present illness, to point to renal trouble, though, of course, that would not of itself eliminate its possibility. But the history was that of an attack of acute appendicitis, and probably you formed that opinion when you heard me narrating the history.

When admitted this patient was in good condition, though she had been able to take but little food, as all attempts to do so caused retching. When we first saw her she was lying on her back in obvious pain, and when she moved about she kept her abdomen stiff and rigid. Her face was flushed, and she had a temperature of 103.4° , her pulse being 140, small and regular. Her urine contained a little albumen, but its specific gravity was 1028. Her abdomen showed distinct fulness in the right ilio-lumbar region; there was some rigidity here, and it was noticed that this area did not move as well as the corresponding area. We could feel here a tender swelling with a rather irregular and angular edge; its surface was not smooth and rounded like that of a kidney swelling, but was distinctly irregular to the touch, and, except to a slight extent by its position, it did not convey the idea of a kidney at all. It was a large swelling, extending from internal

back into the loin like a kidney; it could not be moved to and fro like a kidney, between hands in front of and behind it. It extended up to within half an inch of the costal margin, and gave one the idea that it ended there, not that it ran up under the liver some distance further than one could feel. Downwards it almost reached the outer half of Poupart's ligament, and ended in a rounded point. *Per rectum* I could feel some hard masses, probably scybala, in a loop of the sigmoid flexure hanging down in Douglas's pouch. The swelling was entirely abdominal. The diagnosis really was quite a simple one. Neither history nor physical examination pointed to kidney; both pointed to appendicitis, with the appendix behind and outside the cæcum. Pus was almost certainly present. I thereupon proceeded to operate. I made an incision well outside the linea semilunaris, and opened into perfectly healthy peritoneum, which was not adherent at all. There was a mass behind the cæcum, and the line of adhesion of the anterior parietal to the visceral peritoneum was just three quarters of an inch outside my incision. It would have been an extremely dangerous thing to lift up the outer edge of the cæcum and let the pus out through the peritoneum. Therefore, having noticed where the peritoneum was reflected, I closed that wound, sewed it up closely in several layers to avoid an abdominal hernia, and guarded it carefully. I then made another incision parallel with the fibres of the external oblique, above the anterior half of the iliac crest and well outside the line of adhesion. I first explored between the inflammatory mass and the iliac fascia, and reached a depth of two or two and a half inches without finding pus. It was then necessary to explore the inflammatory mass from behind, a somewhat delicate proceeding; for, having passed through a layer of matted fat, I came to a membrane, and the question was whether this membrane was the back of the bowel or peritoneum. I wanted to open the abscess, and not the bowel; but I could discover no clue to the nature of the membrane, and therefore made a small puncture, which I could easily sew up again afterwards if necessary. Fortunately it struck the pus—probably about two ounces, and very foetid. I enlarged the opening and examined for the appendix, but could not feel it anywhere. In such cases it is not wise to search far for it by enlarging the wound and breaking

down adhesions; it is best simply to drain the cavity, and the majority of cases, I believe, get well without much difficulty, though a sinus not uncommonly persists for a few weeks or months; it does not prevent the patient from getting about. I put a drainage-tube in here and tried to run the pus from it into a bottle, but failed; so we have had to take the discharge from the wound into the dressing. The difficulty then was to keep the first wound aseptic, but we succeeded perfectly. The operation was done a fortnight ago, and you see that the first wound has healed up by first intention throughout. We put on a scab of collodion three layers thick, with iodoform thickly dusted between the layers. As is usual in these cases, the discharge is now slight, and it has lost its fœtor. A small hard mass of fœces—a fœcal "calculus"—has escaped from the wound, and the patient will shortly be able to leave hospital with a small tube in the sinus.

Ankylosis of Hip at 85°.

This young woman is aged 19. Her history was that at the age of six she fell out of a cart and damaged her hip. She became at once lame, but no treatment was obtained for her. The lameness increased gradually until three years ago; then it began to increase rapidly, and at the same time a dull aching pain developed about the region of the hip. For some time she earned her living as a sewing maid, and got on fairly well so long as she continued that occupation. But when required to assist in housework she found that she could not do it, and therefore she came up to town with the idea of undergoing an operation for the rectification of the trouble. Going up and down stairs was a source of great trouble to her, which you will readily understand when I tell you that her hip was ankylosed at an angle of 85°, that is to say, her thigh formed almost a right angle with the long axis of the body. That had to be compensated for by enormous lordosis; you can see at once that she worked at a very great disadvantage. As far as we could tell the ankylosis was bony, and it is rather difficult to believe her story of recent rapid increase of deformity. On examination we found that the trochanter was above Nélaton's line, and above the trochanter in the line of the femur we could feel a rounded mass under the atrophied muscle, which was the head of the

bone. There was adduction to the extent of about 15°, but no inversion or eversion. The patient had evidently suffered from a slow tuberculous affection of the hip-joint, and the upper margin of the acetabulum—that against which the femur pressed—slowly disappeared under the combined inflammation and pressure, allowing the head to wander out of the acetabulum on the dorsum ilii, where it became ankylosed as the disease subsided. Ordinary dorsal dislocation had not occurred, or there would have been more inversion. Now the question was what was to be done for her? We had a choice between doing a section of the neck of the femur and a subtrochanteric osteotomy. Now an osteotomy through the neck of the femur seems to have this great disadvantage in a case such as this, that the cut surfaces must tend strongly to separate when you straighten the limb. As you let the limb fall the femur rises on to the hip-bone. Consequently I have always done, in cases of this kind, a subtrochanteric osteotomy, and one not at right angles to the shaft. If I were to cut the femur straight across and bring the limb down, the result would be that the lower fragment would be at once dislocated forwards on the upper, so I try to cut somewhat obliquely upwards and backwards across the shaft, and thus to leave a projecting upper fragment, which may prevent this displacement forwards of the lower fragment. On October 22nd I made an incision here from the great trochanter downwards two inches, and cut down on to the femur, elevated the periosteum from it, and chiselled it completely through, so that no violence was required to break it. I think it is important to chisel the whole way through in this way. Well, this femur hardly fell at all. Everything about the joint had undergone adaptive shortening, and, in endeavouring to press the femur down, the dislocation I wished to avoid did occur. It was easy, however, to bring the surfaces again into apposition. Then arose the question how further to improve the position of this limb. I first divided the adductors subcutaneously; I passed a tenotome inside and superficial to the adductor longus tendon, which stood out prominently, and divided it and the brevis and many fascial bands behind it. Still there was great resistance to the falling of the thigh bone, but it was now transferred chiefly to the structures below the anterior spine. Everything there was very

tight, so I subcutaneously divided the tense facial bands and the superficial muscles running down in that region. But that was not sufficient, so I made an incision one and a half to two inches long below the anterior superior spine, and exposed and divided the rectus femoris; then the limb began to fall. You will see that this was a pretty extensive operation. First of all I chiselled the femur through below the trochanter; next I divided the adductors longus and brevis, and neighbouring fascia; then the sartorius, tensor and rectus femoris, and many bands of dense connective tissue, yet you see that there has been practically no rise of temperature, and that all wounds are now soundly healed.

Even after this free division of bone and of soft parts the thigh remained a good deal flexed, and the spine still showed marked lordosis. Even now the spine and thigh are far from being in the same plane; yet much improvement has been effected, and a skiagram shows that the lower fragment remains hitched against the upper, just as I had designed. This improvement has been gained by keeping the patient on her back, maintaining constant weight extension from the limb, and giving some support to the limb by means of a pillow beneath the thigh, the pillow being gradually withdrawn. No support was placed beneath the lordosed lumbar spine. Under these circumstances the thigh has gradually fallen, the lumbar spine has become flatter. At first this caused a good deal of backache, but that ceased in a few days. To bring about still further straightening of the lumbar spine I have now had the pelvis raised on a water-pillow, so that her lumbar spine forms the top of a long arch, which rests only its pillars—the dorsal spine and the sacrum—on the bed. Under these conditions the summit of the arch should sink rapidly.

Tubercular Disease of Ankle and Astragalus.

This little child is aged three, and is said to have had a swelling of the right ankle since September 24th. It got very much worse during the week before admission (October 15th). She was always a delicate child, suffered from bronchitis, had sore eyes for several months before admission, and was obviously tubercular and delicate generally. We found her anæmic, but fairly nourished. Her ankle

was uniformly and very considerably swollen in the way that is characteristic of tuberculous disease affecting this joint. All hollows are obliterated. There is a swelling behind each malleolus, more or less effacing the bony prominence, and these two swellings are joined together by less marked fulness beneath the tendons in front of the joint and the tendo Achillis behind, so that the whole forms practically an annular mass. This child showed that swelling very characteristically. On examining the movements of the foot we found that extension and flexion could still be performed through 30°, which, though considerable, was not nearly enough for the ankle-joint. Inversion and eversion, which take place in the astragalo-calcaneal joint, were free. We consequently came to the conclusion that this patient had tubercular disease of the ankle-joint. By way of treatment we first tried the effect of feeding the child up and of fixing the joint by compression with cotton wool. I kept this treatment up for a fortnight—namely, from October 14th to 30th, but was then forced to the conclusion that it had failed. The ankle-joint was not more swollen than it was before, but a swelling had appeared on the dorsum of the foot, over which the skin was red and tense. It was evident that if I spent more time in endeavouring to obtain a cure by rest a sinus would soon be added to the condition of things already existing. So I made up my mind that something operative must be done. I had her in the theatre yesterday, and began an operation below the inner ankle, exploring and extending my incision as I went along rather than carrying out a previously decided line. I first reached a lot of œdematous connective tissue, and then I got into the swollen synovial membrane. I passed a spoon into the joint, and found that the synovial membrane was tubercular, thus making certain that the diagnosis was correct. The disease might have been purely synovial, but it is impossible to expose that membrane thoroughly, and still more impossible to get at the bones unless the joint is opened freely. There is, in my opinion, no way of exposing the ankle-joint so satisfactorily as that introduced twenty-five or thirty years ago by Professor Hüter, of Bonn. He was one of the first surgeons really to appreciate what antiseptics could do, and in his 'Grundriss der Chirurgie' he advocated many daring operative procedures which have since been widely adopted.

Among others he suggested this method of excising the ankle-joint through an incision from malleolus to malleolus through all the structures in front of the articulation. In one sense it is the crudest possible procedure. I have modified it to a slight extent in all the cases I have done by raising a flap of skin from the dorsum up to the level of the ankle-joint; there I have divided the tendons and the nerves between two sutures inserted in each, and the vessels after double tying. If the ligaments of the ankle-joint are now divided and the foot simply extended, you obtain a full view of the ankle-joint. That was what I did in this case. As soon as the joint was thus exposed I discovered that there was a bony focus; in this patient the disease was primarily osteal. In the rapidly growing tissue of the astragalus some bacilli found a resting-place, giving rise to tubercular osteitis, which had slowly increased. So long as the tubercular inflammation was confined to the nucleus in the centre of the astragalus, there would be nothing except slight lameness, which probably existed for some time before the mother noticed it. Finally the tubercular disease reached the surface of the bone on the upper surface of the neck of the astragalus, and infected the whole synovial membrane. You will see in this specimen the hole which I gouged in the neck of the astragalus, thinking I might save the bone. On cutting through the cartilage shell at the inflamed spot, I saw an opaque yellow area in the bony nucleus about the size of a split pea. I applied a gouge here and gouged away until I found that I was going through the astragalus, and it was obvious that the whole nucleus of ossification was diseased. It then seemed to me that the child's chance of cure would be far greater if I excised the astragalus, which would not ossify after removal of its nucleus, whilst the foot left after excision of it is a good one. I therefore passed a knife through the interosseous astragalo-calcaneal, and other bands, and removed the whole bone, taking every precaution against infection of fresh surfaces from the diseased focus in the astragalus. Finally I removed from the posterior and other aspects of the joint the synovial membrane, which was everywhere tubercular. Where the disease of the synovial membrane had been most marked I applied pure carbolic acid. I then sutured the divided ligaments and tendons,

dressed the wound with cyanide gauze, wrapped the part in a large quantity of cotton wool, without any splint, bandaged it firmly, and slung it so as to prevent oozing. The child has been fretful and has some fever, which is probably due to oozing into the wound under pressure. He is taking his food pretty well, and I think there is no reason to be dissatisfied with the result so far.

Note.—The temperature fell. The general condition of the child improved daily. On the eighth day the wound was all but healed, was dressed similarly, and put up in a light plaster splint.

Mal-union of Fractured Humerus.

The history of this boy is that he fell off a scaffold at Chatham two months ago, breaking his humerus high up and damaging his elbow-joint. He was treated by the application of an internal angular splint with, apparently, an external straight splint (we have only the description of the boy to guide us). Now, I have no doubt that the fracture was a difficult one to treat, but the result cannot be regarded as satisfactory. I have a skiagram of the case, which will enable you to see some points about it, but you learn more by careful feeling. There has been a very oblique fracture down and in through the upper fourth of the humerus. You can both see and feel the upper end of the lower fragment displaced upwards and outwards, and sticking through the deltoid beneath the skin. There is an inch of shortening. Union is firm. The movements of the shoulder-joint are perfect. The question is, can we improve the condition at all? The boy complains of pain, and it is quite possible that the spine of bone through his deltoid may give rise to it. It would be easy enough to chisel that off, or to chisel through the union and to endeavour to get them into a better position. But should we succeed? For two months the tissues and muscles have been contracting, and should we now by traction be able to get the limb to the length of its fellow? I very much doubt if we could stretch the limb out to anything like an inch; and unless we could put a screw or peg in through the two fragments at once, we should probably lose all or most of what we had gained in length by traction, &c., under the anæsthetic. Moreover, the loss of an inch in the length of the humerus is not a very important matter. It may, perhaps, be worth while chiselling off the point of

bone which is sticking through the deltoid. [This was done through an incision along anterior edge of deltoid, the muscle being raised from the projecting bone. This plan was chosen to avoid injury to the circumflex nerve, the position of which could not be ascertained.]

Passing now to the lower end of the humerus, the movements of the elbow are limited; it cannot be fully flexed or extended. In the region of the external epicondyle there is a marked thickening, but no evident displacement. If you

improve. Children often recover in a marvellous way from injuries to joints which would permanently cripple an adult.

Popliteal Aneurysm.

The next case is of a kind we do not often see now-a-days, although in my student days I used to see a fair number. It is a case of popliteal aneurysm in a man aged thirty-five, that is to say in the prime of life, and it is in the prime of life that this disease occurs. He was a soldier and then a



Fracture of right humerus and mal-union, upper end of lower fragment projecting beneath skin over deltoid.

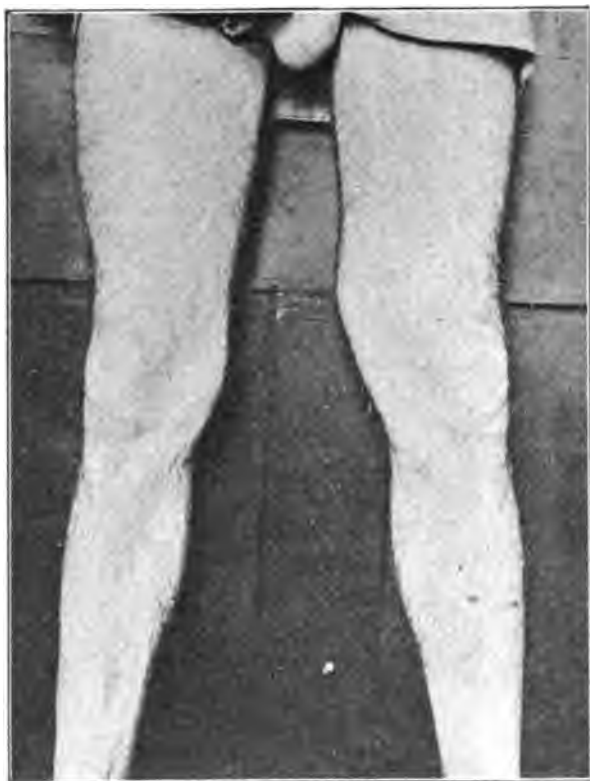
put your finger into the groove between the olecranon and inner epicondyle, you will find it is much deeper on the left side than on the right. No doubt there was a fracture of the outer half of the lower end of the humerus; the callus thrown out is still present in quantity. As it is absorbed, movement will improve. There is no need to do anything here. You know that a boy does without thinking many things which an adult thinks a good deal about. This boy will probably soon wear the joint surfaces into form, and movement will

improve. Children often recover in a marvellous way from injuries to joints which would permanently cripple an adult. This patient had gonorrhoea in 1886, but he has never had syphilis. Since 1875 he has been a total abstainer, so that that excludes some of the recognised causes of aneurysm.

A year ago he noticed some lameness and sense of deadness of the lower limb, but that passed off. Then the symptoms reappeared ten weeks ago, and he noticed a swelling pulsating in his popliteal

space, and the sense of deadness recurred. He rested for two weeks, and the swelling diminished. He then went back to work for two weeks, and the swelling markedly increased, so that it reached a great size. He was told by a doctor that he must lie up at once, and when he did so the swelling went down. He was a healthy-looking man, presented no signs of syphilis, and had a swelling in the lower third of his thigh which extended from the popliteal crease to the opening of the adductor magnus. It was four inches long by

we may thus be deceived into thinking that expansile pulsation exists. We found when we compressed the femoral that the pulsation stopped, but the swelling diminished in size, which no solid or fluid swelling in contact with the wall of the artery, and therefore having only a transmitted pulsation, would do. This diminution in size would be a very good test if we could always apply it, but we cannot; when we press upon a swelling we may indent it or send it deeper into the tissues, and get the impression that it diminishes



six inches across, and bulged more on the inner than on the outer side. It increased the girth of the thigh by three inches. It pulsated expansively in the most typical way; that is to say, the pulsation separated two fingers laid on opposite points. There is only one other condition which may be confounded with aneurysm when there is that definite expansile pulsation, and that is pulsating sarcoma. When a swelling only comes to the surface at one spot and we put two fingers on it, separation of the fingers may result from the driving of the body up between the fingers, and

in size. There was no venous engorgement or œdema of the limb, and no evidence, other than a sensation of numbness, of pressure on nerves. The foot was warm, and the circulation in the foot brisk, but no pulse could be felt in either tibial artery. It would have been a very big business to dissect out this aneurysm, it was so large. Although there is much to be said for this method of treatment, I did not think that there was any special indication for it—*e.g.* need for relief of pressure on other vessels—in this case, in which it could be done. I thought I should get a satisfactory

result from the relatively slight operation of ligature. You may ask why I did not try compression. I did not because it is more distressing to the patient; it is not safer, and is not so likely to be successful as ligature. It is, I think, better to tie than to compress. In this instance I tied the femoral in Hunter's canal, rather low down, so that the operation somewhat resembled Anel's (immediately above the sac), in which the obstruction of the ligature adds little to the already existing obstruction of the sac. The patient was as well directly after the operation as before. You see that the wound has now (eighth day, first dressing) healed perfectly; the foot has been pale and cool, and its circulation slow since the operation; sensation has been perfect throughout. The aneurysmal swelling has distinctly diminished, and there has been no recurrent pulsation.

Methods of Preserving Needles, with Recommendations of a New One.—At a recent meeting of the New York Surgical Society, Dr. Dawbarn ('Annals of Surgery,' February) recommended the use of a saturated solution of washing soda in water, for the purpose of preserving surgical needles. He stated that in his opinion this method of keeping the needles and cutting instruments untarnished was superior to any other which had thus far come within his experience. He showed needles kept bright in this way for a year and more. Most of the other methods are open to some objection. As to keeping them in carbolised oil, this to some extent dulls the edge of the needles. Lysol being a dark solution, the needles are not very easily seen in it; calcium chloride in a closed place to keep the air dry, with the needles lying on a dish near it, produces a curious tarnish, almost like rust; in fullers' earth or other powders it is difficult to find the smaller needles. A solution of borax in water, as recommended by M. Maréchal, will also rust the needles as soon as the thin plating, which all needles have at first, becomes worn away from usage, exposing the steel beneath. Perhaps keeping needles in alcohol is almost as good a way as that under discussion; its only objection being the unpleasant oiliness. Calcium chloride in absolute alcohol—thus keeping it absolute—is effective, but comparatively expensive. The method now advocated—washing soda dissolved in water to saturation—is without expense, and has been proved to be reliable.—*N. Y. Med. Journ.*

A CLINICAL LECTURE ON ACUTE GASTRIC CATARRH IN CHILDREN: ITS DIAGNOSIS AND TREATMENT.

BY

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GENTLEMEN,—We have lately seen in this room some cases the diagnosis of which, uncertain at the outset, has been proved to be that of acute gastric catarrh. These cases have all been those of children or young people, they are by no means uncommon, and often give rise to a good deal of uncertainty, and to no little anxiety. I propose to select one or two cases presenting more or less the same features, and to remark on the points important in diagnosis, and the mode of differentiating cases in which the symptoms are more or less alike. Let me briefly record the history of the first case. The patient, a tall, well-nourished girl aged thirteen, had been ill about ten days when first seen. During this time she had been complaining of headache, loss of appetite, and constipation, sleep had been disturbed, and she had been very thirsty. There was no known cause for such symptoms, and her previous health had always been excellent. The patient was rather flushed, the tongue thickly coated with a greyish brown fur, and rather dry, the breath offensive. The abdomen was distended and rather tender, the pulse rapid, and the temperature raised to 101° . The spleen was not enlarged, and there were no spots. The chief complaint was that of headache and languor. Here we have a group of symptoms pointing strongly to enteric fever.

The next case is that of a boy of five. Perfectly well previously, he had been ailing in an indefinite manner for about a week. He had complained of headache, loss of appetite, drowsiness, and diarrhoea. His face was flushed, his pulse rapid, and his temperature 101° . The abdomen was not tense, but markedly distended; the spleen could not be felt. No spots were observed. There was slight bronchial catarrh. The tongue was very

slightly furred and moist. But the child when first seen did not appear to be very ill; he was fairly lively, and altogether appeared to be less seriously affected than the girl. As you will see directly, however, the subsequent progress of events shows that in the case of the boy the illness was by far the more serious of the two. I have notes of a third case. The patient was a boy, who, until ten days before admission, was in all respects in good health. The day before his illness he had partaken largely of raw fruit. He complained at the commencement of languor, loss of appetite, and headache, exactly as in the previous cases. The temperature was raised—about 102° . There had been no diarrhoea or sickness. The abdomen was flaccid and tender, the spleen not enlarged; no spots were noticed. Here, then, we have three cases—all children—presenting marked symptoms, all of which bear a strong resemblance to each other. For all the onset was more or less gradual, in all there was lassitude and headache, in all loss of appetite. It might very naturally be inferred that all three cases were examples of one and the same malady. This, however, is not the case. The girl—the first case—continued to present the same symptoms for about a week after she was first seen, then gradually she amended. The abdomen became lax, the tongue cleaned, the appetite returned. The second case—that of the boy aged five—who when first seen presented very slight features of disease, became rapidly worse, with high fever, and the case proved to be one of enteric fever. The boy—the third case—continued more or less *in statu quo* for a fortnight, the temperature, indeed, remaining up the whole time, but afterwards falling slowly. At no period in the course of his illness was there abdominal distension, and he remained intelligent and bright throughout. I have no doubt in my own mind that in the first and third cases the symptoms were due to pronounced gastric catarrh, although as compared with the third case the girl was far less ill, and had a far milder attack. You will gather from all this how extremely difficult, indeed impossible, it is to diagnose cases such as these at the first interview. It would be most unwise to commit yourself to a positive opinion one way or the other until the case has been observed some days. In only one of the cases

could direct evidence be obtained of dietetic error. Such error should always be carefully sought for. If ascertained it is of considerable value, as supporting the diagnosis of gastric catarrh. On the other hand, it is perfectly certain that acute gastric catarrh is not in all cases due to error in diet. Possibly insanitary conditions of the dwelling and bad smells may be important factors in its causation. I need scarcely say that such conditions are perhaps the most usual causes of enteric fever. The condition of the tongue is of importance. In the first and third cases the tongue was thickly coated with a heavy greyish-brown fur, whilst in the second case the tongue, though coated, was so to a slight extent only, and was not altogether dry. This is an important distinction. When the tongue is thickly coated and the breath offensive, as in the first and third cases, the condition points to gastric disturbance pure and simple; and though such conditions occur also in enteric fever, still they are less commonly observed than in gastric catarrh. This point is an important one to bear in mind. I attach no importance to the condition of the bowels. Nothing is more usual in enteric fever of childhood than for constipation to exist from start to finish, and in gastric catarrh diarrhoea is by no means infrequent. In none of the cases was enlargement of the spleen apparent. This sign, when present, is a real help, and points strongly to typhoid, but its absence is no indication that the disease is not enteric fever. Further, an enlarged spleen in tuberculosis is of frequent occurrence, so that when present the diagnosis may still be that of tuberculosis rather than of typhoid. It is a physical sign that should always be looked for. As regards spots, they were absent in all these cases, and in children enteric fever frequently occurs with no eruption whatever. The condition of the abdomen is worthy of note. As a rule, in gastric catarrh it is not distended or tense, while in typhoid it is both; but here again there are many exceptions. If, however, on repeated examination the abdomen is observed to be neither distended nor tense, the fact of this condition being present goes against the diagnosis of enteric fever. Much importance attaches to the mental condition. The drowsy, lethargic, indifferent aspect is of much value in the diagnosis of enteric fever or of tuberculosis, and especially

when it persists. Such a state may occur in gastric catarrh, but if so it is not of long continuance. Hence, whenever this mental condition persists or becomes worse the more serious diseases may be confidently diagnosed.

These cases of acute gastric catarrh may sometimes, and in the course of a few days from the onset, become complicated with a simple jaundice. In all probability this is due to the catarrh of the stomach extending to and blocking the common bile duct. Such cases, however, are not of course of common occurrence, but it is well to be acquainted with the possibility of this accident happening. I am decidedly of opinion that many cases of slight febrile attacks in children are due to this condition of stomach. Some years ago it used to be the fashion to speak of attacks of "febricula"—a term apparently intended to avoid the necessity of closer definition—in which slight fever, nausea, headache, and loss of appetite occurred, especially in young people. These are just the symptoms we are now dealing with, and it is very probable that this febricula is really due to attacks of gastric catarrh. I should add a few words on the treatment of these cases, and more especially on that of acute gastric catarrh. In many of such cases it is impossible to be sure of the diagnosis, and a guarded opinion should be given at first. There is no doubt that an excellent treatment for cases seen quite early is the administration of an emetic—a teaspoonful of ipecacuanha wine for instance. In gastric catarrh the emptying of the stomach often relieves all the symptoms at a very early stage. On the other hand, if the case is an early stage of enteric fever or tuberculosis, a simple emetic is not likely to do harm. I may say, however, that after the condition has persisted some days emetics do no sort of good; it is only quite early that they are efficacious. Great caution should be exercised in the administration of aperients. They may do much harm, and no evil results can occur from a few days' constipation, and the unfortunate but universal tendency to administer purgatives in all and every complaint of childhood is often attended with disastrous consequences. When it is clear that the affection is gastric catarrh only, then a dose of castor oil may be desirable, but not before. The diet should be fluid only, milk, broth, beef-tea; and careful attention should be given to the child's diet for some

time after recovery, and all things avoided which are likely to set up fermentative processes in the alimentary canal. The child should also be carefully guarded against cold and wet. The way in which young children in this country, though dressed up smartly externally, are allowed to run about in all weathers practically naked from the arm-pits downwards is most reprehensible; there is no surer way of exciting not only bronchitis and pneumonia, but also catarrh of stomach and intestines. In many cases, especially in those in which frequent attacks of gastric catarrh occur, it is advisable to have a well-fitting flannel bandage adjusted to the abdomen. I ought to add that the form of gastric catarrh which we just now considered, though occurring at all ages, is less common in infants and very young children than in those aged from two or three to fourteen or fifteen. There is nothing new in what I have to-day called your attention to; it is a trite, well-worn subject, yet an accurate knowledge of the history, course, and treatment of such condition you will find far more useful in practice than an acquaintance with the latest views on the bacteriology of the intestinal canal, or of the physiological chemistry of its various secretions.

The Disinfection and the Disinfecting Power of the Skin.—Dr. R. Binaghi ('Centrb. f. Chirurg.', July, 1898), as a result of many experiments, found that after an ordinary cleansing of the hands, followed by simple drying, the number of bacteria was invariably increased (due, doubtless, to the softening of the outer layers of epidermis). After washing in soda solution and repeated rubbing with a dry, sterile towel, the number of bacteria was lessened; also after soaping and scrubbing; also after washing in alcohol or ether. As disinfectants, the following agents gave the best results: Corrosive sublimate, 1 in 1000; carbolic acid, 5 per cent., and potassium permanganate, 1 per cent. A simple long-continued washing with any one of these solutions (without previous preparation) never sterilised the skin. For complete sterilisation the author recommends the following procedure:

1. Wash and scrub in warm soap and water.
2. Wash in a warm alkaline solution (soda or soda carbonate, 5 per cent.).
3. Sterilised water.
4. Rub with sterile towel.
5. Alcohol or ether.
6. Warm sublimate solution, 1 in 1000.

This invariably produced sterility of the hands.

The Post Graduate, February, 1899.

CHAPTERS FROM THE TEACHING OF DR. G. V. POORE.

No. XVII.

GENTLEMEN,—At the close of my last lecture I was showing you common tests for arsenic. Before proceeding with that, I will, in case I forget it, remind you that arsenical poisoning may simulate disease. And I would recall to your recollections the case of Mary Ann Cotton, who was tried at the Durham Assizes in 1873. Cotton was indicted for the murder of her stepson in July, 1872. The body was exhumed, and arsenic was found in it, and was proved to be the sole cause of death. The woman had, at different times, killed by poison her mother, fifteen children, three husbands, and a lodger, making twenty persons in a few years; and the lives of all of them were insured. One of the husbands and four of the children were insured in one office. The death of Cotton's victims was ascribed to "gastric" fever, and wherever this woman moved there seemed to be a mild epidemic of "gastric" fever around her. I may mention another case which is interesting. A number of children at Loughton, in Essex, in 1878, were made ill from the use of violet powder, which was found to contain 38.5 per cent. of arsenious acid. A new-born child was dusted over with this powder twice on the first day, and four times the second day. There was marked redness of the skin, and this led to a very free use of the powder, which, of course, increased the trouble. Notwithstanding the substitution of starch for the powder, the inflammation continued, and the child died on the tenth day from exhaustion caused by sloughing of the skin. Six and a half grains of arsenious acid were recovered from the body of this child *post-mortem* by Tidey. That is a very interesting case.

Now, when arsenic is mixed with organic matter, the separation of the arsenic is not very easily done, and it is not done without a great deal of care. You have seen how volatile arsenious acid is; in a few seconds over a spirit lamp the whole of it volatilised and disappeared. Therefore, the use of strong heat in separating arsenious acid from organic matter is not to be thought of. You

may destroy organic matter without the application of much heat by mixing the organic matter with potassium chlorate, and heating it over a water bath with hydrochloric acid, and in that way your arsenic will not be volatilised. That is not a lecture-room experiment, because the amount of chlorine given off would make the room uninhabitable.

Another way of getting arsenic free from organic matter is by making use of its volatility, and obtaining the volatile arsenical chloride by distilling the organic matter mixed with a good deal of hydrochloric acid. For qualitative analysis, however, one may proceed in another way, and I am now going to show you a test which is of the greatest importance in medical jurisprudence, and whenever you are confronted with an organic fluid, such as beer or gruel, you must always test that fluid by *Reinsch's test*. I have here some common porter. I will put into a test-tube a drop of solution of arsenious acid, and on to this I will pour some of the porter. We will suppose that is a portion of porter which you have to analyse. Take a piece of clean copper foil and cut it into convenient sized strips, place them in a test-tube, and add your organic solution. Then add hydrochloric acid. I want to warn you that the hydrochloric acid in Reinsch's test is not to be added in drops, but in considerable quantity,—a quarter or one fifth will not hurt, one sixth, I think, is about the usual quantity mentioned in the text-books. This you must boil, and if you have very little arsenious acid present, you may have to boil for a long time. In a recent case the mistake was made of not trying the urine with Reinsch's test long enough, and it is evident that in such a case, where only a decimal part of a grain of arsenic was found in the liver, the amount of arsenic in the urine must have been exceedingly small, and the urine ought to have been put aside to boil gently for an hour or more until it was concentrated. Here I have a relatively large amount for the purposes of demonstration, and there will be no difficulty at all. Reinsch's test is peculiarly useful, because it is applicable to organic fluids. When only a small quantity of poison is present, and when it is intimately combined with the organic matter, you do not get the ordinary reactions. There may be mercury, there may be arsenic, there may be antimony in, say, a person's urine, or in the contents of the stomach,

but unless there be a great excess you will not get an ordinary reagent such as sulphuretted hydrogen to give you a precipitate. But with Reinsch's test you get evidence in organic fluids, and therefore it is a test of the greatest possible value, not only for arsenic, but for antimony and mercury and other metals, amongst others bismuth. But we are only concerned here with the poisonous metals. If you try Reinsch's test and get no result from it, you may be perfectly sure that neither arsenic, antimony, nor mercury is present, and, of course, that is a great piece of knowledge. If you try Reinsch's test for a long time, and you find no deposit upon your copper, you may be sure none of these metals are present. I have boiled this porter and arsenic, and now I pour the fluid off and shake the pieces of copper on to cartridge paper. Here is the copper coated with a grey film. There is no doubt about it, it is not merely a dirtiness, but a definite grey film. In sulphurous fluids you may get a dirtiness of the copper which you must not mistake for the real deposit in Reinsch's test. Reinsch's test being applicable to three metals with which we are concerned, and as utility in this class must take the place of order, I will anticipate, and do Reinsch's test with antimony and mercury so that you may see the difference. I will do precisely the same thing with antimony. Here is the porter and I add hydrochloric acid, and indeed repeat the process. I ask you to look at the deposit now; you are sometimes told that there is a difference in the appearance; perhaps there is, but you cannot tell that difference, unless you have the two together to compare them side by side. Without this comparison all you know is that you have one or the other there.

I will now go a step further. Here is some perchloride of mercury, porter, hydrochloric acid, and copper strips. This test is very important, and often helps you to a definite result. If you look closely at the copper you will see something white and shiny, which tells the tale; if I rub the copper with the mercury upon it with a cloth I get a mirror-like polish, but if I rub the one with arsenic or antimony I do not get that result. This mirror-like polish is conclusive as to mercury.

It is, of course, possible to apply further tests to the films which we have upon the slips of copper; but the quantities of material upon the films is so small that practically you will find it best to have

recourse to the original liquid, and, armed with the knowledge which Reinsch's test has given you, to move in a new direction. Whereas the first part of Reinsch's test is very easy, the further testing of the films is not so easy, and is, indeed, rather confusing.

The moral of this is that when you get a positive result with Reinsch's test in toxicological analysis you have every reason to suppose you have either mercury, arsenic, or antimony present, and with mercury the test is conclusive. My advice is that you then immediately proceed with an organic fluid to do Marsh's test. That consists in the generation of arseniuretted hydrogen. I am accustomed to do Marsh's test both with antimony and arsenic, so that you may compare results. I have here two Wolff's bottles, the corks of which I carefully examine. The bottles contain zinc and water. I will add sulphuric acid to both to generate hydrogen.

In using Marsh's apparatus you must be careful not to get an explosion. I hold a test-tube over the exit of the Wolff's bottle and then hold it to the flame, and I get a small explosion, which tells me that I have air mixed with my hydrogen. I wait a little, and repeat the trial; the explosive noise is replaced by a slight sound, showing that probably pure hydrogen is being driven off. I now light the gas at the exit of the Wolff's bottles. Next we have to test whether the zinc and the sulphuric acid are pure. I put the back of a white plate in front of each of the flames of the Wolff's bottle and there is no stain; therefore our reagents are pure. I will now add a very small quantity of the mixture of arsenic and porter to one Wolff's bottle, and wash it down with a little water; and similarly I add a little of the mixture of antimony and porter to the other Wolff's bottle. When we add our suspected fluid there is a very great increase of action. I now apply the plate again to the flame, and you will see that the antimonial flame deposits black spots upon the plate immediately, and the arsenical flame gives us brown spots where it impinges.

I have here three arsenical spots and three antimonial spots in parallel rows upon the plate. The first reagent I will apply is a little ammonium sulphide to each. Notice that the antimonial stain is dissolved and the arsenical stain is not. The next reagent we will use is a solution of bleachi

powder, which I will add to the portion of each on the plate. The greater part of the arsenical stain is immediately dissolved, and the antimonial stain is not. The third reagent is protochloride of tin, and we note that the solubility of the antimonial stain is greater than that of the arsenical stain. The best test is the bleaching-powder solution.

Relapse in Scarlatina.—Richard D. Kennan remarks that one is struck, in looking up the literature of scarlatina, by the fact that the text books of even recent publication contain little or no reference to the occurrence of relapse. Indeed, they are fairly unanimous in the opinion that a relapse, or second attack, of scarlatina is to be ranked among the very exceptional experiences in medical life, and that when it does occur it is invariably after some years have elapsed since the first attack. But second attacks taking place so soon after the first as to be actually called relapses were not seen apparently till recent years. It is not reasonable to suppose that such an occurrence, evident as it now is, could have been overlooked by the many careful observers who had been attracted by the study of scarlatina. Nor is it probable that the character or life history of this fever is very appreciably changed. It seems as if the practice, which has grown up with the modern developments of public health administration, of isolation of dangerous infectious disease in large hospitals, is directly responsible for it. This may require a word of explanation. In private, and in all cases before isolation on a large scale as at present carried out was thought of, each patient was subjected to an ever-diminishing intensity of infection. He was himself daily becoming a lessened source of danger to others, and also to himself. In a hospital ward, however, in which the atmosphere and contents are kept, by the constant introduction of acute cases, charged with infective material, this diminution or attenuation is not obtained. The patient whose attack has left him with but slight protective power may fall a victim to a second attack. Of course it is not to be understood that this is by any means a common occurrence, yet Dr. Caiger, Superintendent of the South Western Fever Hospital, found that a true relapse or early second attack occurs in 5 per cent. of all scarlatina cases admitted to the hospital.—*Dublin Journ. Med. Sci.*

DEMONSTRATION OF CASES AT THE NORTH-WEST LONDON CLINICAL SOCIETY.

Dr. MACEVOY in the Chair.

MR. JACKSON CLARKE said that he had listened with great interest to Dr. Sutherland's account of the case of coxa vara reported in the *CLINICAL JOURNAL* of January 11th. He remembered that some year or two ago Dr. Sutherland had kindly afforded him an opportunity of seeing the patient. At first sight he was inclined to think that the condition was a double congenital dislocation of the hip. On examination, the absence of any



Fig. 1.

descent of the trochanters on making traction upon the limbs and the smoothness of the working of the head of the femur in its socket enabled him to recognise the condition as coxa vara. Since that time Mr. Clarke had had three cases among his out-patients at the City Orthopaedic Hospital. All the patients were young children; two were aged two years, and the other three. In all there was complete eversion of the lower limbs, as shown in the photograph (fig. 1) from one of

the two-year-old patients. The two younger patients, like the one figured by Dr. Sutherland, could easily place their feet at the sides of the face, and assumed that peculiar posture naturally. The displacement of the great trochanter above Nélaton's line was slight in all cases, and this led him to conclude that in young children the posterior part of the neck of the femur gave way before its upper part, the reverse of what had been observed in adolescents.

As to the treatment, Mr. Clarke was averse to operation in young children with active rickets. All his three patients had been cured by using a walking apparatus that kept the feet pointing forwards, and relieved the neck of the femur of much of the weight of the body.

Mr. JACKSON CLARKE then showed a man aged thirty-five, a case of old congenital talipes that had relapsed owing to insufficient surgical treatment in early life. He said he brought the patient forward to show how far they were justified in treating such cases. The patient (a painter) came some months before with the condition shown in the photograph exhibited. He walked on the outer border of the right foot, and his boot was a study, because an additional sole had been placed on the outer side of the foot. It could be seen that there was a marked condition of equino-varus, and they could measure the degree of deformity as much by the wasting of the limb as by any other means. He drew attention to the inside irons, the outside T strap, and the sole-plate in the boot. That was the principle of a Scarpa's shoe, which acted as a simple lever. The patient was operated on first when he was ten years of age, and the deformity dated from some time before that. As the man's age was now thirty-five, the deformity had become fixed by alterations in the bone structure. The question arose, should they attack the bony skeleton of the foot and remove wedges of bone or not? He thought not. All he felt justified in doing was to make the patient comfortable, and enable him to walk on the sole of the foot. He had operated only upon the worst foot; on the other leg there was a good calf, although there was still a certain amount of "pes cavus;" but there was practically no equinus to trouble him, though, of course, the clawing of the toes remained. In the foot upon which he had operated the patient had rested upon the outer border, where he had callosities over the

scaphoid and cuboid bones. He now rested well on the sole of the foot evenly. When the shoe was on, and the irons in position, any remaining tendency to varus was corrected. The patient was much relieved, and that was the limit of the interference he proposed. Tarsectomy would doubtless give a better-looking foot, but it would be weaker, and not of much service to the patient. In many of such cases one might as well do a Symes' amputation as a tarsectomy, as far as the utility of the foot was concerned. In the present case he divided the tibiales tendons, and he found the tendo Achilles a good deal shortened, and the heel was drawn up. That tendo Achilles had been operated on twenty-five years before, and he knew there would be adhesions round the seat of the original operation, and that if he did a simple tenotomy there he would get no result. Therefore he exposed the tendon, and freed it from the adhesions, and having done so he found that the reproduction of tissue had been very complete, and but for the adhesions he would not have been able to say that a previous tenotomy had been done. The scar-tissue that repaired a divided tendon seemed to grow with the individual, and that fact was very important in the surgery of tendons. To elongate it he made a Z-shaped section, and thus made the tendon one and a half inches longer, stitching the ends of the tendon with silk, over that stitching the sheath with catgut, and over that the skin with silkworm-gut. The patient did not have an ache or a pain after the operation, and he already walked in comfort and with less fatigue than before the operation.

Mr. Jackson Clarke showed, for Mr. W. H. Battle, a boy aged 16, an errand boy, who was admitted into the Royal Free Hospital on account of giving way of the left knee. The notes of the case were as follows: About five months ago, as the patient was getting into bed one night, he noticed that his left knee was swollen. It was not in the least painful. The patient thinks the swelling has increased. For the last two months the knee has "given way" at various times when walking, but he has never fallen. The patient applied at the out-patient department a month ago, and was recommended to wear a band round the knee till admission. He has great support from this. The patient

measles, scarlet fever, and whooping-cough. He had an "abscess" in his neck when a child; he has had no swollen glands since. There is no history of rheumatism, and there has been no winter cough. His father died of pneumonia. His mother and one sister are alive and strong. His local condition is as follows: There is considerable apparent swelling of the left knee, the femur projecting forward at the lower end. The tibia appears to be on a posterior plane to the femur, in fact to be displaced backwards. There is no pain on movement, no creaking in the joint. There is some rotation of the tibia and foot outwards, considerable over-extension of the knee upon the thigh, some lateral displacement, together with some lateral movement. There is no tubercle to be felt on the left tibia, the upper end of the bone being very narrow from before backwards. Measurements: There is a one and a half inches shortening of the left leg; the shortening is found to be in the tibia, and not in the femur. The left patella appears to be half an inch below the right. From the external condyle to the inner malleolus of the left foot is fifteen inches. From the external condyle to the inner malleolus of right foot is sixteen and a half inches.

Mr. Clarke observed that it would be noticed that there was considerable apparent hyperextension of the knee-joint, and that the edge of the tibia was nearly up to the angle between the anterior inferior border of the condyles, therefore there was not so much real hyperextension as appeared. Was it a case of genu curvatum? If so, it was due to partial paralysis of extensor muscles of the thigh. Mr. Battle has sent a skiagraph which would throw some light on the matter, but it was a little obscure above the epiphysis. It might be due to premature ossification of the upper epiphyseal cartilage, with want of proper growth anteriorly. At any rate, he thought the deformity was not chiefly in the knee-joint, but at the junction of the epiphysis and the shaft. He assisted one of his colleagues some time ago to operate on a case of a similar deformity, but that was a rickety case, published in the 'British Medical Journal' by Mr. John Poland.

Mr. Jackson Clarke next showed, for Mr. Mayo Collier, a boy who was the subject of obliquity of spine. On making the patient stand in his normal posture a hollow was noticed on one side

of the waist, but not on the other, the trunk deviating to the left. The right side was higher than the left, and that would bring the head out to the left of the body if there had been no compensating curve in the spine. That compensating curve was a total scoliosis involving practically the whole spinal column. He put his left foot upon a thick book, and they would notice that there was still more hollowness on the right side than on the left. They must conclude that the whole deformity was not merely a postural one, but was in part permanent, and was present even when he lay down. There was also increase in the normal dorsal kyphosis, and increase in the normal lumbar lordosis. The spine was weak, and the patient was said to have been weak ever since he had scarlet fever. Attention would have to be directed to his general health, and any nasal obstruction that might be present should be attended to.

Therapeutic Suggestions for Children.—

Louis Fischer devotes an article to the consideration of those diseases which require immediate and sometimes heroic treatment. In the treatment of convulsions a history of an overloaded stomach, with a consequent high temperature and ptomaine poisoning, and the absorption of this poisoning, causing toxæmia, will call for, first, cleansing of the stomach with lavage, using a normal saline solution. Meanwhile a strong mustard foot-bath can be used to relieve the cerebral hyperæmia, and, if necessary, a leech can be applied behind each ear over the mastoid process. After this the colon should be flushed with warm salt water. Hypodermoclysis has been of service to the author in cases of extreme exhaustion following long-continued attacks of diarrhæa, in which the blood is de-hydrated and almost thickened. It is an invariable rule of the writer never to permit a child to retire for the night without a movement of the bowels; consequently, if the infant has been constipated during the day, he advises the injection of one pint of a mixture consisting of two-thirds warm water and one-third glycerin. Another alarming symptom, anuria, can frequently be relieved by a simple procedure—immersion of the child in water, the temperature of which is 105° to 110°, raising and lowering the infant, and continuing the bath for about one minute. It is advisable to notice whether the child passes its water while in the bath. If this does not succeed, dry cups may be applied over the lumbar region for three minutes at a time, and renewed, if necessary, in an hour, but choosing different parts of the region for the application.—*Amer. Journ. of Gynecol., Canadian Practitioner.*

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A CLINICAL LECTURE ON SOME CASES OF RENAL PAIN.

Delivered at St. George's Hospital, February 21st, 1899,

By W. H. BENNETT, F.R.C.S.,

Surgeon to the Hospital; Joint Lecturer on Surgery in the Medical School; Member of the Court of Examiners, Royal College of Surgeons of England; Examiner in Surgery, University of Cambridge, &c.

GENTLEMEN,—Renal pain, or perhaps I ought to say pain in the loin in the region of the kidney, is a symptom which very commonly leads patients to seek advice. Clinically, this so-called renal pain consists of two distinct classes: one in which the pain is consequent upon injury or disease of some kind, for example calculus in the kidney, tuberculous disease, growth, or hydronephrosis. The other class, in some respects the more interesting, of which I propose to speak to-day, is that in which pain, sometimes very acute, exists without any obvious or discoverable disease in the kidney or in the parts immediately around it; in fact, the parts to which the pain is referred appear upon examination to be normal, excepting in one respect, to which I shall presently refer.

Cases of renal pain of this type may be usefully classified as follows:

Renal Pain exclusive of that due to Calculus, Tubercle, Growth, Injury, or Hydronephrosis.

Class I.—With hæmaturia	Tenderness generally present	{ Kidney on exploration may be small but otherwise apparently quite normal.
" II.—Without change in urine		
" III.—With swelling in loin without hæmaturia		
" IV.—With movable kidney.		

It may seem strange at first sight that an organ like the kidney should give rise to so much pain, as it does at times, in the absence of actual disease of the organ. It is still more remarkable that there should exist, in some of these cases, a tendency to attacks of hæmaturia, sometimes profuse, although the kidney itself, as far

ascertained, is, as I have already said, apparently healthy. That these cases are comparatively common there is no doubt. I have, for example, had several of such under my care lately in the hospital, and their occurrence enables me to call your attention to the subject now—I trust with some practical result.

First let me remind you of a woman who was in the Winchester ward, having been admitted under my care on December 24th, 1898. She was forty-six years of age, and had been in the hospital on another occasion in consequence of obscure abdominal pain, with occasional hæmaturia and discomfort in the region of the liver and left loin. When previously in the hospital she was under the care of one of my colleagues, and an abdominal section was made with a view to ascertaining, if possible, the cause of the abdominal discomfort. After a careful examination, the conclusion was arrived at that the woman was the subject of a cirrhotic condition of the liver and in all probability of granular kidneys, because the kidneys seemed smaller than natural, and were a little irregular on their surfaces. Nothing further having been made out, the abdominal wound was united, and she recovered.

No relief from the trouble of which she complained followed. For a year subsequently this constant abdominal discomfort continued, with occasional hæmaturia which latterly had become so much worse that it was very unusual for her urine to be quite free from blood. She was, therefore, induced to apply again to see if anything further could be done for her. On her admission upon this second occasion she complained of very slight general abdominal discomfort, hæmaturia, and marked tenderness in the left loin over the region of the kidney. This tenderness was confined to a spot just below the twelfth rib, which is a very common situation for tenderness in cases of calculus in the kidney. The pain, hæmaturia, and tenderness, were increased by exercise. Here, then, was a case in which most of the classical symptoms associated with stone in the kidney were present. It was therefore determined to explore the left kidney through the loin, in order to ascertain whether there was a stone in it, or whether any other unnatural condition existed which might have caused the symptoms. I did not myself expect to find a stone, but I thought it probable that

the operation would remove the symptoms. I cut down through the loin in the ordinary way, and explored the kidney. It was very small and lobular, almost like a foetal kidney. The lobulation was clearly not the result of disease, but was in my judgment a congenital condition, which I have no doubt, when felt through the peritoneum at the previous operation, gave rise to the impression that the kidney was in a condition of granular disease, an impression which would be strengthened by the small size of the organ. It was clear upon examination that there was no stone present. Bearing in mind the existence of the pain and the hæmaturia I slit up the capsule of the kidney from end to end in its long axis, as I always do in these cases, for the reason I shall mention presently. The operation wound in the loin was closed with fish-gut in the usual way, no drainage being used. Immediate union of course occurred, and a rapid recovery followed. Both the pain and the hæmaturia were completely relieved, and there has been no recurrence of either up to the present time. The proceeding adopted at the operation appears to have been sufficient to relieve the patient of all her trouble. That is a characteristic example of one of these cases of renal pain, associated with hæmaturia and other symptoms, which would be held generally to indicate the presence of calculus in the kidney; yet upon exploring the organ there was obviously no calculus there, the slitting up of the capsule being all that was required to relieve the symptoms.

Cases in the second category in the table are very interesting. The symptoms are the same as those in the case just described, but *without the hæmaturia*. There is continual pain in the loin, shooting round into the back, coming very rarely into the groin, with tenderness under the twelfth rib of the kind already indicated. The tenderness in these cases without hæmaturia is sometimes extremely acute, much more so, as a rule, than in those cases in which there is bleeding—a practical point worthy of special note. The following is an example of this condition:—A girl, twenty-seven years of age, complained of the symptoms I have mentioned—continual pain in the left loin, extreme tenderness under the twelfth rib, which, upon pressure being used, shot through to the front, as pain connected with renal calculus will often do. The pain was in addition very much exaggerated

if whilst pressing the finger upon the tender spot a sudden jerk were given to it, so as to shake the parts somewhat, precisely the sort of proceeding which frequently increases the pain in renal stone. No amount of manipulation, however roughly conducted, would produce blood in the urine, a sign contra-indicative of the existence of stone. The distress which this girl suffered was very great. Although she might be said to have belonged to the class of subject sometimes called neurotic, she was not in any way hysterical; indeed, she was a very sensible girl, who had to earn her own living, and therefore had no time to indulge much in unnecessary consideration of her complaints. Under the circumstances I thought it better to explore the kidney, not because I expected to find anything obviously abnormal, but because I thought the case curable by operation. The kidney, therefore, was examined through the loin, and seemed quite normal, but as it was of full size and rather resistant to the touch I split the capsule, and in order to be sure that no foreign body was present I passed my finger into the pelvis through the renal tissue, searching it carefully. I however failed to find anything abnormal. The wound was sewn up in the ordinary way, and the patient recovered. The operation was performed two and a half years ago, and there has been no recurrence of the symptoms since. Immediately after the operation there was, of course, a certain amount of blood in the urine in consequence of the immediate damage to the kidney structure, but it disappeared in due course, and she got quite well very rapidly.

In connection with the two foregoing cases the following is interesting:—A woman of thirty-three years of age had very much the same symptoms as the patient I have just mentioned, and they led, for the same reasons, to exploration of the kidney. The organ was exposed through the loin, and examined with the finger; there was obviously no stone in it, and the whole condition of affairs was too natural to admit of the possibility of the existence of disease. Nothing further was therefore done; the patient rapidly recovered from the operation, but *she did not lose her pain*, which continued in the same way as it had done for the previous eighteen months. As I felt some apprehension that there might have been still something wrong which had been overlooked, the operation

was performed again. The kidney was explored, and, having in mind the failure of the previous treatment, I split the capsule almost the whole length of the kidney. The wound was sewn up again in the ordinary way without drainage, and it healed up rapidly. It is fifteen months since the second operation, and there has been no further pain, and I do not suppose any will again be felt.

I mention these cases together because it is, I think, clear that the relief of the symptoms was in each due to the free division of the capsule of the kidney.

The next class of case is an extremely curious one. The symptoms are precisely the same as those to which I have referred in the cases without hæmaturia; pain and tenderness in the loin, the pain being always present, and generally exaggerated when the patient stretches the body out or moves the trunk laterally, the sensation felt being suggestive of the existence of some foreign body which requires to be got out of the way. The interesting peculiarity in this class is the presence, in addition to the pain and tenderness (without hæmaturia), of *a swelling in the loin over the kidney*. I have had a case of this kind recently in the hospital in a patient, aged twenty-five, who for years had suffered from constant discomfort in the left loin. From time to time the discomfort increased to attacks of acute pain, so acute, indeed, that complete rest in bed was repeatedly necessary, sometimes for weeks at a time. So severe were the pain and tenderness that a proper digital examination was impossible. Although no history of hæmaturia was forthcoming, one could not help feeling that there might be in this case a stone in the kidney. Upon examination some days after the patient came into the hospital, a distinct swelling was found in the loin in the situation of the kidney, such as to suggest the presence of hydronephrosis, possibly from impacted calculus, or from a kink or twist in the ureter. The idea that the swelling might have been due to one of these causes was, however, purely speculative, as the parts were too sensitive for effective examination without an anæsthetic, which up to this time had not been given. There was also another alternative in this case, viz. sarcoma of the kidney, which is sometimes associated with extreme sensitiveness, but the symptom generally did not in any way point to such a ser-

condition. The peculiar characteristic, then, in this case was the swelling. The symptoms being so distinct, it was decided to explore the kidney, and an anæsthetic was in due course administered for that purpose. When the patient was placed on the operating table, before the administration of the anæsthetic, the swelling in the loin was quite evident, it could be taken between the two hands and moved from side to side, although it certainly did not roll about under one's fingers like a renal swelling as a rule does, but it was very distinct not only to the touch, but also to the eye. Upon attempting to examine the swelling after the patient had been fully anæsthetised, it had entirely disappeared—it could neither be seen nor felt. The swelling was in fact a phantom tumour, and the *coexistence of these phantom tumours with pain apparently of renal origin* is an extremely interesting matter which you will not, to the best of my knowledge, find mentioned elsewhere. The operation was subsequently conducted in the usual way; the kidney having been explored, the capsule was split, as in the other cases which I have described, the wound sewn up, and the patient sent back to bed. The wound healed rapidly, and the relief has been complete. There has been no recurrence of the swelling, nor has the pain returned; the patient is, in fact, now perfectly well.

These cases taken together form a very interesting group. In each case there were distressing symptoms, which might possibly have been taken to point to stone, tubercle, or other conditions, in one even to sarcoma. In a considerable number of cases of renal pain, however, it is certain that the symptoms occur without any appreciable organic lesion at all, being due to some transitory change in the kidney which gives rise to the trouble, and which can be relieved by operation. For the relief of the symptoms which arise from these conditions, about which we know little or nothing, one thing seems essential, viz. the division of the capsule of the kidney. The capsule should be split from end to end along the convex margin of the kidney; very little bleeding occurs in consequence into the peri-renal cellular tissue, no oozing of urine takes place, the wound in the loin may be sewn up in the ordinary way, no drainage-tube is necessary, and the patient gets well rapidly. In the majority of such cases, pro-

vided the patients be not hysterical, such an operation will cure all the symptoms, and the patient subsequently remains well.

It is at first sight curious, in the cases of pain of which I am speaking, that *if hæmaturia is present the tenderness and pain are, as a rule, less during the attacks of hæmaturia when this is intermittent, and when, if the bleeding is continuous, the flow of blood is greatest, than they are in the intervals of attacks of intermittent bleeding, or when in continuous hæmaturia the bleeding is very slight.* This points very clearly to the symptoms being connected with a condition of abnormal tension in the kidney itself. It is unnecessary to remind you that any organ which is contained in an inelastic covering, so that it cannot expand to some extent, will, when it becomes engorged with blood, give rise to pain and irritation; it is equally sure that division of the rigid envelope will relieve the symptoms due to the tension which arises from engorgement of the contained structure. To give examples of this would be superfluous. It is reasonable to conclude that, in the majority of the cases of renal pain of which I am speaking, the symptoms are due to increased tension in the kidney. Sometimes this tension relieves itself for the time being by spontaneous hæmorrhage, hæmaturia being the result; thus the diminution of the symptoms during the bleeding is explained.

The division of the capsule of the kidney, therefore, under these conditions, is merely the application of a treatment which common sense would naturally suggest; whether the finger should subsequently be introduced into the pelvis through the renal tissue must depend upon the nature of the indications. If there is any reason whatever to suspect stone it should be done; as a matter of routine it is unnecessary, and may be harmful. In two of the cases which I have mentioned it should be noted that the kidneys were abnormally small. Whether the small size of the kidney was due to any contraction of the capsule, by which the increased tension was caused, it is of course impossible to say; but it would be extremely interesting to ascertain, if it were possible to do so, the effect of the division of the capsule upon the size of the kidney later on.

In patients who are of the true hysterical type you must not expect to produce with any sure-

ness the satisfactory results I have indicated. In such subjects one of the commonest apparent causes of renal pain is movable kidney, and there are several interesting points connected with the pain which is associated with that condition which differs somewhat from that occurring in the other cases to which I have directed your attention. Movable kidney is not an uncommon condition; it occurs in many people, but only a small minority of these suffer real discomfort. It is a mistake to suppose that a movable kidney need give rise to trouble. At times grave trouble arises; hydronephrosis may occur, for example, the result of a twisted ureter or some other condition giving rise to obstruction. Very freely movable kidneys too, floating kidneys properly so-called, may from the occasional injury which they are subjected to in the course of their erratic migration over the abdominal cavity, be bruised, slight hæmaturia resulting. But those are exceptional cases. There are, as I have said, a considerable number of people whose kidneys are not speaking strictly, fixed, but very few of such people suffer from any inconvenience in consequence. A certain number of people who are the subject of movable kidney do, however, suffer from pain, and this pain is frequently one of the most difficult of symptoms to relieve. A kidney having once been detected as movable, must, if it seems to cause pain or discomfort, be dealt with in some way. In some instances, when the pain is due only to the preternatural mobility, a cure can be effected by stitching the kidney in its normal site, but simple cases of this kind are exceptional. It may be well to remind you here that one of the earliest and commonest symptoms associated with movable kidney is dyspepsia, in consequence, some people think, of the way in which the stomach is dragged upon or is interfered with by the unnatural mobility of the organ. It is, however, very doubtful whether the mobile kidney is itself the cause of the dyspepsia in the majority of cases in which that condition is present, since movable kidney is often only a single factor in a condition of general laxity of the parts inside of the belly. In many cases of floating kidney the stomach hangs low, it may be dilated; the condition called gastric ptosis, together with looseness of the connections of the liver and other parts, may be present, facts which are most important for con-

sideration in relation to treatment, because it is not uncommonly found that after treating these movable kidneys by fixing them by stitches in their normal position, the symptoms of which I have been speaking are not relieved, for the simple reason that they have not been due to the rolling about of the kidney, but have occurred because the stomach, like the kidney, is out of place, and frequently dilated. With regard to the pain referred to the kidney in these cases, I know of nothing more difficult to cure. The patients are nearly always neurotic, and the pain as often as not begins only after the accidental discovery of the movable organ. Occasionally sufficient relief may be obtained from a properly fitting mechanical support; in certain cases fixing the kidney by operation will effect all that is required. In the majority of instances, however, in which patients complain of acute and continual pain, the operation of stitching the kidney to the side does not, so far as I have seen, relieve the symptoms, although the kidney may remain fixed. Therefore, in discussing the benefit which a patient is likely to derive from an operation of this kind two conditions should always be kept in mind; first, *the mobile kidney* and then *the pain* of which the patient complains and usually attributes to the mobility of the organ. By a proper operation the mobile kidney can be effectually fixed in its normal position for periods which differ frequently in different subjects, so that a cure for the preternatural mobility may be promised generally for a long period and sometimes permanently, but a cure for the pain cannot be promised with certainty, and herein lies the weak point of the operation of nephrorraphy, inasmuch as although the organ remains fixed the patient is not necessarily any the better for it. It would be easy to give many illustrations of this fact; for example, there was a girl in the Drummond ward recently who illustrates the point very well. She was originally admitted into Wright's ward on April 16th, 1898, on account of a movable kidney on the right side, with much pain. I stitched the organ to the side in the usual way, and it has since remained perfectly fixed. The fixing of the kidney was complete, but the benefit to the patient by the operation was nothing. Her pain continued exactly the same as before. The girl was of a highly neurotic type, and I originally had very little

of benefit from the radical treatment. She left the hospital about six weeks after the operation. On September 28th, 1898, she applied for admission again, begging for further operation or any other treatment from which benefit was possible. Her symptoms were the same as on her former admission. I felt pretty certain that no further operation would help the patient; but after a consultation it was thought desirable to make an abdominal section, to ascertain whether any previously undiscovered condition could be found to account for the continuance of her symptoms. The abdomen having been opened, a careful examination was made; nothing abnormal could be detected, the right kidney, formerly movable, was firmly fixed in its natural position; there was no adhesion, displaced viscus, or any other condition to which the pain could have been due. The state of the patient was unchanged by the operation, the symptoms continuing as before. Probably nothing will cure her excepting isolation, &c., after the Weir Mitchell method, but this is not practicable in her case.

As a corollary to this case the following is worth relating:—The patient was an educated woman, thirty-six years of age, who had to earn her own living, and therefore could not afford to be laid up more than was absolutely necessary. Her symptoms commenced with an occasional feeling of cramp in the right loin whilst she was exerting herself rather violently. A little later on she began to feel intense pain whenever she stretched herself in a certain direction; and one day, whilst attempting to lift something, she felt a "start" in her side; upon placing her hand on the spot she found a round lump there. Somewhat alarmed at the condition, she laid down, upon which the lump immediately disappeared. From that time until a few months ago she was being constantly troubled by the erratic behaviour of this lump in her abdomen, which, on examination, proved to be a typical movable kidney. Belts and every other contrivance having failed to give her relief, she was submitted to operation, because the kidney was not only movable but was also apparently causing acute and constant pain, almost from morning to night. She was only without pain when lying on her back. I explored the kidney through the loin, and found it movable, but healthy. It was

fixed by four kangaroo tendons, and she rapidly got well of the operation, but she did not lose her pain. Now, this patient, although apparently not hysterical, was of the type that I am in the habit of calling the quiet neurotic type. She appeared to be a most self-restrained common-sense woman, but at the back of all this placidity there was a quiet neurotic condition. No benefit, so far as her pain was concerned, was therefore derived from the operation on the kidney; and although that organ remained fixed, the pain continued. Whilst she was away during her convalescence she had an attack of pain near the lower end of the kidney which had been stitched to the side; the medical man in attendance diagnosed the condition as an attack of appendicitis, and told her so. She recovered from that attack, remaining in the country for some time subsequently, and then came back again asking for further relief if such were obtainable. The discomfort due to the mobility of the kidney was of course gone; but there was still continual pain, intensified at a spot just below the kidney, which prevented her undertaking any exercise or active occupation. Had I used silk or any other unabsorbable material for suturing the kidney, the pain might perhaps have been attributed to the irritation caused by its persistence, but the kangaroo tendons must have disappeared long before her return. I examined her most carefully in every way, I could detect no indication of present or past appendicitis. She went to other surgeons and to one or two physicians, but only one of them could find any evidence of appendicitis, but he had no doubt as to the existence of appendix disease. The result was that she, feeling her suspicions of the existence of appendicitis confirmed by this opinion, begged to have the operation for the removal of the appendix performed. Under the circumstances I saw no reason why she should not have the operation, although I could not myself make out that the appendix was diseased in any way. I removed the appendix, which, beyond being small, was perfectly normal microscopically and to the naked eye; its lumen could certainly never have been occluded, and from its condition I think it inconceivable that it could have accounted for the swelling which was felt by one of those whom the patient consulted, and led to the diagnosis of appendicitis. The second operation was for a

time followed by relief, as was the first operation ; but when I last saw the patient there was a threatening of returning pain in the old region, and I strongly suspect that the return to active life will be followed by a recurrence of her trouble in its full degree. The recurrence of pain or other symptoms in cases of movable kidney after the performance of nephrorrhaphy naturally tends to raise the question of the desirability of the removal of the kidney in cases in which other methods of treatment have failed. Provided that the kidney concerned is not diseased, I believe nephrectomy in cases in which nephrorrhaphy has failed is entirely unjustifiable, and should not be countenanced, mainly for the reason that the more serious operation is practically certain to fail as the less severe one did, and the symptoms which continued or recurred in spite of the latter will not be cured by the removal of the kidney, since the pain in such cases is not truly renal, but the result of some nervous condition which the removal of the kidney would in no sense effect. Having regard to the uncertainty of the results which follow upon nephrorrhaphy, it may fairly be asked, "What is the justification for operating in so many of these cases?" The answer to the question is merely this :—It is almost impossible, excepting in a very small number of cases, to tell before operating whether the patient will receive any benefit from the treatment or not. If, for example, twenty successive patients such as those I have spoken of are taken, I do not think that it is possible to predict, with any approach to accuracy, the number which will be cured by operation, using the term "cured" as meaning complete relief from the symptoms, and not merely the successful fixation of the loose kidney. In some of these cases there is no doubt that pain follows the operation of nephrorrhaphy in consequence of the passage of the sutures through the renal tissue. If the sutures used are of an absorbable material, kangaroo tendon, for example, which is sufficiently resistant to absorption to hold the kidney long enough in position for it to become fixed by permanent adhesions, the pain so produced disappears as the suture becomes absorbed ; on the other hand, if a permanent material such as silk is employed, a continual irritation may result, which is prone in some cases to give rise to persistent pain disappearing only upon the removal of the sutures

by a subsequent operation. The whole question of the treatment of the pain which occurs in some cases of movable kidney is, as I have already said, extremely difficult, one of the most important practical points being that connected with the necessity of avoiding the mistake of thinking that the successful fixation of the kidney is followed in all cases by the relief of the symptoms of which the patient has complained. A sufficient realisation by the practitioner of this fact will often save the patient much vexation, and himself not a little disappointment.

Salicylate Delirium.—It is interesting to take notice of a case reported by Rendu at a recent meeting of the Société Médicale des Hôpitaux ; it was that of a woman of thirty who was admitted to the hospital owing to an attack of subacute polyarticular rheumatism. An examination of the urine showed it to be normal, and no visceral lesions existed as complications. The salicylate method of treating rheumatism was at once instituted, as much as $1\frac{1}{2}$ drachms of salicylate of sodium being given each day. At the end of thirty-six hours the joint symptoms had almost disappeared, and the fever was completely dissipated. That night, after a chill, she became violently delirious, had hallucinations of sight and hearing, with extreme agitation, and could only be restrained by force. The temperature rose several degrees ; the urine was abundant, and contained considerable quantities of albumin and indican. The tendency was to regard the rise of temperature and the cerebral manifestations as signs of cerebral rheumatism, and the possibility of uræmia was considered. Finally it was decided, however, that the symptoms were due to salicylic intoxication. Salicylate of sodium was therefore stopped, and sixty grains of bromide of potassium administered ; a hot bath was also given of half an hour's duration. A milk diet was ordered. At the end of two days the patient was much better, and the joint symptoms, which had temporarily become slightly worse, were relieved by the use of antipyrin and sulphur baths.

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THE DIAGNOSTIC IMPORTANCE OF UTERINE HÆMORRHAGE.

BY

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UTERINE hæmorrhage is due to a variety of causes, both general and local. In many cases an examination shows at once what the cause is, but in others the question of treatment may have to be considered before the cause is certainly known. So it is important that all the possibilities should be present in the mind of the medical attendant when brought face to face with a case.

The general causes may be dismissed in a few words on account of their rarity. They are such conditions as hæmophilia, purpura, and scurvy, *i.e.* the blood-dyscrasias, high living combined with sedentary occupations, and residence in hot climates. But whilst a mere enumeration is sufficient for our present purpose, it must be borne in mind that even rare conditions are sometimes met with, and consequently must not be left entirely out of account when thinking of the possibilities of any given case.

In considering the local causes of uterine hæmorrhage, it will be convenient to review them in relation to the age and sexual history of the patient; for these factors come within the cognisance of the medical attendant and will guide him, to a certain extent, in forming a diagnosis.

GROUP I.—The patient is a virgin.—Below the age of twenty the most common cause of hæmorrhage is *uterine congestion*, which in turn may be due to cold or exposure at a menstrual period. The hæmorrhage takes the form of menorrhagia, the flow being more abundant or its duration being prolonged. Between the ages of twenty and thirty bleeding is most often due to a *uterine polypus*; and the fact of its occurring in the intervals of menstruation, as well as at the periods, will generally serve to distinguish a polypus from a simple congestion. A polypus is sometimes met with under the age of twenty, but it is rare. In the next decade, from thirty to forty, *uterine myoma* becomes an important factor. In the early stages a slight increase in the quantity of blood lost during menstruation is noted; later on the increase is more considerable. A distinction

must be drawn between the different kinds of myoma, with regard to their influence on hæmorrhage. Thus a subserous myoma has often no effect at all; an interstitial tumour generally causes menorrhagia, whilst in proportion as the growth is submucous and tends to polypisation, the tendency to hæmorrhage increases, till an almost continuous metrorrhagia may result. It is important to remember that when uterine hæmorrhage is so profuse as to lead to excessive blanching, a polypus, either mucous or fibroid, may be safely diagnosed. In the decade under consideration, *i.e.* between thirty and forty, uterine congestion and mucous polypus also figure as causes of hæmorrhage. Further, just as a polypus may be found in persons under twenty years of age, so a myoma may be found under thirty; but this is relatively seldom.

Above the age of forty menorrhagia is sometimes caused by *senile or hæmorrhagic endometritis*; but if the conditions previously enumerated can be excluded, hæmorrhage after forty must be assumed to be due to *malignant disease*, unless the contrary can be proved. In virgins malignant disease nearly always takes the form of carcinoma or sarcoma of the body of the uterus; whilst epithelioma of the cervix is rare. Hæmorrhage due to malignant disease is intermenstrual as well as menstrual; the blood is generally mixed with watery discharges, and before long acquires a foul smell.

Having considered the symptomatology of hæmorrhage occurring in virgins, we may sum up the results in a table, as follows:

Age.	Menorrhagia.	Metrorrhagia.
Under 20	Uterine congestion.	(Mucous polypus.)
20 to 30	Uterine congestion. (Interstitial myoma.)	Mucous polypus. (Submucous myoma.)
30 to 40	Uterine congestion. Interstitial myoma.	Mucous polypus. Submucous myoma. (Carcinoma or sarcoma of the body of the uterus.)
Over 40	Uterine congestion. Interstitial myoma. Senile endometritis.	Mucous polypus. Submucous myoma. Carcinoma of body. Sarcoma of body. (Epithelioma of cervix.)

The diagnosis must, of course, be reserved until the physical signs have been investigated, and this

will be done either by bimanual examination or by exploration of the uterine cavity, with the addition, if necessary, of histological examination. The consideration of, physical signs may conveniently be deferred until the next group of cases has been discussed.

GROUP II. — The patient is a married woman.—To simplify the matter as far as possible we shall treat of this group of cases in three divisions.

a. The patient has never been pregnant.—The causes of hæmorrhage in a nullipara are those met with in single women, with some additions. The first we must consider is *pyosalpinx*. When a woman who has been married but a short time gives a history of a purulent discharge with scalding micturition, followed by iliac pain and menorrhagia, there is ground for suspecting that she is suffering from gonorrhœal salpingitis. If, on the other hand, there is no history of such infection, and her complaint is of menorrhagia, accompanied by backache and intermenstrual leucorrhœa, the condition is likely to be one of *uterine congestion* brought on by a want of moderation in the sexual functions. Judicious interrogation of the husband may then clear up the diagnosis and indicate the line of treatment. Metrorrhagia coming on in the absence of any signs of pregnancy points to a *polypus*; menorrhagia followed by metrorrhagia in a woman over thirty-five will make one think of *uterine myoma*; whilst hæmorrhage occurring after forty is suspicious of *malignant disease*. In their tendency to the last two conditions, married nulliparæ resemble unmarried women, *i.e.* they are prone to myoma, and to carcinoma of the body of the uterus; whilst they are relatively exempt from cancer of the cervix.

b. The patient is pregnant.—It is well known that a few women menstruate during pregnancy in the absence of any pathological condition. This fact must be borne in mind. As a rule, hæmorrhage coming on after a few months of amenorrhœa points to a *threatened miscarriage*; and when it is rather profuse and accompanied by rhythmic pains, the miscarriage may be regarded as inevitable. The possibility of a *hydatid mole* must also be remembered: the characteristic vesicles in the discharge will lead to a correct diagnosis. From the middle of the term of pregnancy onwards, hæmorrhage may be due to *placenta prævia*, or to

partial detachment of the placenta. Sometimes an unusual form of hæmorrhage is met with. Thus in one instance I saw a patient at the sixth month of pregnancy, suffering from severe hæmorrhages. Ordinary means of arresting it were tried without success, and as the patient's health was seriously affected, labour was induced. It was not a case of *placenta prævia*; nor did the placenta show after expulsion any sign of previous detachment. Again, a medical man in practice in Curaçao told me that on one occasion he was called to a patient bleeding freely. He packed the vagina carefully, but the bleeding continued, and on removing the packing he found the deeper portions of it dry. The bleeding was due to a varicose vein in the vaginal wall, which had burst without apparent cause. Occasionally *epithelioma of the cervix* may complicate pregnancy and cause metrorrhagia. It is not necessary to discuss here the question of postpartum hæmorrhage. We must, however, refer to a form of hæmorrhage associated with *extra-uterine pregnancy*. The typical picture of such a case is as follows: The patient has not previously been pregnant, or she has had a fairly long interval of sterility since her last pregnancy. She misses one or two periods, and the presence of other symptoms leads her and her medical attendant to believe that she is pregnant. When the pregnancy has lasted six to twelve weeks the patient is seized with severe pain, or an attack of syncope. The pain may continue at intervals, and irregular hæmorrhage comes on, which may also become practically continuous. The symptoms suggest miscarriage, and an examination will be needed to clear up the diagnosis.

c. The patient has been recently pregnant.—When menorrhagia comes on shortly after a miscarriage or a confinement at term, it is frequently due to *subinvolution*. The patient also suffers from backache and lassitude, and complains of a more or less profuse leucorrhœa. Subinvolution is more likely to occur when lactation does not follow confinement at term, and when the patient resumes her ordinary life and duties too soon. When menorrhagia is more pronounced, and especially when associated with metrorrhagia, the possibility of a *placental polypus* must be thought of. In a woman who has had several children, metrorrhagia coming on after the age of forty must always lead one to suspect *carcinoma of the cervix*; two con-

ditions simulate this, *senile endometritis* and the irregular and sometimes profuse menstruation that may accompany the *menopause*. The supposition that it is a case of the latter kind must never be considered a reason for not examining the patient.

Hæmorrhage commencing some months or years after the menopause is almost invariably due to malignant disease. Occasionally a simple polypus may be the cause, and I have also known bleeding to occur from vaginal ulceration, the result of a long-retained pessary.

A uterine myoma is very seldom responsible for hæmorrhage commencing again after the menopause, for this kind of tumour starts growing, as a rule, during the menstrual life; and, when once started, the hæmorrhage continues till the myoma is dealt with surgically, except in those rare instances in which the menopause brings about a regression of the growth; and then hæmorrhage is not likely to recommence later on.

The hæmorrhages met with in married women may also be stated in tabular form as follows:

	<i>Menorrhagia.</i>	<i>Metrorrhagia.</i>
Nulliparæ	Uterine congestion. Pyosalpinx. Uterine myoma.	Uterine polypus. Uterine myoma. Carcinoma or sarcoma of body of uterus. (Epithelioma of cervix.)
During pregnancy	Menstruation during pregnancy.	Threatened miscarriage. Accidental hæmorrhage. Placenta prævia. Hydatid mole. Tubal gestation. (Epithelioma of the cervix.)
After pregnancy	Subinvolution. Senile endometritis. Irregular hæmorrhage of the menopause.	Placental polypus. Epithelioma of cervix. Uterine myoma. Carcinoma or sarcoma of the body of the uterus.

Physical signs.—1. If on examination the uterus is found to be only about the normal size, and if the cervix and the appendages are also normal, then, if the patient be young, the condition may be regarded as one of uterine congestion, or of a small mucous polypus; if the patient be old, we shall think of a small polypus, of senile endometritis, or of functional climacteric disturbance. In either case, metrorrhagia points to polypus, menorrhagia to the other conditions enumerated.

2. If the uterus be somewhat above the normal size, and the tubes considerably enlarged, in a case with a history of xanthorrhœa and menorrhagia, the condition is probably one of pyosalpinx. If there is a well-marked swelling on one side only, and the symptoms are those described under extra-uterine pregnancy, this should be the diagnosis.

3. If there is a moderate enlargement of the uterus and the appendages are normal, menorrhagia points to endometritis, subinvolution, or an early myoma; the history will help in the differentiation of these conditions. With the same physical signs metrorrhagia is probably due to polypus, fibroid or placental, or to malignant disease of the body of the uterus. Pain, an offensive discharge, and emaciation are in favour of the last-named condition. To complete the diagnosis, exploration of the interior of the uterus must be resorted to, with the further assistance, if need be, of histological examination of fragments removed.

4. A polypus protruding from the os uteri, or epithelioma of the cervix, will be evident to the touch, and confirmed by inspection by means of the speculum.

5. A considerable enlargement of the uterus is generally due to either pregnancy or uterine myoma, but it must be borne in mind that malignant disease will sometimes cause marked increase in the size of the uterus. The differential diagnosis of pregnancy and myoma is seldom difficult, and when there is any doubt time will generally clear it up. The cases likely to present considerable difficulty are those in which pregnancy is complicated by the presence of a myoma.

The question of treatment cannot be discussed here, as it covers too wide a field. And further, for our present purpose it is not necessary, for the principal difficulty usually consists in deciding on the cause of the hæmorrhage. Once this is made out, the appropriate treatment will readily suggest itself.

In conclusion, the following rule of practice may be laid down: *In every case of uterine hæmorrhage, never fail to make a careful vaginal examination at the first opportunity.* The only permissible exception to this rule is in the case of menorrhagia of recent duration occurring in a young unmarried woman, when the proper procedure is to first try medicinal measures.

CHAPTERS FROM THE TEACHING OF DR. G. V. POORE.

No. XVIII.

GENTLEMEN,—The next poison I go to is *antimony*. The effects of antimony are very similar to those of arsenic. Antimony, like arsenic, is an irritant; it is more depressing than arsenic; its depressant effect upon the heart is very great indeed. Like arsenic, when applied externally it produces a pustular rash, and you will bear in mind that bodies which are able to irritate the skin are very able indeed to irritate the stomach; that goes without saying. Antimony is usually given in the form of tartar emetic—the potassio tartrate. This potassio tartrate of antimony is easily soluble in water in poisonous quantities. Antimony is a very powerful emetic, and the consequence is that, if a large dose of antimony is given, the whole of it is often expelled by vomiting, and death consequently does not result. So that it is one of those remedies, which I have quoted as an example, which may *sometimes* be an exception to the rule, that the danger of a poison is in proportion to the size of the dose. It is said that, like arsenic and like phosphorus, it tends to produce fatty degeneration, and it is an interesting fact that these three bodies—phosphorus, arsenic, and antimony, all combine with hydrogen, forming phosphuretted hydrogen, arseniuretted hydrogen, and antimonuretted hydrogen, and that they all seem capable of producing fatty degeneration. But of these three, phosphorus has the greatest effect in that direction. The *post-mortem* signs of antimonial poisoning are those of gastro-enteritis. The fatal dose is very difficult to estimate; one twelfth grain will act as a sudorific, two grains as an emetic, but the fatal or lethal dose is exceedingly difficult to determine. There is not the slightest reason why you should not use the stomach pump in cases of antimonial poisoning, and wash out the stomach. But you must bear in mind that poisons are sometimes given, not singly, but in company with others; antimony may be given with another poison such as opium, which dulls the sensibility of the stomach and possibly lessens the tendency to vomit. Tannin precipitates the salts of anti-

mony, and solutions containing tannin are therefore recommended; boiled tea, boiled oak bark, or any other body containing tannin may be given. In a trial, of which I shall give you the facts presently, it was alleged that the antimony was given in some burgundy, and evidence was given at the trial that a solution of tartar emetic could not be mixed with burgundy without great danger of the consumer having his attention arrested by the fact, because the tannin in the burgundy would cause a precipitation of the antimony. I have, however, mixed some solution of tartar emetic with rough burgundy without causing any cloudiness of the wine.

The only good case of acute antimonial poisoning which has been before the public of late years was that known at the time as the "Balham Mystery" or the "Bravo case." An inquiry was held concerning the death of Mr. Bravo, barrister, aged thirty, who was living in Balham, and was taken ill on Wednesday April 18th, 1876, and died on April 21st. When first seen by a medical man at 10.30 on the night of the 18th, he was leaning back in an armchair in his bedroom, perfectly unconscious and the pulse scarcely perceptible, and the doctor's first notion was that he was suffering from narcotic poisoning. The history given was that after going to his bedroom he was heard to cry out with sudden pain, and that he had vomited after mustard, and the judgment ultimately formed was that he was suffering from an irritant poison. He vomited bloody matter and passed bloody urine, and was in the most excruciating pain. He remained unconscious for about five hours. Sir George Johnson (then Dr. Johnson), the first consultant to see him, said that his first impression on seeing the patient was that he had had an epileptic fit, but the bloody vomit soon aroused the suspicion of collapse from irritant poison. It was supposed that the antimony had been given in burgundy, and the witnesses all concurred in the statement that the antimony and burgundy would make a muddy, cloudy mixture, owing to the tannin. He died thirty hours after the dose. Bravo had vomited once out of a window and into a leaden gutter. This vomit was kept separate, and this, as well as other vomit, was analysed by Professor Redwood. In both he found antimony, and in the first lead also, owing to its having been scraped from the leaden gutter. Antimony was also found

in the urine, in the intestinal contents, and in the liver. The professor said a man might take thirty or forty grains of tartar emetic in a glass of water without tasting it so forcibly as to make him desist. That is true enough. If a man were to sip his wine he might notice a strange taste, but if he took a draught of it that would not be the case. Thirty or forty grains could easily be got into a glass of liquid, because it is so easily soluble. The *post-mortem* examination was made by Dr. Payne, of St. Thomas's Hospital. A toxicological *post-mortem* made by a trained morbid anatomist is of peculiar value, and Dr. Payne stated that "there was no appearance of inflammation, congestion, or ulceration of the stomach. The colour of stomach and intestines was yellowish. At the lower part of the small intestines there were some red spots. The large intestine was of a deep red colour, and contained clots of blood, and there were small ulcers in the cæcum, the probable source of the hæmorrhage. The large intestine was filled with a red material, fæces mixed with blood. All the other organs were healthy. The lungs were congested. All cavities of the heart contained blood." The witness, in answer to questions, stated that the antimony had probably been given in a diluted form, owing to the absence of inflammation of the stomach. The condition of the large intestine and rectum quite bears out what I have said before, that the large intestine is a powerful channel of elimination, and it is very interesting that this man who lived *three days* had practically nothing the matter with the upper part of his alimentary tract, while the lower part of his alimentary tract showed intense inflammation and congestion and hæmorrhage; the same thing is noticeable in mercurial poisoning. The result of this Bravo case was that great suspicion fell upon his wife, but it was never discovered where the antimony was bought, or who bought it, or who gave it, and nobody was put upon trial for the murder. Now comes the explanation. It came out afterwards, as an undoubted fact, that Mrs. Bravo was a dipsomaniac, and it was said that Bravo had some quack powders for the cure of dipsomania which contained antimony. The explanation of the case ultimately given was this: that for some reason or another Bravo first took opium, and then swallowed the antimonial powder as an emetic. The cause of the comatose state in which he was found was probably opium,

while the antimony did not really act until later. At the time, all the papers wrote as if the woman murdered her husband, and, indeed, the facts looked very black. As a matter of fact, there was no vestige of evidence on that head.

Now, with regard to the tests for antimony. I have shown you a good many already. I showed you that antimony, that is to say tartar emetic, is easily soluble; we will dissolve some and heat it over a spirit lamp. You see that nearly the whole of it is dissolved in a very short time. Antimony is much more soluble than arsenic. You proceed with this analysis as you would with any other, and the first reagent you add is hydrochloric acid. That produces a white precipitate. I ask you to remember that, because in your books on qualitative analysis you read of silver, lead, and subsalt of mercury coming down with hydrochloric acid. So does antimony. Antimony, however, is soluble in excess of the acid, while the other three metals are not. I mention that most particularly, because when you are pressed for time and making analyses at your examinations, you add a little hydrochloric acid to a solution of tartar emetic, and you rush to the conclusion that you have silver, lead, or a subsalt of mercury. Remember that antimony is soluble in excess of the acid. When you dilute it again, by pouring into it a large quantity of water, you get a re-precipitation of the antimonial salt. That is a very important matter to remember. The next test is sulphuretted hydrogen. I now add some sulphuretted hydrogen to the original solution, and an orange-yellow precipitate is caused. You see it is dissolved in a large quantity of hot hydrochloric acid, and then, on pouring it into the water, we get the antimonial salt reprecipitated. I have showed you Reinsch's test with copper, and also Marsh's test, and I need not repeat them now.

We now go to another poison, *mercury*. That is one of those things which has been given in the metallic state, in large quantities, without causing any toxic symptoms whatever. In the former days it was not uncommon to give as much as one pound of liquid mercury to overcome intestinal obstruction. When mercury was given in that way it may have done mechanical injury, but it did not produce any toxic symptoms. Mercury is not absorbed unless it is given in a state of very fine subdivision, as in blue pill or grey powder.

The vapours of mercury, of course, cause constitutional symptoms—salivation, dysenteric diarrhæa, and swelling of the gums. There is also another constitutional symptom from which workers in mercury suffer, and that is mercurial tremor. Mercurial tremor is getting more and more rare in this country, because mercury is less and less used in the arts. Looking-glasses are no longer silvered with an amalgam of mercury, they are coated by some manipulation with the salts of silver itself. But the making of mirrors with mercurial amalgam was at one time a common cause of mercurial tremor, and some of you may have seen an example of it in an old Italian mirror maker who has been under my care periodically for what is known in the trade as "the trembles." His attacks of tremor were quite characteristic. Sometimes they attacked him in the legs so that he could not walk, and sometimes in the upper limbs so that he could not feed himself. These tremors were coarse tremors, and were quite indistinguishable from the tremors of disseminated sclerosis; that is to say, he was fairly quiet until he tried to pick up something, when he began to shake violently. Therefore it was an "intention tremor." The only way in which the mercurial tremor in the patient I am alluding to differed from the disseminated sclerosis, was that he had had repeated attacks and had got well, whereas disseminated sclerosis is one of those things, the diagnosis of which means that the patient's days are numbered. There are several other trades in which mercurial poisoning is common, such as mercury miners, furriers, preservers of dead animals (taxidermists, as they call themselves), and hatters. It is a very dangerous thing when mercury is spilled about a house or about a ship. In 1809 a remarkable instance of the noxious effects of mercurial vapours was observed in the case of the ship *Triumph*, while conveying a cargo of quicksilver off Cadiz in April, 1809. By some accident the leathern bags containing the metal burst, and three tons of quicksilver were dispersed through the vessel. The crew soon began to suffer from salivation, partial paralysis, and disorders of the bowels. In three weeks no fewer than two hundred men were salivated. Two men died from excessive salivation; one lost some of his teeth, and his cheeks were in a gangrenous condition; the other lost

the whole of his teeth, the greater part of his tongue, and at the time of his death the lower lip was in a state of gangrene. The interior of the ship was covered with a black powder, and the copper bolts were mercurialised. The vapour proved fatal to the animals on board, for nearly all the poultry, sheep, pigs, mice, goats, cats, and dogs, and even the canary bird died from its effects (Paris and Fonblanque). The poison in this case was not merely the vapour of metallic mercury, but probably in part oxide of mercury produced by friction, and diffused as fine dust.

Mercury, like other irritants which I have mentioned, such as antimony and arsenic, is an irritant when applied externally. There is a case on record, a very sad one, in which, to cure an eczematous scalp, a strong solution of perchloride of mercury was applied to the head of a child. Sloughing of the scalp resulted, and the child died of mercurial poisoning. I remember an instance, when I was at the hospital, of a student who, being troubled with pediculi pubis, applied perchloride of mercury, 80 grains to the ounce, with the result that he not only destroyed the pediculi but very nearly destroyed their habitat, root and branch. He turned homœopath, which, perhaps, is not surprising. You may remember that I had under my care in the hospital, a year or so ago, a case of mercurial dysentery which had arisen from the application of solution of perchloride of mercury for an extensive skin disease. Therefore, you must be very careful when you use these mercurial preparations, whether externally or internally. Chronic mercurialism, like other chronic metallic poisoning, causes anæmia; it causes lassitude, salivation, inflammation of the mucous membranes of the mouth and gums, and tremor. There has also been described "mercurial erythema," in which there is some mental excitement, nervousness, sleeplessness, morbid shyness, and an indisposition to go into society, and which is said to pass off entirely when the effects of the mercury have worn off. I need hardly say mercury is especially dangerous when there is chronic disease of the kidneys. In the old days people were salivated much more than they are at present, and it is said that children have been born of mercurialised mothers with tremor and salivation. I give you that for what it is worth; I should not attach very much importance to it. The most common salt

and the most dangerous salt is mercuric chloride, corrosive sublimate. Corrosive sublimate is very easily soluble in water and also in alcohol and all kinds of spirits. It is said there is a certain amount of toleration for corrosive sublimate, and it is said that in some parts of Asia Minor there are sublimate eaters who take it constantly. It is also said that they use it for very much the same purpose as opium is used in other parts of the world. When corrosive sublimate is given in large quantities, you get the symptoms of acute irritant poisoning, symptoms which are indistinguishable from the symptoms which I read to you in connection with arsenic—intense pain, vomiting, retching, collapse, and so forth; and in corrosive sublimate poisoning the urine is often bloody and albuminous, and it causes inflammation of the kidneys if given in large quantities, and, in consequence, the urine is sometimes suppressed.

Other symptoms which I will incidentally mention are, if the patient lives, dysentery and tenesmus. Of course, the dose of mercury which may produce salivation differs enormously, and, if there is any disease of the kidneys, a very small dose is sufficient to give it. I have never seen a man really salivated but once in the old-fashioned way, and I may tell you how that happened. I was at the time in charge of the Great Eastern steamship, and we had been three months at sea cable laying, and there had been some want of fresh meat and vegetables amongst the crew. Nothing favours salivation more than a taint of scurvy, and I accidentally gave three grains of calomel to a man who had a scorbutic taint upon him. I remember perfectly well the swelling of his gums, and how he lay in his bunk with tongue swollen, gums swollen, and the saliva running from him in enormous quantity.

It is said by some that salivation is one of those things which may cease and recur. That I think is a very doubtful statement. It may recur if the cause recur, but that it does so without a repetition of the cause I very much doubt. At Reading, in 1845, a man labouring under disease of the kidneys had placed himself in the hands of a quack who promised to cure him. Part of the treatment consisted in the administration of small doses of mercury. Profuse salivation came on, and the patient ultimately died. A coroner's inquest was held, in order to determine whether he had not

died from improper treatment. It appeared in evidence that some calomel pills were prescribed; and that, had the prescription been followed, the deceased would have taken no more than six grains in the five days that he was under treatment; but, in consequence of some mistake, he took $11\frac{1}{2}$ grains, *i. e.* $2\frac{1}{2}$ grains daily for five days. On an inspection of the body, the gums were found ulcerated, and the mucous membrane of the tongue, mouth, and throat was in a state of intense inflammation. Both kidneys were enlarged and in a diseased state. After hearing the evidence of several medical witnesses, the jury returned a verdict that the deceased died from natural causes. Mercury and quacks are both things which exist in nature, and I suppose the verdict, if we look at it philosophically, was a correct one.

In poisoning with corrosive sublimate, you find *post-mortem* inflammation, softening, and erosion of the stomach; and it is exceedingly common to find a great deal of blackening, probably owing to the fact that the intestines get covered with mercuric sulphide. The large intestine as well as the stomach gets coated with a black deposit; the kidneys are inflamed very often as a result of giving mercury, and there is one case on record of perforation of the stomach from a large dose of corrosive sublimate. Three grains of corrosive sublimate are said to have killed a child, and five or six grains have killed an adult. Recovery, however, has taken place after thirty or forty grains. In these poisons which cause vomiting, the lethal dose is very difficult to fix. The shortest fatal period on record appears to be three and a quarter hours; on the other hand life may be prolonged for some time after a dose of corrosive sublimate has been taken.

Now, the treatment of corrosive sublimate poisoning undoubtedly would be the stomach-pump; and one of the best antidotes you can give is white of egg. I have here some white of egg in water, and to it I will add a little solution of corrosive sublimate. When I do so you will notice there is immediate clotting.

I will now show you a test for mercury. You will remember that with a beer containing mercury we got a mirror-like deposit upon the copper. I now put a solution of perchloride of mercury into this beer, and we will imagine we have done Reinsch's test and got a mirror of mercury. We

add some hydrochloric acid, and pour a drop of the solution on to a sovereign, and touch the sovereign through the solution with a piece of metal, such as a key; and in that way you may get the sovereign coated with mercury. That is a very useful test, and a perfectly correct one. If you have not got a sovereign a penny will do. Of course, by heating the sovereign you can get rid of the mercurial stain.

With regard to the liquid tests for mercury, confirmatory tests only, I begin in the usual way with hydrochloric acid and get a negative result. Next, with sulphuretted hydrogen, you get precipitation of sulphur, and after a time, if you use enough, you get a yellow precipitate which turns black. I will now go on and use ammonium sulphide, when I get a black precipitate. If you want a confirmatory test, one of the best you can use is potassium iodide, which gives you a red precipitate. Therefore, we have no doubt that we have a persalt of mercury. Now take the subsalt of mercury (the mercurous nitrate), add hydrochloric acid, and you get a white precipitate. You want to know what that precipitate is, and you add ammonia. If it is silver it will be dissolved, but by adding ammonia we see it is blackened. You have to remember that when you have given your white of egg to your patient in order to cause coagulation of the albumen, you have got in the stomach an albuminate of mercury, and that that albuminate of mercury may be digested and absorbed, and having given your white of egg it is none the less incumbent upon you to get that white of egg out of the patient's stomach as quickly as possible by washing out the stomach and by giving a purgative.

TREATMENT OF RHEUMATISM.

CATRIN* thinks that the usefulness of salicylate of methyl in relieving the pain of acute articular rheumatism is incontestable. The doses commonly employed are as much as one ounce a day. In regard to the local use of the salicylate of methyl, bands of tarlatan soaked in salicylate of methyl may be wrapped around the joint and covered by an impermeable dressing, with great advantage to the patient.

Mosher† initiates treatment with large doses of

the salicylate, and continues for from three to five days, till the good effects against the rheumatic condition begin to be felt. Then daily doses of salophen to the extent of 30 to 45 grains are given. This drug is not absorbed in the stomach. On reaching the intestine it is split up into salicylic acid and acetyl-paramidophenol, and is always well borne. This is kept up as long as treatment for the condition is needed.

Klimenko* has employed salophen in forty cases, giving 15 grains six times daily. It has a good effect in acute and chronic articular and muscular rheumatism.

According to F. Falk,† piperazin (Bayer) is an ideal therapeutic agent in chronic rheumatism; it controls the pain without the use of opiates, and a general improvement takes place at once.

C. W. Canan‡ states that for the past seven years he has used tongalin with particularly good results in rheumatism. In very severe cases of inflammatory rheumatism he begins by giving a teaspoonful every hour, in a wineglassful of water as hot as the patient can bear it. After each dose more hot water, as much as the patient can take, is given. If the stomach will not tolerate tongalin, he has the painful parts sponged with alcohol or soda water, preferably the latter, and then rubbed with tongalin. Heat is then applied by means of a hot-water bag or by some other convenient method.

J. Lee Beck§ states that in acute articular rheumatism, electricity in any form is of doubtful value.

Subacute articular rheumatism is far more favourably affected by electrical treatment than the acute form, but even in these cases the remedy acts with a degree of uncertainty, and is often very discouraging. There is only one satisfactory method of electrical treatment in these cases, and that is the method of general faradisation with the descending current. Purely local applications, while not altogether useless, are by no means so efficient as the general method. All three forms of electricity—galvanic, faradic, and static—are of value in the treatment of muscular rheumatism. In muscular rheumatism of the neuralgic type, and with considerable tenderness to pressure, especially slight pressure, the galvanic, or the faradic current

* 'Therap. Monats.,' July, 1898.

† 'Journ. of Pract. Med.,' April, 1898.

‡ 'St. Louis Med. and Surg. Journ.,' 'N. Y. Med. Journ.,' January 15th, 1898.

§ 'Med. World,' May, 1898.

* 'Journ. de Méd. de Paris,' June 11th, 1898.

† 'La Semaine Méd.,' No. 46, 1898.

of high tension, is recommended, preferably the galvanic. In chronic cases the method to be adopted is the simple one of static insulation and submitting the patient to the effects of the roller electrode over the affected parts. In consideration of the treatment of the chronic articular forms of rheumatism, the application of the faradic brush should be mentioned. A vigorous current, sufficient to produce intense redness of the skin, is sometimes followed by sleep.

The value of the electric bath in chronic rheumatism is strongly supported by Chauvet.* He either uses pure Royal water or adds 2 per cent. lithium chloride to it. The current is produced by a battery of 50 Leclanché cells, and is employed at an intensity of from 15 to 30 milliampères. The electrodes consist of charcoal covered with flannel, and each sitting lasts from ten to thirty minutes. The treatment is not indicated where Heberden's nodes are found to exist. In any case, however, it does no harm, and during its continuance there is an absence of the acute exacerbations which are so common in rheumatoid arthritis.

A. Bier† says that some years ago he used the hot air treatment for chronic rheumatism. It undoubtedly lessens in a striking manner both the pain and stiffness in the joint. Almost the only effect produced by the hot air is the active hyperæmia. Better results can be obtained, however, by passive hyperæmia. The leg should be bandaged from the toes up to the affected knee, and then an elastic bandage should be applied above the knee over another bandage. Each case must be observed as to how far the passive hyperæmia can be borne with safety. It must not cause pain, and if pain is previously present it should be relieved by it. If the joint is painful the method may be used continuously, but the position of the bandage must be changed every twelve hours. In other cases it may be employed either during the day or night. The diseased joint itself is not bandaged.

Gustav Singer‡ details the treatment of acute articular rheumatism by intra-venous injections of sublimate, as follows: The arm is left hanging over the edge of the bed, in order to bring the veins into prominence, or a compress above the elbow is employed for this purpose. The skin in the region of the elbow is then disinfected first by

energetic rubbing with soft soap and warm water, next with a $\frac{1}{10}$ per cent. sublimate solution, and lastly with ether and alcohol. A sterilised syringe provided with an asbestos piston is filled with the solution and inserted into the lumen of the vein in the direction of the blood-current and parallel to its course. As soon as the colour of the solution in the syringe has indicated that the needle has entered the vein (blood entering reddens the solution), the compress is removed and the injection is slowly made. The point of insertion is covered with the finger, the needle removed, and the opening closed with iodoform gauze and an appropriate bandage. The solution employed is corrosive mercuric chloride, 1 or 2; sodium chloride, 1 or 2; distilled water, 10. The dose of sublimate is from one sixth to one third grain. The injection can be made alternately in each arm. Six to ten injections in the course of treatment, at most, being necessary, it is well to begin near the wrist, and later inject nearer the elbow. If the weaker solution is employed, daily injections are given; if the stronger, then once in two days is sufficient. The mouth and teeth must be carefully looked after and cleansed. For this purpose potassium chlorate is used as a mouth-wash. The indications for this method are—acute articular rheumatism of infectious initial processes (erysipelas, phlegmon, puerperal affections) or peculiar history (chills, recurring endocarditis, frequent exacerbation, protracted subacute course, metastases) which point toward a pyæmic character. In acute disease, when the salicylates are forbidden, or when the inflammation is localised in single joints, this method is recommended. The contra-indications are to be found in very weak individuals, severe renal degeneration, and by onset of severe symptoms of poisoning.

John O'Connor* reports two cases of acute articular rheumatism that were subjected to surgical treatment, consisting in opening the joint by one or more incisions, thorough irrigation of the wound, and packing the wounds with mercuric-chloride gauze. In one case the ankle, elbow, and knee were subjected to this treatment at one operation, and in another both wrist-joints and the left knee. The results obtained in each were excellent. In those cases that do not respond to internal medication, prompt resort to surgical procedure is thoroughly justifiable.

The Monthly Cyclopædia, February, 1899.

* 'Glasgow Med. Journ.,' September, 1898.

* 'Arch. d'Elect. Méd.,' April 15th, 1898.

† 'Munch. med. Woch.,' August 2nd, 1898.

‡ 'Centralb. f. d. Gesamte Therap.,' H. 1, S. 1, 1898.

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* Specially reported for *The Clinical Journal*. Revised by the Author.

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A POST-GRADUATE LECTURE

ON

CHRONIC PELVIC INFLAMMATION.

Delivered at Charing Cross Hospital, Feb. 2nd, 1899,

By T. W. EDEN, M.D., C.M., M.R.C.P.,

Assistant Obstetric Physician to the Hospital; Senior Out-patient Physician, Chelsea Hospital for Women.

LADIES AND GENTLEMEN,—I want to speak this afternoon on the subject of chronic pelvic inflammation, and to ask you to look at some examples of this condition in the wards.

The liability of the female genital tract to inflammatory infection is so well recognised that it is unnecessary to emphasise it. Acute pelvic inflammation, comprising such lesions as acute cellulitis and peritonitis, or the formation of pus in the Fallopian tubes, cellular tissue, or peritoneal cavity is a very grave condition, and we are all familiar with its importance. There exists, however, a large class of cases less obtrusive than those just mentioned, which, though they cause prolonged suffering and ill-health, are not attended with risk to life, and therefore often pass unrecognised. Such cases are often classed as neurotic, and their sufferings attributed to "nerves." It is true that patients suffering from chronic pelvic inflammation are very liable ultimately to become neurotic also, but it is important not to miss the inflammatory lesions which are the primary cause of their hysteria. It is, of course, true also that there are numbers of women with apparently healthy pelvic organs, who suffer from symptoms which closely mimic the symptoms of pelvic inflammation. But I am convinced that the neurotic class is often unduly swelled by the wrongful inclusion among them of patients who are really suffering from chronic pelvic disease, and we have always to bear in mind that, next to hysteria, chronic pelvic inflammation is probably the most common cause of confirmed invalidism in women.

Owing to the bacteriological advances which

have been made in the last generation, we know a good deal more about the causes of pelvic inflammation than we knew twenty or twenty-five years ago. In the text-books of that period a prominent place in causation is given to such conditions as chill or over-exertion during the menstrual periods, excessive indulgence in sexual intercourse, and such like. We do not accept such explanations now, or we regard them with extreme caution. We recognise that inflammation, wherever it may be, is the reaction of the tissues to an irritant conveyed to them from without or elaborated in the organism itself, and usually the irritant is a micro-organism. Therefore we believe that the predominant factor in the causation of pelvic inflammation is microbial invasion, and we also know fairly well what are the principal organisms concerned. We recognise three principal groups. First of all there is the SEPTIC GROUP, due to the invasion of the tissues by the common organisms of suppuration—the *streptococcus pyogenes albus* and *aureus*, the *staphylococcus pyogenes*, &c. The second group comprises those due to GONORRHOEA; they are caused by the invasion of the tissues by the gonococcus of Neisser, an organism which brings in its train others of a different kind, leading to what bacteriologists call "mixed infection." The third group of cases are those in which the inflammation is due to the *tubercle bacillus*. In addition to the three foregoing, there is another smaller class, but one which is well recognised, viz. the class due to bowel infection, that is, to the invasion of the pelvic tissues by the *Bacillus coli communis*. A number of cases are on record where, following dysentery or enteric fever, there have been adhesions of bowel to the tubes and ovaries, and, as a result of this, suppuration has occurred in these organs. These are what are known as cases of bowel infection. And, further, we must of course recognise that the mucous membrane of the uterus and Fallopian tubes, like the mucous membranes of other parts of the body, is liable to that form of inflammation which, wherever it occurs, is known as catarrhal, but this is a simple form of inflammation which does not tend to spread to the peritoneum or cellular tissue. So that, excluding these latter cases, we may say that pelvic inflammation is an infective disorder due to sepsis, to gonorrhœa, to tubercle, or to infection from the bowel by the *Bacillus coli communis*.

Pelvic inflammation may be either acute or chronic in its course. It is quite a mistake to conclude that, because after a difficult labour or an instrumental abortion the patient did not immediately suffer from an acute illness such as cellulitis, that therefore she was not infected at the time; she may have been infected by a poison less virulent and manifesting itself less rapidly, and leading to less well-marked changes than that of acute inflammation. She may, therefore, not become ill until three or four weeks after her confinement or miscarriage. Then she develops the train of symptoms which I shall describe as those of chronic pelvic inflammation. Or again, chronic cases may be the residue of acute lesions. You may have a patient who was suffering in the first place from acute cellulitis which resolved to a certain extent, but left well-marked lesions behind it.

We may now go on to describe the lesions which may be met with in chronic pelvic inflammation, and the symptoms to which they give rise. In nearly all cases the lesions are multiple, and this point is important in connection with diagnosis. In the first place, there are, in most cases, signs of traumatism; one finds perineal cicatrices, deep lacerations of the cervix, with eversion of the lips of the os, and cicatricial bands in the vaginal roof indicating old tears. All such lesions, it must be remembered, have been possible channels of infection. In the second place, the uterus is always enlarged, generally tender, often retroverted or retroflexed, and, most important, more or less limited in mobility with sometimes complete fixation. Outside the uterus one generally finds either one or more well-defined localised swellings, or indefinite areas of thickening and tender resistance in the region of the broad ligaments or of the utero-sacral ligaments, or in the position of the tubes and ovaries. Thirdly, similar swellings may rarely be made out in a position remote from the uterus, such as the iliac fossa, above Poupart's ligament, or even in the buttock. These latter are cases of "*remote parametritis*," and are explained by the fact that at one time there was an extensive pelvic cellulitis which spread to the abdominal wall or the iliac fossa, or through the sciatic notch, and the outlying parts have either persisted in the form of inflammatory thickening or gone on to suppuration, while around the

uterus the inflammation has resolved. The immobility of the uterus, and sometimes its displacement also, are the result of pelvic peritonitis leading to adhesions of the uterus to surrounding parts, generally to the pouch of Douglas. The enlargement of the uterus is due to endometritis. The extra-uterine lesions represent salpingo-oöphoritis, cellulitis, or localised peritonitis. As a rule, it is possible to make out the presence of these lesions by a careful bimanual examination, and the greatest possible assistance will be derived by examination *per rectum*, either recto-abdominal, or recto-vagino-abdominal. If there is any special difficulty, it will be advisable, so as to be clear, to examine under anæsthesia. To test the degree of immobility of the uterus, it is better to confine oneself to the use of the fingers rather than to pass a sound. It is not always easy to determine what structures are actually involved in the extra-uterine swelling, but the point is not very important either for prognosis or treatment.

The symptoms to which these conditions give rise are as follows:—In the first place, the patient always complains of pain; she puts in the forefront of her complaints that for months or years she has never been free from pain. This pain she feels in the back, in the lower part of the abdomen, on one or both sides, and down the thighs. She also says that after walking a short distance she finds the pain much increased. She may tell you that she has had to give up all active employment, although previously of energetic habits. Generally the pain is relieved if she lies up for a few hours. We learn that her periods come regularly but are too profuse, that she loses much more than she was accustomed to lose, and that the pain of which she complains is very much aggravated at these times; so much so that she may be laid up for a part of the menstrual period. The increase of pain generally antedates the menstrual flow. She also suffers more or less discomfort, or it may be actual pain, on micturition, and there is generally also frequency. In the menstrual intervals there is a more or less profuse muco-purulent discharge. The pain is mainly due to the pelvic peritonitis. Mere enlargement or backward displacement of the uterus of themselves do not cause very much pain, if the pelvic peritoneum remains healthy and the uterus is still movable; pelvic cellulitis is not a painful affection unless it

spreads to the peritoneum. The increased loss of blood of which the patient complains is due chiefly to endometritis. The endometritis is also responsible for the free muco-purulent discharge which she complains of during the menstrual period. A great deal of the bladder discomfort is often due to involvement of the peritoneum which covers the bladder; occasionally, as in gonorrhœal cases, the bladder symptoms are due to the persistence of chronic urethritis, and if you examine your patient carefully you will find that on pressure a little milky or turbid secretion can be expressed from the urethral canal. You may also see the deep red area of injection around the mouths of the ducts of Bartholini's glands, sometimes called the "gonorrhœal macula."

Now, the history of the onset of the illness in these cases is often very instructive from the point of view of determining the cause which has been at work in each individual case. The typical history in a septic case is that the patient had a difficult confinement or abortion, possibly requiring manual or instrumental interference. Since this occurrence she has never been well. The symptoms she complains of began insidiously; then as weeks and months passed on they gradually increased in severity until at length she could hardly bear them. She is occasionally better during her menstrual intervals, and seems to be improving; but when her next menstruation comes on there is an exacerbation of pain, with possibly some rise of temperature to 100° F., and thus she loses ground again. These patients go on slowly from bad to worse, until at length they become confirmed invalids.

The history of chronic gonorrhœal infection is usually different from this. Very often these patients have never conceived; but that is not by any means always the case, because gonorrhœa is a frequent cause of miscarriage. But if the patient has never been pregnant, and has not had an operation performed upon her, the chances are that she is the subject of gonorrhœal infection. The tendency of gonorrhœa to persist in the cervical canal and urethra, often affords aid in diagnosis, for although the discharges at this period do not contain the gonococcus, even in undoubtedly gonorrhœal cases, the persistence of these lesions is highly suggestive. In gonorrhœal infection, too, there is often a history of infection

of the external parts, and perhaps of abscess formation in the vulva. These conditions are rare in septic cases, but are common in those in which gonorrhœa is the cause. And lastly, in gonorrhœal cases the onset is usually marked by urinary symptoms.

As regards the tubercular cases the difficulties are very much greater. One can only diagnose tubercular disease of pelvic organs by a process of exclusion. If you can exclude, with a fair amount of certainty, septic infection and gonorrhœal infection, all you can say is that the next commonest cause of chronic inflammation is tubercular disease. Tubercular disease of the tubes, ovaries, and uterus is rarely primary, but much more often secondary to tubercular lesions in other parts of the body, such as pulmonary phthisis or tabes mesenterica, and tubercular peritonitis. Therefore in such cases you can often find signs of tubercle in the lungs or in the abdomen, and then it is a reasonable assumption that the pelvic disease is tubercular also. Of course you may be wrong in that supposition, because a woman who is the subject of tubercular disease may also become infected through the ordinary channels, and the case may be complicated by a second factor. Still, after all the deduction is a fair one. Occasionally it is possible to get some direct evidence of tubercular disease. For instance, uterine scrapings may show well-marked giant cells and tubercle bacilli, proving the existence of tubercular endometritis, and the conclusion is then justifiable that the thickening in the region of the tubes and ovaries is due to tubercular disease also, but aid of this kind is rarely forthcoming. The way in which the tubercular infection reaches the pelvis is a question which has not been settled yet. There are two possible channels. In the first place the bacilli may make their way into the circulation and be carried by the blood to the pelvic organs; or they may reach the pelvis directly through the genital canal. There is a certain amount of evidence accumulating that tubercle bacilli can be carried to the uterus and tubes by tubercular semen, but it cannot be said that this point has been proved to satisfaction.

The sources of septic infection, perhaps, deserve one or two further words. I think we must admit that a woman who is the subject of septic pelvic inflammation is a woman who has been infected

either by dirty fingers or dirty instruments. There is no evidence whatever to show that septic inflammation ever occurs from vitiated air. It used to be taught that puerperal fever might arise from contamination by sewer gas. The view, however, which I think is now held by most responsible obstetricians is that sewer gas causes sewer-gas poisoning, but not septicæmia. That opinion is supported by the results of the bacteriological examination of sewer gas, which it is found does not contain *streptococci* and *staphylococci*. We believe that septic diseases of all kinds are due to these organisms; and if these organisms are not present in sewer gas, sewer gas cannot be the cause of septicæmia. We must be prepared to face this point, and to make up our minds that when septic disease occurs it is due to communication from without. It is therefore a point of primary importance that even the minor operations of gynecology shall be carried out on aseptic methods. There is a good deal said about the predisposition of the pelvic organs to infection, a sort of arrangement for which we are not responsible, but which renders these organs specially liable to infection, so that no matter how much care we may take these accidents must sometimes occur. This so-called predisposition on the part of the pelvic organs is said to be displayed in the first place by the fact that every month a certain amount of denudation of the endometrium takes place in menstruation; and secondly, that in abortions, or as a result of full-time labour, an extensive denudation of the uterine wall occurs, and thus blood-vessels and lymphatic channels are laid open for the possible absorption of infective matters. Also it frequently happens that blood-clot or other dead tissue is retained in the uterine cavity, which may form a suitable nidus for the growth of organisms if they find their way into it. But it is clear that the fact that blood channels and lymphatic channels are laid open, and that decomposable matter is retained in the uterus, does not of itself explain the occurrence of septicæmia. One more factor is necessary, and that is the introduction of the organisms; and if we refrained from introducing the organisms, these so-called causes of pelvic inflammation would be quite inoperative.

I may now, perhaps, relate to you one or two cases which have occurred to me within the last few years, illustrating some of the points mentioned.

A married lady, æt. 33, consulted me in August, 1897. She had had five confinements and two miscarriages. The first three children were living, the fourth dead. Then followed two miscarriages, and the last confinement, also with a dead child, occurred upwards of five years previously. After this confinement she had an attack of peritonitis of several weeks' duration, and since this event she had never been well. She complained of continual pain in the back, and inability to sustain exertion of any kind. She had frequent exacerbations of pain, and at such times she often had a rise of temperature to 100° F. The periods lasted fourteen days, while previously they lasted only seven. For the first week the loss was moderate; then for a few days it became very profuse, and the pain was greatly increased. During the menstrual intervals there was a free whitish-yellow discharge, and frequently there was irritability of the bladder and pain on micturition. Her life, she said, was not worth living under present conditions. She has been twice curetted without benefit, the last time indeed with positive aggravation of her sufferings. On vaginal examination the cervix was found to be deeply split on both sides, with erosion and eversion of the lips of the os. The uterus was bulky, hard, tender, lying in the anterior fornix, with only a slight angle of flexion, and was greatly limited in mobility. Attempts to move it caused pain. Both ovaries and tubes could be easily palpated, and they were thickened and tender. There were no deposits in the broad ligaments or the utero-sacral ligaments, and the pouch of Douglas was apparently free.

You see this patient had a moderately enlarged uterus, and a damaged cervix; and the existence of pelvic peritonitis was inferred from the immobility of the uterus and the pain which attempts to move it caused, and lastly, from the thickening, matting, and tenderness of the tubes and ovaries. Though this patient had been twice curetted she was extremely anxious that something further should be done for her. It had been suggested to her that her tubes and ovaries should be removed, but, for reasons which I shall have to mention shortly, I decided to remove her uterus. This I did by vaginal hysterectomy. I found the tubes and ovaries were simply adherent to the back of the broad ligament, so I separated the adhesions and left them. The uterus was much

enlarged, and exceeded the normal weight of the multiparous uterus by nearly 50 per cent. The utero-vesical pouch was obliterated by adhesions, but there were no purulent collections in any part of the pelvis. The patient has been entirely cured of her trouble. She, of course, does not now menstruate, and therefore she escapes the severe monthly loss of blood; she has no discharge, no pain, no bladder symptoms, and has had practically none of the symptoms which sometimes attend an artificial menopause.

Another case was that of an unmarried woman, æt. 38, who consulted me in July, 1896. She had the reputation of being a confirmed neurotic, and her medical attendant sent her to me more because she had become a nuisance to him than for any other reason. She had a history of an attack of perityphlitis in 1895, and she had also been treated for backward displacement of the womb. She complained of pain in the right iliac region, and in almost every other part of her body. There was constipation, dyspepsia, nausea, but no vomiting. In appearance she was a typical neurotic. She flushed readily, her lips trembled as she spoke, she wept as she recounted her troubles, and groaned with agony at the gentlest touch. Her periods were regular and moderate in amount. She had never been pregnant. On examination the uterus was found to be bulky and anteverted. Behind it was a tense, round, fixed swelling about the size of a tangerine orange, and high up in the left fornix was a similar but larger swelling; their outlines could be best made out from the rectum. I diagnosed the presence of a double ovarian cyst, and at the operation, undertaken a month later, this diagnosis was confirmed. Both cysts were intimately adherent to surrounding structures, and their contents grumous. The patient made a good recovery until the end of the second week, when phlegmasia of the left leg set in. This in due time resolved, and she recovered. Probably the source of infection in this case was the bowel at the time of the attack of perityphlitis, the ovarian cysts becoming infected with the *Bacillus coli communis*. The case affords a good example of the way in which pelvic lesions may be masked by a train of hysterical symptoms.

We now come to the treatment of these conditions. The treatment of chronic pelvic inflammation is either *palliative* or *radical*.

The first degree of *palliative* treatment consists of prolonged rest in the recumbent position, either in bed or on a couch, combined with hot douches, glycerine or ichthyol plugs, and blistering, or counter-irritation by application of iodine to the anterior abdominal wall. There should be most careful attention to the general health, and to the *primæ viæ*. This method of treatment is a promising one if the patient has the means at her disposal to carry it out. Unfortunately, most of the cases that one sees in hospital practice are among those who find it impossible to lie up for any length of time; their great desire is to be put right as soon as possible, or at any rate to be made so much better that they can go back to their work. Still one can try the method I have mentioned in private practice, and the best results often follow if the patient has sufficient patience to carry it out, or if she has some insuperable objection to being operated upon. But it must be persevered in for a long time. After having the patient under your care for two or three months it is advisable to send her to one of the continental or home spas. Afterwards she can make a yearly pilgrimage to the particular spa which she affects, and by this means can often keep herself in fairly good health. I would warn you, however, that you must be prepared for disappointment in a considerable number of the cases which you treat in this way.

If this plan has been tried without much success, the next thing to do is to curette the uterus. This appears to act as follows:—Foci of infection remain in the endometrium, and a certain amount of absorption occurring through lymphatic channels from these foci, the inflammatory process is thus kept alive. The uterus thus forms a source of continual re-infection of the deep parts. If you curette the uterus and cauterise the raw surface afterwards, you will probably succeed in destroying the source of infection which has kept up the trouble, and give the lesions in the deeper parts a chance of resolving. It is often necessary to curette a second time.

If very little benefit accrues after the second time, the only thing left to do is to perform a radical operation. This consists either in removing the tubes and ovaries, or in removing the uterus and leaving the tubes and ovaries, or it may consist in removing the uterus, the tubes,

and the ovaries. All that one can say is that you must be guided by the conditions which are found as to what operation shall be done. It is advisable in many cases to take away the uterus. The almost universal practice on the Continent and in America is to remove all the pelvic organs, *i. e.* uterus, tubes, ovaries—to make a clean sweep of the whole thing. One is certainly often disappointed if one removes inflamed tubes and ovaries while leaving a chronically inflamed uterus. In such a case the patient goes on bleeding, either regularly or irregularly, her white discharges do not stop, and she has a good deal of pain. You may again have to curette the uterus once or twice, and even after you have done this you may have finally to remove it. Therefore the question has to be seriously considered whether it is not preferable to remove the uterus in such cases. If the ovaries can be saved, this is no doubt a great advantage to the patient, but if there is pus in the tubes or ovaries they must, of course, be removed. If there are purulent collections in the peritoneum or broad ligaments they must be evacuated, for if pus be left anywhere you will get a bad result. Therefore, curetting is contra-indicated if you have reason to believe that suppuration has occurred in any part of the pelvis, for not only is it useless, but you may get a rupture of the abscess into the general peritoneal cavity, leading to death from peritonitis. Generally speaking, I think the risk is rather in doing too little than in doing too much. It is better to remove organs whose functional capacities have been destroyed by disease than to leave them as a possible source of future trouble.

Primary Tuberculosis of the Cervix Uteri.

—F. S. Matthews reports a case of tuberculosis of the cervix which occurred in a negress 22 years old. Examination of the cervix showed it to be large, worm-eaten, and its cavity excavated. It bled freely. The growth extended to the vagina. The right tube and ovary were enlarged. The uterus and appendages were removed, and upon microscopical examination the cervix was found to be the seat of tuberculous inflammation. The uterus, appendages, and the broad ligament were free from tubercles.

Med. Rec., Dec. 17th; Amer. Journ. Obstet.

A CLINICAL LECTURE ON SENILE ENLARGEMENT OF THE PROSTATE.

Delivered at the Westminster Hospital, Dec. 6th, 1898,

By **CHARLES STONHAM, F.R.C.S.,**
Surgeon to the Hospital.

IN TWO PARTS.—PART I.

GENTLEMEN,—To-day I propose to say a few words on the subject of senile enlargement of the prostate; I do not call it hypertrophy, because, although the increase in its size is often so spoken of, yet, so far as its pathological anatomy is concerned, it is not such in the true sense of the word.

With regard to the ætiology of prostatic enlargement, there is nothing certainly known about it beyond the fact that the enlargement occurs more frequently in the better classes than it does among the poor, and that it practically never occurs before fifty-four or fifty-five years of age. There seems to be some probable association between a life of sexual vigour and the oncoming of enlargement of the prostate in later years, and it is suggested that at a time when a man's sexual vigour begins to leave him, the inherent prostatic energy is taken up in growth instead of in performing its sexual functions. This is at present mere hypothesis, and how far it may be true is a matter for doubt. It would appear that sedentary occupations are liable to produce enlarged prostate. It is suggested by some that antecedent gonorrhœa, perhaps repeated attacks of gonorrhœa, and especially when such have been associated with prostatic congestion or inflammation, may be forerunners of enlargement of the prostate. It used to be supposed that the condition was a perfectly physiological process, occurring as penalty for advancing years, but it is now well known that that is by no means true; a man may attain a great age without any prostatic enlargement at all, and the proportion of those who do suffer is comparatively small. While senile enlargement does not occur before fifty-four or fifty-five years of age, it rarely does so after sixty-five; if a man escapes up to that age he will probably escape entirely, or, if his prostate enlarges

at all, it will not be sufficient in degree to occasion much, if any, trouble.

The normal prostate gland consists of two lateral lobes and a median portion or isthmus, which projects into the trigonum vesicæ, at the part known as the uvula. Nearer its anterior than its posterior border is the prostatic urethra. The prostate is usually described as being about the size of a large horse-chestnut. Enlargement of the prostate may affect the whole gland, or one or other lobe; the resulting symptoms are not dependent upon the mere size, but upon the position of the enlargement. For example, you may get great increase in size of the lateral lobes in which the flow of urine is not appreciably interfered with, while on the other hand you may find a small prostate, which by rectal examination does not appear to be enlarged at all, yet in which case the obstructive symptoms may be exceedingly pronounced and serious. The portion of the prostate which produces the more serious symptoms is the middle lobe, projecting, as I have said, into the base of the trigonum vesicæ, behind the neck of the bladder, and thus offering an effectual bar to the escape of urine from the bladder. Occasionally you find that an hypertrophy of the prostate is a perfectly localised overgrowth, which may even become pedunculated and project from the surface of the prostate; such tumours are commonly known as prostatic adenomata. More usually, however, although the main stress of the enlargement falls upon a definite part, yet the whole gland is unequally enlarged. Now the effects upon the urethra vary according to the seat of the enlargement. If the right lobe is affected, the urethra may be elongated owing to the increased antero-posterior diameter of the gland; it will, moreover, be deflected to the opposite side. If both lobes are enlarged, the urethra will be deflected to the side least so. It will, moreover, become crescentic, the apices of the crescent looking towards the most enlarged side.

In the case of hypertrophy of the isthmus and middle lobe the crescent of the urethra will look downwards, the convexity of it upwards. The alteration in the urethra is not by any means the most important of the effects of enlarged prostate. The most important are those produced upon the bladder. The enlargement of the middle lobe acts like a ball-valve against the vesical neck as

soon as the patient tries to pass his water. The consequence is that there is almost, if not quite, complete blocking of the orifice if the patient attempts to bring into play his abdominal muscles or the muscles of his bladder wall. The effect of this must be that there is some chronic retention of urine, and this sooner or later produces a condition of atony of the bladder wall, which further aggravates the condition. The bladder wall, finding out, as it were, that its contraction is useless to expel the urine, ceases, or almost ceases to attempt it, and permanent atony results. Again, continued retention of urine tends to pouch the bladder behind the middle lobe of the prostate, so that, the orifice being above the fundus of the bladder, and the walls of the bladder themselves being useless to pump out the urine, that urine which does escape is merely overflow; the urine which is left behind is termed "residual." The effects upon the higher urinary tract will depend upon the duration of the mischief, on what complications may have arisen, and on the treatment which has been adopted. If treatment has been adopted early, and the retention of urine has been combated, and no cystitis or other troubles occur, the higher urinary tract may remain sound; but if, on the other hand, complications have set in, or treatment has been neglected, sooner or later the bladder condition will involve secondary mischief in the ureters and kidneys. The mechanical changes thus induced are dilatation of the ureters and hydronephrosis, and by the spread of inflammation, or from chronic irritation and congestion, chronic interstitial nephritis, and perhaps pyo-nephrosis, and other acute inflammatory conditions may be excited. Not a few of these patients die of an acute attack of nephritis, with scattered points of supuration—so-called "surgical kidney."

Now with regard to the symptoms, the patient will usually come to you complaining of frequency of micturition, and will probably tell you that he is suffering from "incontinence of urine." If you ask him when his incontinence is particularly noticeable, he will say at night, when he not only passes water more frequently than during the daytime, but also in greater quantity than normal, the quantity being usually quite as great as it is in the daytime. The man naturally enough thinks that he is really suffering from "incontinence of

urine," and if you try to persuade him that his bladder is full he is quite astonished and incredulous when he considers how much water he is passing, and how frequently repeated is the act. You may find, moreover, that not only is he passing urine frequently, but that it is even continually dribbling away from him, or at all events, that it does so when he uses any extra exertion or even coughs; such severity of symptoms indicates very marked obstruction with a large quantity of residual urine. In the very earliest stage of enlarged prostate, the frequency of micturition is doubtless dependent upon the vesical irritation due to the middle lobe projecting behind the neck of the bladder. In the latter stages it may also be partly due to this cause, but it is chiefly so to the fact that the patient never empties the bladder. Thus, supposing a bladder is capable of holding forty ounces of urine, and that only ten ounces are capable of being passed owing to obstruction at the neck of the bladder and concomitant atony of the bladder walls, it follows that when the man has secreted ten ounces of urine he wants to pass water, since three quarters of the potential capacity of the bladder is occupied by the thirty ounces of residual urine.

As a rule, you will find that the patient complains of neither pain nor hæmorrhage, unless there is some complication present. The complications which produce pain are cystitis and stone in the bladder; those which may produce hæmorrhage are congestion and inflammation of the prostate, stone in the bladder, and perhaps acute cystitis. Any pain that the patient may feel in a simple uncomplicated case of prostatic enlargement is usually slight, and is felt before micturition, and as the bladder tries to empty itself and partly succeeds, the pain correspondingly diminishes. The fact is that such pain is dependent upon over-distension of the bladder, and as this is relieved the pain disappears. If a patient tells you that when he has passed as much water as he can he gets an extremely smart pain at the neck of the bladder, which lasts a few minutes, it is probable that he has got a stone complicating the enlargement of the prostate.

Next with regard to the state of the urine. You may find that the patient's urine is perfectly normal, not only in quantity, but in appearance, or that he is passing a large quantity of urine of low specific

gravity, with perhaps a trace of albumen, such a condition being indicative of chronic renal mischief. Again the urine may be turbid and very offensive, perhaps containing large quantities of ropy mucus, due to the presence of chronic cystitis which he has contracted in the course of the disease.

The amount of residual urine present varies according to circumstances; there may be only two or three ounces, or as many pints. The quantity will depend on how soon he comes for treatment.

There are certain general effects of enlarged prostate that are dependent upon the broken health into which these patients fall, due to the discomfort and annoyance the frequency of micturition causes them, especially at night, so that they do not get a good night's sleep. The consequence is that these patients have frequently a tired, anxious, worn look, which is more marked if the case is complicated by cystitis or stone, and still more so if the kidneys are diseased. It is noticeable also that the subjects of enlarged prostate frequently have perverted moral sense; they sometimes are very erotic; not uncommonly they commit acts which are offensive to public decency, and even occasionally get themselves into criminal trouble from this perverted sexual sense.

Now with regard to the physical examination of a case of prostatic enlargement. If a man comes to you complaining of the symptoms I have indicated, the first thing you should do is to examine the abdomen, with the view of finding by percussion and palpation if his bladder extends above the pubes, and if so to what extent. By this means you may make a somewhat rough estimate as to the quantity of residual urine present. If you find that the bladder is pretty much distended and rises high above the pubes, you should be exceedingly careful how you at first set to work to empty that bladder, because it has been known that a patient with a large quantity of residual urine, which has probably been present for months, has died of syncope in consequence of that urine being suddenly drawn off. Having examined the abdomen and come to some conclusion about the condition of his bladder, the next step is to tell him to pass water. You must bear in mind that some people cannot pass water while you are looking at them, so if you find a man hesitates about it, leave him alone for a few minutes, and tell him to pass as

much as he possibly can; but if possible, watch him micturate. You will find that in so doing he assumes a perfectly characteristic attitude, and the stream is characteristic. From this alone it is possible to make a certain diagnosis. He slightly bends his whole body forwards and stoops, thereby relaxing the abdominal muscles; very likely he also slightly bends his hips and knees. This is in marked contrast to what occurs in stricture. A patient with stricture holds himself fairly upright, closes his mouth, holds his breath, and strains with his abdominal muscles, to force the urine past the obstruction; any force in the case of prostatic enlargement only makes matters worse by forcing the middle lobe against the neck of the bladder. When the urine does come (and very likely the man will have to wait a minute or two before it does), it is in a small absolutely forceless stream, that will pulsate with respiration. Indeed, it is hardly worth calling a stream at all, one might more correctly say that the urine just dribbles away from the overdistended and atonied bladder. You will notice that if the man attempts to strain in the very least, the forceless stream is either rendered very much smaller or is completely stopped for the time being, but if he stops straining it will gradually flow on again. Owing to the fact that the stream is so small and forceless it follows that to get rid of a definite quantity of urine the patient must take a very much longer time than would be occupied in so doing under normal conditions.

Now when you have seen the man pass his water, you should then suggest to him that you pass a catheter. The probability will be that he will object; he will tell you that he does not want a catheter passed; that he has come to see you not because he has got a stricture, but because, on the other hand, he is passing his water too frequently and passing too much of it; in fact, he tells you it is not "retention" from which he is suffering, but "incontinence." You must explain to him that in the great bulk of instances frequency of micturition and presumed incontinence of urine mean retention; that the frequency is dependent upon over-distension of the bladder; and it is the effort of Nature to get rid of the urine as fast as she can, although she cannot empty the bladder. After a little judicious persuasion and explanation of the circumstances of the case, the patient will usually

begin to see the matter in your light, and will consent to let you pass a catheter. In this you must be very careful what you are doing. In my opinion it is unwise to pass the catheter in your consulting room; it is better to tell the patient to go home, and that you will see him there later, when he is comfortably housed in his own place. As a matter of fact, you never know how the passage of the catheter is going to affect the patient, and this is especially true in the case of those who have been suffering a long time from retention of urine, who are advanced in life, and whose kidneys are, probably, not as good as they ought to be. Of course, circumstances may arise in which this precaution cannot be adopted. You should be careful that, in exposing the patient, you do not let him get cold. If there is a large quantity of residual urine, you must only draw off sufficient to prove to the patient that you are right in your view, and thus gain his confidence. If he has six or eight ounces only, you may completely empty the bladder; and you may then still further enhance your reputation in the patient's mind by making a simple calculation. Supposing you find that he is passing, on an average, five ounces of urine at each act of micturition, which is repeated every two hours; that will be fifteen ounces in six hours. Now, he has already passed five ounces before you; you have drawn off ten—totalling fifteen ounces. That man will not want to micturate again until he has secreted about another fifteen ounces, and you know he is secreting at the rate of five ounces in two hours. You tell him that he will not want to pass any water for about five or six hours. He will go away doubting you, but he will find you to be quite right.

Finally, in completing the diagnosis, you examine *per rectum*. Bear in mind that there is a source of fallacy, and it is this: you may find a prostate that, to your sense of touch, is, perhaps, very little enlarged, and yet the man may have most severe symptoms of obstruction, perhaps complicated by acute cystitis, or even stone. Do not let that take you in, and cause you to believe that the condition you have to deal with is dependent upon some other cause, because I have told you it is the *position* of the enlargement of the prostate which is important. On the other hand, you may, on making a rectal examination, find the patient has a prostate half as big as an orange or larger, which,

however, is producing no symptoms at all. All that it means is that the enlargement affects the lateral lobes and not the middle; it is the middle lobe which does the mischief.

Having determined the fact of enlargement, the next point in your diagnosis is to discover the quantity of residual urine which the patient has got; but, if this is large, you should not at the first draw it all off, but must draw more and more each day until the bladder is emptied. Next you must try and find out what is the condition of the man's kidneys. I need hardly point out to you that all the facts which you need to find out in cases of enlarged prostate cannot be obtained at one interview; as I shall tell you at my next lecture, when I speak of the treatment of enlarged prostate, you will have to keep the patient confined to the house for ten days or perhaps a fortnight, and during that time you will be able to form an approximate estimate as to the condition of his kidneys, and the work that they are doing. I may say provisionally here that the mere fact that a patient knows he is passing 50 or 60 ounces of urine in 24 hours does not show that his kidneys are doing their work. It is true that it is the duty of the kidneys to get rid of a certain amount of fluid, but a still more important one is to excrete certain waste products of metabolism, notably urea; the water is a convenient means of holding these substances in solution and getting rid of them. Therefore the main point you have to determine in urinary disease, where you have to submit the patient to long catheterism, is what amount of urea is being excreted. You must not only know that, but you must estimate also the quantity of urine which is passing, and the quantity of albumin. The renal function must be continually watched to see if it is improving under treatment, or whether it is progressively getting worse, for by such observation much valuable information is obtainable from the point of view of prognosis and treatment.

(To be continued.)

Recurring Purpura and Anal Hæmorrhage.

—The 'Bulletin Méd.' of January 4th relates the case of a man of 46 years who has a hæmorrhage from the anus recurring regularly every month, simulating the menses, accompanied by patches of purpura.

Journ. Amer. Med. Assoc., March 11th, 1899.

MEETING OF THE SOCIETY OF ANÆSTHETISTS,

At 20 Hanover Square, February 17th, 1899;

The President, Dr. DUDLEY BUXTON,
in the Chair.

Mr. McCARDIE read the following paper :

On the Prolongation of Nitrous Oxide Anæsthesia in Dental Operations by means of a Mouth-tube.

In what follows I shall freely quote from what I alone, and in collaboration, have previously written on the subject.

If we can satisfactorily lengthen the usual period of anæsthesia obtained by the administration of nitrous oxide, the use of chloroform, that drug so dangerous in dental surgery, can be minimised, and ether, with its inconveniences and awkward after-effects, need very seldom be given. Years ago, I believe, the late Mr. Clover tried to prolong nitrous oxide anæsthesia by running a stream of gas into the mouth, but his attempts appear to have been unsuccessful. At a meeting of this Society, held on March 17th of last year, Mr. S. A. T. Coxon, a dental surgeon practising at Wisbech, described his method of continuing the administration by means of a mouth-tube during the performance of the operation, and gave records of some of his cases. His apparatus consisted essentially of a curved metal tube (in diameter about half an inch), which was made to fit in place of the mouth-piece. The gas was warmed before being utilised by passage through a metal coil lying in a hot-water chamber. When the patient was fully anæsthetised, Mr. Coxon rapidly changed the face-piece for the "injector," which he passed into the mouth nearly as far as the uvula, and then kept up a steady stream of gas into the pharynx from a well-filled gas-bag. The tube was useful, in addition, as a tongue depressor.

I was much interested in the demonstration of his apparatus by the inventor, and, when I returned home, had a tube very like Mr. Coxon's made for me. It was half an inch in diameter, and boldly curved at the free end, forming about a quarter of a circle, whose diameter would be nearly three inches. I did not use any means for warming the gas, thinking it to be unnecessary. At the Dental

Hospital, Birmingham, with the assistance of Mr. Thompson Madin, one of the dental surgeons, I used Mr. Coxon's method in many cases needing prolonged anæsthesia. Mr. Madin changed the face-piece for the tube, and directed the latter while I attended to the gas supply, condition of the patient, and managed the gag. I may here recapitulate what I consider to be some of the important points in Mr. Coxon's original method :

1. The tube should be fully half an inch in diameter, so that a full stream of gas can be passed.

2. The gas-bag should be kept fully distended throughout, and may be compressed to increase the delivery pressure.

3. The end of the tube, which lies on the tongue, should be placed well to the back of the throat. The curved part should lie horizontally, so that the stream of gas is directed towards the side wall of the pharynx, and may not blow blood down towards the air passages.

4. Nitrous oxide should not be administered through the tube until the natural colour has partially returned to the patient's face, and then such a continuous stream of gas should be kept up as to maintain a moderate blueness of the lips and face throughout the operation.

5. It is very advantageous that where possible the tube be directed by an assistant, who can also swab out the mouth if necessary, while the anæsthetist compresses the nares or adjusts the clip, regulates the gas supply, manipulates the Mason's gag, commands the head and jaw, and concerns himself with the state of the patient.

The ordinary, almost vertical, position in the dental chair is used. The curved part of the tube lying on the back of the tongue tends to prevent the escape of blood or foreign bodies backwards into the pharynx. If the mouth prop slips, the calibre of the tube is sufficiently great to allow of the easy introduction of the beaks of a Mason's gag. It might be thought that the presence of the tube in the mouth would be embarrassing to the operator, and it certainly is so until he becomes used to it.

Mr. Coxon's method is extremely simple, ingenious and adaptable. It involves, of necessity, no cumbrous or delicate apparatus, merely a short metal tube being needed.

I used this method in a good many cases at the

Birmingham Dental Hospital, and formed a high idea of its merits. The great drawback to its use was the large quantity of nitrous oxide used in very prolonged administrations. However, by employing it I avoided, several times, the otherwise necessary inhalation of ether.

The procedure just set forth, and that modification of it about to be described, may, I think, with some propriety, be called "The Oral Method of Prolonging Nitrous Oxide Anæsthesia in Dental Operations."

Now I will describe a variation of Mr. Coxon's original method, and one which I, and others in Birmingham, used with marked and increasing success during a large part of last year.

A very few months after I had begun to use Mr. Coxon's method, Mr. Thompson Madin, whom I have before mentioned, bethought himself of the expedient of manually closing the nares. He also proposed to pass the end of the tube only a short way within the mouth, just at its corner, and to merely keep it full of gas. (Mr. Coxon tells me that since his original paper he has independently adopted the same idea.) Mr. Madin himself tried the experiment with immediate success.

I suggested—and had made for me—a nasal clamp to compress the nares, thereby leaving the hand at liberty. It was rather a difficult matter to obtain a clamp which would fit and not slip off from some or other of the large variety of noses met with, and ultimately I had a paper holder with a strong spring cut down, and found it very good for my purpose.

I may here mention that, after removal of the face piece in an ordinary administration, I always close the nostrils by finger compression, and find that it very appreciably lengthens the duration of anæsthesia. Again, one can obtain fair command of the head in most cases by a firm grip of the nose.

If the nasal passages be left open during the administration of nitrous oxide by the mouth tube, I think that there is a tendency for the patient, even though he be a mouth breather, to breathe through the nose, owing perhaps to reflex effects arising from the interference in the mouth, and the gas poured into the pharynx is of course very largely diluted with the air inhaled through the nose. During the pause between expiration

and inspiration there is obviously a very free vent for the escape of the gas.

Supposing the nostrils to be closed and the mouth and naso-pharynx to be kept filled with gas, pure nitrous oxide is inhaled straight to the lungs at the very commencement of inspiration, the naso-pharynx acting as a kind of supplementary reservoir; then follows an admixture of gas with a comparatively moderate amount of air, moderate for the reason that the stream of heavier nitrous oxide tends to displace the lighter air. Hence by compression of the nostrils far less gas is needed to maintain anæsthesia, and one can much more easily than in Mr. Coxon's original method regulate the depth of that anæsthesia.

The extraction of many teeth is of itself a rather serious operation, and may be followed by a good deal of shock, and where possible there should be forethought in the preparation of the patient. He should be given instruction as to regulation of his food, bowels, and bladder, and where any serious lesion is suspected he should be methodically examined. It is well, also, to ask him to blow his nose immediately before the inhalation. There should be a plentiful supply of gas in the cylinders, say one hundred gallons or thereabouts, controllable, preferably by the foot of the anæsthetist.

An assistant skilled in the use of the tube, and accustomed to work with the anæsthetist and operator, makes success assured in most cases. Since in any major operation in general surgery there are usually one or more qualified assistants at hand, it is surely not unreasonable to require that in a long dental operation, conducted under the influence of nitrous oxide administered by this method, the anæsthetist should, whenever possible, have an efficient helper.

A central gag, *e.g.* Brunton's, should be used wherever it is possible, and often it needs not to be changed; but if this be necessary, Mason's instrument may be easily inserted later in the operation.

Coming now to the actual administration—nitrous oxide is administered in the usual way to the full anæsthetic degree. Then the face-piece of Hewitt's or Barth's apparatus is rapidly replaced, preferably by the assistant, for the tube, the patient's nose is quickly compressed either by the fingers or clamp, and the mouth tube is in-

serted without loss of time into the corner of the mouth, or any available space away from the operator's hand, and a moderately full stream of gas is allowed to flow into the mouth, continually or intermittently as may be necessary. If the operation be on the lower teeth, it is often most convenient to raise the bag and pass the tube into the mouth from above. Otherwise the tube is passed from below or from the side.

As a general rule, when I have an assistant, I prefer holding instead of clamping the nose. I find that the best guide as to the depth of anæsthesia is the colour, and I try to keep the patient's lips of a slightly bluish colour in most cases. Respiration should be carefully watched; it is generally either quiet or moderately snoring.

Dexterous co-operation of operator and anæsthetist or assistant will soon overcome any difficulties of inserting or changing the position of the tube, which may be used to compress a sponge against the alveoli on one side of the mouth while the operation is proceeding on the other.

In the case of students who are operating, or of colleagues not accustomed to work together, success is not always at first attained, because, as in other things, practice is needed.

In Hewitt's stop-cock, and Barth's also, the metal tube of the shape used by Mr. Coxon is, when fixed, at right angles to the axis of the gas-bag, and therefore rather awkward to manage; so I have had a tube made right-angled at its connecting end, in order that its main axis should correspond with that of the delivery system. This arrangement conduces to keep the gas-bag, which in Hewitt's apparatus is close to the stop-cock, out of the way, and generally adds to convenience in manipulation.

For a short prolongation of the ordinary period of anæsthesia assistance is not always necessary, and with the help of a good nose-clip one can generally manage unaided; but it is indeed of great advantage to have an assistant if the operation is likely to last more than one or two minutes, and one who is thoroughly competent is almost essential.

Recovery from anæsthesia is rapid, and not marked, as a rule, by any after-effects ascribable to the anæsthetic much more severe than those following an ordinary inhalation, and most of the patients have left the hospital within half an hour. I have

not seen any dangerous symptoms during anæsthesia, or even such disagreeable events as defæcation or micturition, although practically none of the patients has been in any way prepared. There is sometimes a tendency to phonation or shouting, which occurs generally when the patient is about to recover colour. If the phonation be not loud, it is of no moment; but if it turn to shouting, the operation must be interrupted till the patient can be more deeply anæsthetised, when it will gradually pass off.

Occasionally, and especially in those cases in which a central gag has been used and the mouth kept widely opened, is seen the condition mentioned in Dr. Silk's book on 'Nitrous Oxide Anæsthesia,' and known as "subluxation of the inferior maxilla."

The after-effects which are chiefly of concern are those resulting from shock due to the length and severity of the operation, and not to the anæsthetic, except in so far as the rapidity of recovery indirectly helps to that result. People who have been much upset by dental troubles, who have had much pain and little sleep for days, or perhaps weeks, are particularly prone to shock, and not seldom complain of frontal headache after operation. Occasionally, too, there is a feeling of faintness for a short time afterwards.

The prolongation of nitrous oxide anæsthesia is of especial advantage in the case of very nervous people who have a horror of an anæsthetic, and particularly dread a second visit for extraction; and in these cases, too, the second administration is, according to my experience, likely to be more troublesome—more severe as regards after effects, and to result in a shorter anæsthesia than did the first.

There is, as compared with ether and chloroform, the exceeding great advantage of being able to almost exactly regulate the duration of anæsthesia.

The advantages of the method I am advocating are many and great. The ability, by very simple means, to prolong nitrous oxide anæsthesia as soon as it is seen to be necessary, say in a case where the extractions are found to be more difficult than was at first supposed, is an immense boon. There is no display or previous arrangement of extra apparatus which may alarm the patient, a point by no means unimportant.

The means needed to obtain the desired end are inexpensive, and consist merely of the metal tube and a nose clip, which cost only a few shillings.

The method is extremely simple in practice, and adaptable; a tube can easily be made to fit any ordinary gas apparatus. There is practically nothing extra to carry about, beyond, perhaps, a spare cylinder.

The cost of gas is the principal factor for consideration, and cannot be overlooked, but the quantity used in the modified method is much less than in Mr. Coxon's original one.

In a demonstration which I gave at the Annual Meeting of the Central Counties Branch of the British Dental Association, held at Worcester in last July, the duration of anæsthesia was three minutes, and the quantity of gas used about forty-eight gallons. Perhaps, on a rough average, 100 gallons of gas would be used in the administration lasting ten minutes.

At the Birmingham Dental Hospital the method is growing in favour, and is being often tried by my colleagues. I only once last year gave ether there, and that was before I knew much about the subject of which I am speaking. Now, I should only in an extremely exceptional case, or, perhaps, not at all, give ether, but should prolong anæsthesia by the tube. As a partial consequence of many successful cases the number of ether and chloroform cases at the hospital fell from thirty-one in 1897 to thirteen in 1898, though the patients come from a very wide area and often need many extractions at one sitting. I have used the method in over 100 cases. The failures have not been more than one or two in ten, and occurred naturally more frequently in the earlier trials. Others without previous experience who have tried the method have often failed, and it is essential for success that anæsthetist, operator, and assistant should be accustomed to work together.

The PRESIDENT said the matter dealt with by Mr. McCardie had come before the Society previously, but, he thought, never in quite the same way as it had been presented that evening. His own experience of the matter had been only of Mr. Coxon's method, and as he had not had the advantage of seeing Mr. McCardie at work, perhaps such an experience would not justify him in making any strictures on what had fallen from

Mr. McCardie. He thought the large quantity of gas consumed was a disadvantage in the method. Mr. McCardie had spoken very wisely when he said—and he thought all present would corroborate that statement—that it was inadvisable to give very prolonged anæsthesia for dental operations. There was sometimes a tendency to attempt the removal of too many teeth at one sitting. He was quite sure that multiple extractions were fraught with a good deal of shock, and an amount of shock which his dental friends would agree was most undesirable, and certainly not to be encouraged. With regard to the occlusion of the nares, that plan had been used for many years in America, and was employed in the early days of nitrous oxide administration in this country. It was well known to all of them who had not forgotten their physiology that most persons breathe almost entirely through the nose, with the exception of those pathological people known as “mouth-breathers;” and by closing the nostrils they certainly were halving—if not more than halving—the area of the ordinary air-ways of the individual, and so undoubtedly materially diminishing the ventilation of the lungs. Probably, if they were to close the nostrils after an ordinary administration of gas, they would notice a very distinctly prolonged period of anæsthesia due to the incarceration of gas in the lungs. He had no doubt that some of the prolongation by the method just explained was due to that cause. Another point often noticed was that in an extraction from the lower jaw where the tongue was forced back, and where the incarceration of gas was affected by the occlusion or partial occlusion of the glottis, a prolonged anæsthesia resulted, and this further demonstrated that anæsthesia was prolonged by the non-escape of gas from the lungs. In passing, one might refer to the remark made by Mr. McCardie that a person who came the second time to the dentist was not so willing to take gas as on the first occasion, his own experience was diametrically opposite to that. He would say they came with trepidation the first time, as to a land which they wotted not of, to face terrors which loomed large in the imagination, whereas the second time they came as to a friend, welcoming the nitrous oxide as a protection from pain.

Mr. WILLETT said he had not much to say about the particular method as he had not seen it. He

was always glad to hear an accurate account of any new kind of anæsthesia, but it seemed to him that the gas and oxygen introduced by Dr. Hewitt was so much superior to ordinary gas for dental cases, that this new method of Mr. McCardie's was not so important as it would have been if they had not Dr. Hewitt's method. Nobody, he thought, was able to judge of the two methods who had not tried them both. Gas and oxygen was so much more easily given, and with so much less expenditure of gas compared with Mr. McCardie's, that he personally did not feel inclined to even attempt Mr. McCardie's system of giving 175 gallons of nitrous oxide. He did not know how it could possibly be carried about or managed in private practice; in hospital it was, of course, another thing. The ordinary 25- or 50-gallon bottle was heavy enough, but how to carry 200-gallon cylinders passed his comprehension. By Dr. Hewitt's method seventy-five seconds anæsthesia could be given quite easily, and he had only recently seen six bad teeth taken out from one side under its administration, and that was generally enough for any one patient on one day. Of course there were exceptional cases in which thirty-two or twenty-six teeth were removed, but surely they were quite exceptional.

Mr. CARTER BRAINE said, since Mr. Coxon read his paper before the Society, he (Mr. Braine) had been working in very much the same grooves as Mr. McCardie, and he had saved a great many patients from inhaling ether. He thought this method of prolonging anæsthesia with nitrous oxide should only be used in those cases where it was desirable to avoid giving ether. He agreed with Mr. Willett that the anæsthesia produced by nitrous oxide and oxygen was better than that produced by nitrous oxide alone, but they could not get an anæsthesia of two, three, or more minutes with nitrous oxide and oxygen. In looking at the tube passed round by Mr. McCardie, he noticed that the one he (Mr. Braine) had had made at random was twice the size. He was astonished to hear Mr. McCardie say that during administration he kept the patient's mucous membranes more or less cyanosed. What he had been particularly struck with in his cases was the absence of cyanosis where he had used the tube. With regard to clipping the nose, he very soon found out that by pressing the nares with his

fingers he very much diminished the quantity of nitrous oxide used. He could not say he had ever had the luxury of an assistant at these operations, he had had to manage entirely by himself; in fact, he did not think they would find in private practice an assistant ready to help them. With regard to the situation of the prop, in the first few instances he had had a central prop in the front teeth, but he very soon discarded that method, and put a small prop far back on the same side as that on which he administered through the tube, it was then very much less in the way of the extractor. The tube could be very readily turned over to the other side if necessary. The greatest number of teeth extracted under that method of anæsthesia was eighteen, and the consumption of gas was twenty-five gallons, using it by very much the same method as in a Junker, that is to say, pumping when inspiration occurred. The only drawback of the method was, he believed, the risk of stumps of teeth falling back into the pharynx and becoming impacted, especially with young extractors, who were very eager to get teeth out rapidly. If plenty of time were taken, and each tooth seen clear of the mouth, he thought the risk would be very greatly diminished. He had administered the anæsthetic by this method in 100 cases, and had been very pleased with the results.

Mr. McCARDIE, in reply, said Mr. Braine had confirmed a good deal of what he had read in his paper. With regard to Mr. Willett's remark as to the superiority of gas and oxygen, that certainly could not be disputed, he often himself gave gas and oxygen, and he only used the method he had described in cases where there were very many and difficult teeth to be extracted, that is to say in those cases where otherwise he would have had to give ether. He had never been able to get an anæsthesia of seventy-five to eighty seconds with gas and oxygen, and he did not think one could often count on it; but by his method one could rely upon a good anæsthesia for a minute at any rate. Compression of the nares was a most important matter. He thought it tended to diminish the total breathing of the patient besides lessening the quantity of gas which need be used. Often, if the patient were breathing strongly, he would go so far as to depress the chin so as to help the incarceration of gas in the lungs. Regarding cyanosis he

always endeavoured to maintain deep anæsthesia; he did not believe in running too near the mark, in which case the patient would come too rapidly round. He watched the lips especially as a guide in that matter. He had not tried turning on the gas during inspiration to any extent; once or twice he tried it, but it did not succeed; he therefore kept up a steady stream of gas and tried to keep the mouth filled with gas all the time, so that when the inspiration occurred, pure nitrous oxide would get into the lungs, and later there would be a mixture of nitrous oxide and air. At a Meeting of the Society (last March) he remembered that the President, Dr. Buxton, remarked on the shock of multiple extractions, which, he said, were fraught with danger. He, Mr. McCardie, thought it was far better to get the operation done at one sitting if possible; he did not believe there was much more shock from having twelve teeth out than from six, nor was the bleeding likely to be proportionately greater. They had to consider the comfort of the patient, who, in many cases, would have to come a distance of some miles on two or three occasions to have the operation completed. Practically he found patients objected to that most strongly, and were only too glad if the operator could promise that the whole thing should be over in one sitting. He advocated careful preparation of the patient before operation, and thought it was well to try and get them to lie down for half an hour or an hour after operation, so as to fully recover. He was interested to hear about occlusion of the nose practised in America, and would be glad to know how it was done?

The PRESIDENT: Between the lips is inserted a tube, and the lips are closed over it, and at the same time the nares are pinched with the fingers. This is to obviate the necessity of having a face-piece.

Mr. MCCARDIE, continuing, said it was against his experience for patients not to fear the second inhalation at all. Many of his patients, who had to come a second time, asked for local instead of general anæsthesia.

Crying in the New-born, by S. Southworth, M.D.—A primipara was delivered of a strong male child, and as the mother's breasts were well developed and the child strong, instructions were given that the child be put to the breast every six

hours for the first day, and every four hours the second day, and that boiled water, sweetened with sugar, be given every two hours between the nursings. It subsequently transpired, that as the child slept soundly between the nursings, the latter part of the order was disregarded entirely or the water given but a few times. At noon on the third day word was received that the child was sick, and on arrival at the house the nurse said that it had cried almost constantly for the past eighteen hours, as if suffering from colic, sleeping but little and passing no urine since the early hours of the morning. When the child was undressed marked priapism was noticeable, and the placing of the somewhat cool hand over the region of the bladder was followed by the discharge of some four to six drachms of urine of a turbid brown colour. This left a yellowish-brown discoloration upon the napkin with no traces of pink. Priapism immediately ceased, and the child who had before been crying steadily fell into so sound a sleep that the subsequent examination did not awaken him. The so-called uric acid infarctions of Virchow formed by the deposits of uric acid and urates in the straight tubules and papillæ of the kidneys in new-born infants have been recognised for some time as a possible source of irritation as they remain *in situ*, or are washed out by the scanty secretion of urine. It is extremely probable that much of the supposed pain of colic in the new-born is due to these sources of irritation in the kidney, ureters, bladder, or urethra. Boiled water, given at regular intervals, will dilute the urine, and prevent or alleviate the discomfort. The rather unusual opportunity of observing matters in this case, together with the subsequent course of events, point clearly to the urine as the cause of the crying, whereas, had no such observation been made, hunger and inanition might readily have been considered a sufficient explanation of the symptoms.—*Archives of Pediatrics*, March, 1899.

Diabetic Coma.—Some of the suggestions made by Dr. Robin ('Bull. gén. de Thérap.,' vol. cxxxvi, p. 353) to ward off coma are: to stop strict *régime*; milk diet; sodium sulphate to increase elimination; five drachms of sodium bicarbonate daily; infusion of digitalis and ergotin if the pulse is small, rapid, or irregular; caffeine citrate and theobromine if slow, soft, and compressible; strychnine before and pepsin and maltine after the milk; two injections daily of a 25 per cent. solution of sodium glycono-phosphate; friction; oxygen.

Medical Record, March 11th, 1899.

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A CLINICAL LECTURE

Delivered at the National Hospital for the Paralysed and Epileptic, Queen Square, London, January 31st, 1899,

By Dr. ORMEROD.

GENTLEMEN,—I propose to-day to take for consideration the symptom of ataxy. This is a symptom which we easily recognise in practice. We all pretty well know what it means, but it is rather difficult to get an exact definition of it, and I do not profess to give one. It is, in the long run, a motor symptom, that is to say, it is concerned with the action of muscles, either in moving about or in moving the limbs, or in the action which is necessary for balancing. It is contrasted with paralysis or paresis, and implies that the power of individual muscles to contract is not lost, but that their power of acting in harmony is lost; in other words, there is an inco-ordination in the muscular action. For nearly all movements, even the simplest, are produced not by any one muscle, but by the resultant action of more than one, and some movements are very complicated indeed, and require the precise co-operation of many muscles, co-operation in force, in time, and in duration of contraction. If the muscles concerned do not properly co-operate, the action becomes wild and disorganised, and that disorganisation is known as ataxy. What the reason of that symptom is, I mean what particular lesion or lesions give rise to it, is very hard to say. There has been a great deal of discussion on that point. Of course the co-ordination or harmony of action in our muscles must be brought about somewhere in the nervous centres. In most cases, at any rate, where the muscular actions have become habitual, the process is an automatic one, or even a reflex one. But it does not follow that the centres are always involved in cases of ataxia. In fact, we know that in some cases of peripheral nerve disease there is ataxy. It appears either that the centre may be out of gear, and so pro-

duce this inco-ordination of the action of muscles, or the messages going up from the periphery towards the centre may be wrong, or the messages coming down from the centre to the muscles may be wrong. Ataxy may therefore result either from disease of efferent fibres, or disease of afferent fibres, or disease of the centres. Again, it may result from impairment of function in the muscles themselves, which prevents them responding in proper time or measure to the stimuli from the centres. You will thus see that it is a symptom which may occur in a great variety of nervous diseases.

However that may be, the disease in which it most constantly occurs, and in which it is best marked, is the disease known as *tabes dorsalis*, or as *locomotor ataxy*. In this disease the symptom nearly always manifests itself first in impairment of function of the lower limbs. Now there is one very good reason for that, namely, that the lesion which produces the disease begins in the lower part of the spinal cord; that is probably the most potent reason. But there is also a reason which is not quite so obvious. You might think that the movements of the lower limbs in standing and walking are a matter of very much less precision than the movements of the upper limbs; the latter certainly seem much more fine and precise. But I do not know that this is entirely true, because in standing and walking it is not merely the muscles of the legs which are brought into action, but the muscles of the body and trunk; that is to say, it is not merely a matter of walking, but of retaining the balance while you do so, and for that a large number of muscles have to act together with considerable precision. For that reason and for the other which I have mentioned, in *tabes* the inco-ordination is nearly always manifested first in the gait. As a matter of fact the symptom is of very gradual onset. There is first of all a stage in which there is no ataxy at all. Then there comes a stage in which the patient begins to be a little unsteady, particularly in walking up or down stairs, and in turning round quickly, or in "walking to order." The unsteadiness is always increased in intensity by the patient trying to walk in the dark or with his eyes shut. It is not quite obvious why this should be so, but it makes the test much more complete if you ask the patient to try and walk or stand with his eyes shut. The next thing the

patient with ataxy does is to take a broader base to walk upon, that is to say, he spreads his feet apart; and further, he begins to look at his feet while walking, and in consequence he adopts a forward stoop, partly to maintain his balance, and partly so that he may see his feet. As the disease advances, there comes a stage in which he manages his feet in an awkward way, he lifts his feet high, and brings them down to the ground with a stamp. Thus in the most exaggerated stage of the ataxic walk the patient keeps his feet far apart and looks at them, leaning forward; he lifts his feet high, and brings them down forcibly upon the heel. This is a marked contrast to the way in which a paraplegic patient drags his toes along the ground. But you cannot see all these things at every stage of the disease. Flourishing of the feet is rather a late symptom, and consequently less common than mere unsteadiness.

I want first of all to show you various cases of *tabes* illustrating as far as I can the several stages of ataxia. I do not know that I shall be able to do it very perfectly, but I may be able to show you some patients who have but little ataxy, and others who have very much.

I will first show you a patient who is hardly ataxic at all, indeed this man may very fairly be called "non-ataxic" so far, or a subject of *tabes* in the "præ-ataxic stage." But when he first gets up, and when he tries to turn round, there is some unsteadiness, and the right foot is moved oddly. When we narrow the base by getting him to put his feet close together he finds a difficulty in balancing himself. When I tell him to shut his eyes he only sways a little. It was not for ataxy that he came to the hospital, but on account of loss of sight. This particular patient illustrates what we often see here, namely, that a man goes to an ophthalmic hospital, and is found to have optic atrophy with certain other symptoms, and he is accordingly sent here as a case of *tabes*. This is one way in which *tabes* sometimes begins. You may say, if a man is not ataxic what right have you to diagnose this disease. Well, you may have to do that long before he becomes ataxic, and it is diagnosed mainly by three symptoms: one is that for which the patient often comes, namely, rheumatic or lightning pains; the second is the absence of knee-jerk; and the third is the pupil, which as a rule is small and does not contract to

light, but does contract to accommodation. When these three symptoms are present the case is almost certainly tabes. This is proved by the ultimate progress, and partly by the fact that such early cases have been examined and found to have posterior sclerosis. This particular patient has got the three symptoms; he has had occasional pains in his legs like rheumatism, his knee-jerk is absent, and his pupils do not contract to light. He has also the symptom which is fairly common in early tabes, namely, commencing optic atrophy. That is commonly said to have a certain curious relation to the ataxy. It is said that when a case of tabes commences by optic atrophy, either the ataxic symptoms do not come on at all, or they come on only very late in the disease, and are then of less severity than in other cases. No doubt this dictum sometimes holds good; let us hope it will do so here. There is nothing further about this man that need detain us, except that he denies the usual antecedent of tabes, namely, syphilis. He is a coachman, and has had a good many accidents; he has been pitched on to his head more than once. He has had another symptom which frequently occurs in early tabes, namely, slight diplopia, which lasted for two or three months.

The next patient is a man who is an in-patient in the hospital, and he is, or has been, more ataxic than the last. However, he has been in for a little time and has improved. The walk is fairly characteristic of slight ataxia. You will notice that he is careful to look at his feet while walking, and that he takes a very broad base. When he puts his feet together and shuts his eyes you will notice a tendency to fall, that is to say, he has "Romberg's sign." There is no actual loss of power in the legs. The handwriting is altered, but I do not think his hands are distinctly ataxic. His knee-jerk is absent, and his pupils characteristic.

One important feature in connection with ataxy is that sensory alteration very often goes with it. It is common to find that in a patient who is markedly ataxic there is some distinct affection of sensation. It is not always the sensation to touch; it may be that the sense of pain or the muscular sense is altered. This patient can feel an ordinary touch, but he cannot feel a pin-prick so well as he could a little time ago. Formerly it took him a little time to feel a pin-prick; on test-

ing now in order to show you this delayed sensation I find he has gone too far for that, as he cannot feel the prick at all. The way in which this patient discovered he was ataxic is one often mentioned, namely, that when he covered his face with his hands and the towel in the act of washing, he fell down. The legs are ataxic for other actions than that of walking, for he cannot accurately touch one knee with the heel of the other leg. The whole duration of his disease is about two years, and he says he has suffered from rheumatic pains for about that period. There is also a certain amount of difficulty in micturition; he has what is sometimes called precipitancy in micturition, that is to say, he has to hurry to pass his water, otherwise it would take him by surprise.

The next case is a female patient. She was also an in-patient. When she came in she was very ataxic and could not walk without help, but she improved very much. She can now walk unaided. But you will see that in getting along she throws her feet about, and walks on a wide base. You will also notice the peculiar way in which she brings her feet to the ground, and she turns with difficulty. Romberg's symptom is well marked in her. She had no loss of sensation in the sense of not being able to feel a touch or a prick, but she had very marked loss of sense of position in her lower limbs. She could not tell in what way her legs were placed unless she looked at them, but she has improved in that respect, which I think is rather unusual. She did show in a very marked way the symptom which has been comparatively recently described in connection with tabes, namely, hypotonia in her muscles. It is rather an interesting symptom because it has been advanced as one explanation of the ataxy. Muscles normally have a certain tonus, *i. e.* they are in a certain state of constant contraction; but in many cases of tabes it is found that they are habitually relaxed, and consequently one may bend and flex the joints to an extreme degree without distressing the patient. It is thought by some that this hypotonia of the muscles gives rise to the ataxy in the way I mentioned at the beginning of the lecture, namely, that the muscles which are lax do not contract so promptly and efficiently as their opponents, and therefore movement is rendered wild. I think while she was in the hospital this symptom also improved. When she was in bed

she used to be able to put her head right down between her knees, because the back muscles and the glutei did not offer that resistance to extreme flexion which they normally do. The disease in this patient came on gradually during the last four years, commencing with pains and difficulty in walking; there are no other symptoms except the usual ones of absent knee-jerk, Argyll Robertson pupils, and ataxy. The only peculiarity is that the patient is a female. The disease is much more common in males, the proportion being about one female to ten males. There is no direct history of syphilis in this case, but there is a probability of it. Her first child was normal, but her third, fourth, and fifth were all born dead. It seems highly probable that she contracted syphilis before the birth of the second child.

The next patient is a man under the care of Sir William Gowers, who has kindly allowed me to show him. He is in a very advanced stage of ataxy, more so than any case we have seen yet. He cannot walk without assistance, and when he does he takes a very wide base, and his feet come down with very considerable force. The walk is a typically ataxic one. He presents other interesting symptoms such as disease of a joint, a perforating ulcer of the foot, &c., which possibly Sir William Gowers may describe at a future date.

This man, under the care of Dr. Bastian, is another case of marked ataxy. He is a sailor, aged thirty-three—rather young to be in this advanced stage of the disease. He has had the disease for about four years. He says it began with a difficulty in walking; he can now hardly stand without assistance. Like the other man, he has considerable anæsthesia from the knee downwards, and he has lost the sense of position in his legs. But you can see that he is able to pick up a stethoscope very readily, and with the eyes open he can touch his nose very well. Even with the eyes shut he does not go far wrong. This shows that the arms are not markedly ataxic. He has another symptom which is usually an early one, namely, ptosis in the right eye, and a certain amount of paralysis of some of the ocular muscles. He has diplopia now. Perhaps I can show you the laxity of his muscles more than I could in the female patient. As he lies on the couch I can flex his thigh almost to the abdomen, and at the same time flex the leg so as to approximate the

heel to the buttocks. By flexing the thigh with the leg extended, the lower limb can be brought well beyond the vertical position, and could be got almost to his head but for the resistance of the soleus and gastrocnemius, which have rather a better tonus than the other muscles. By abducting his thighs they can be got almost flat on the bed. I pass round a photograph showing the degree to which the woman patient could flex her thighs and trunk when she was in the hospital.

I think these cases will illustrate sufficiently the various degrees of ataxy, and the different stages of ataxic gait met with in tabes.

I should like now to mention a few other diseases in which this symptom occurs, because they may give rise to confusion. The first disease is peripheral neuritis. In some cases of peripheral neuritis there is a very close assimilation to tabes. There may be the ataxic walk, the pain in the legs, the absence of knee-jerk; but there is never the Argyll Robertson pupil. Therefore the pupil phenomenon is very important in the differential diagnosis of these two diseases. I am afraid I cannot show you a case of peripheral neuritis in which there is true ataxy, though such cases do exist. But I can show you a case which is possibly peripheral neuritis, in which there is a strange walk which might be confounded with ataxy. It is not really ataxy at all, but is a high-stepping gait due to a particular form of paralysis. This woman has a peculiar gait which does not consist in unsteadiness or in flourishing the feet, but in lifting the knees unduly high. She has paralysis of the dorsiflexors of the feet and toes. She cannot lift her toes up properly, therefore to clear the ground she lifts her knees unduly high. Such a gait as that might give rise to confusion if you are not familiar with it. Her knee-jerk is absent, and there is no particular atrophy of muscle. It also happens that her present condition has developed very slowly, which again heightens its resemblance to tabes. Her pupil still reacts to light. I only introduce this case to you to show you the contrast between this gait and the gait of tabes, and to emphasise in your minds the fact that neuritis may conceivably be mistaken for tabes. In most cases of peripheral neuritis the disease develops rather quickly, whereas tabes almost always comes on slowly.

Another form of disease in which there may be

the ataxic gait is disseminated sclerosis. I do not think it is very common to see the subjects of disseminated sclerosis walk with a truly ataxic gait; it is more commonly a paraplegic gait in that disease. But there is a case in the hospital which I think you saw at Dr. Buzzard's lecture, of a man with a very typical disseminated sclerosis, who walks in an ataxic way. His knee-jerks are exaggerated, and he has typical nystagmus, tremor, and an affection of speech. In this case, therefore, there is not much risk of confusion with tabes.

Another affection in which there may be ataxy is cerebellar disease. The gait in cerebellar disease is not quite the same as in tabes. A cerebellar subject staggers, but he rarely flourishes his feet. Still, in the early stage it is conceivable that the two diseases might be confounded. In cerebellar disease there is no impairment of sensation, and the pupils usually react to light. The knee-jerk may or may not be absent.

There is in the hospital a patient who was a very excellent case of cerebellar ataxy whom I hoped to show you, a case under Dr. Tooth, but he has been operated upon. He had the typical "cerebellar reel."

There are other diseases which may give rise to ataxy, and may therefore possibly be mistaken for tabes. They belong to the class of diseases in which not only the posterior columns are degenerated, but the lateral columns also, known anatomically as postero-lateral sclerosis or combined sclerosis. This combined sclerosis occurs in a good many diseases which clinically are not very much alike. For instance, it is found very often in general paralysis of the insane. In that disease the principal and most striking lesion is in the brain, and usually the most obtrusive symptoms are mental. But there are certain cases in which the spinal cord is involved, the posterior and lateral columns being degenerated, and the mental symptoms not very marked. In such there is risk of confusion with tabes.

Another form of combined sclerosis is known clinically by the name ataxic paraplegia. In this the gait is not altogether ataxic, and not altogether paraplegic, but rather a kind of mixture of the two; the ataxic or the paralytic characters respectively predominate according as the posterior or lateral columns are principally involved. I cannot show you an example at the present time.

There is another class of case in which both the columns are diseased, namely, hereditary ataxy or Friedreich's disease. In that affection the posterior columns are involved from the first, but we do not know when the lateral columns are attacked. Still, in almost all the cases in which post-mortem examinations have been made the lateral columns were involved, though less deeply than the posterior. Though I cannot show you the ataxic gait of Friedreich's disease, it will be interesting to show you a patient, under the care of Dr. Hughlings Jackson, who is the subject of it, but who cannot walk. It is a very typical advanced case. The man is aged twenty-five. The family history in these cases is important, because we generally find that several members of the family are diseased. That is not the case in the other diseases I have mentioned. There is no history of his disease in any of this patient's ancestors, nor any history of any nervous disease. But an elder brother suffers from the same disease in a more advanced stage. He has a younger sister who is alive and quite well. This patient used to be strong and athletic. But when ten years of age he noticed that he stumbled when he ran, that is to say, he began to be ataxic at that age. In these cases the first symptom is usually ataxy beginning in the legs and slowly spreading to the arms. At eleven years of age he had begun to have shooting pains. These lasted two years. That is rather unusual, for in this disease the shooting pains are not such a prominent nor early feature as in tabes. Very often they do not occur at all. At the age of thirteen he staggered when he walked, especially when he walked in the dark, and his feet began to be deformed. That is a symptom not found in ordinary tabes, but common in Friedreich's disease, the inability to walk being completed by the deformity of the feet. You will see that this patient's foot is humped, with a very high plantar arch. The toes are very much drawn up. I presume it is due partly to paralysis of the interossei and partly to contraction of other muscles. The curling up of the toes is generally the commencement of this deformity. His knee-jerks are absent, and that is the case in nearly every subject of this disease, even in the very early stage. With his ataxy and the foot in this condition it is impossible for him to stand. I had a case under my care once at St. Bartholomew's Hospital, and I had the

deformity put right by the surgeon. But it was not much good, for in a very short time the feet were as deformed as ever. This young man's speech also is altered. Affection of speech is fairly constant in this disease, but it never occurs in tabes. I ask him to repeat phrases after me, and you will notice the peculiarity in the speech; it is not easy to describe it, but it may be said to be a thick elisive sort of speech, the syllables being run together rather than disjointed as in disseminated sclerosis. At the age of twenty-two he began to get worse, and could not stand. He noticed that the spine was getting deformed soon after the disease started. You will see that it is very markedly curved. Kyphosis and scoliosis of the spine are likely to occur in this disease, and that may partly account for the humped-up appearance, which you see the patient presents as he sits here. This is a typical case, for he has nearly every symptom that such cases ordinarily show. When you see the disease in an advanced stage there should be no danger of confusion whatever, neither indeed do I think there should be confusion in the early stages, for it generally runs in families; it occurs in young people, and the sensory symptoms which are characteristic of tabes—pains, anæsthesia, and such like—are usually completely absent. Of course when you find deformity of the feet and of the spine that clinches the diagnosis. Another symptom which occurs in this disease (late, as a rule), but which as far as I know never occurs in tabes, is nystagmus. This patient has it, but owing to the oscillation or tremor of his head it is not easy to demonstrate to you.

Ulcerative Stomatitis.—A. Kissel ('Arch. für Kinderhk.,' vol. xxii). A 3 per cent. boracic acid mouth-wash is used every hour, and three times a day the entire oral cavity is washed out with this solution, a saturated tampon being employed. This method suffices for the average case, but for obstinate cases the following method was employed:—The ulcers on the gums, tongue, and buccal mucous membrane are curetted once with a sharp curette; the surface of the wounds is then touched with iodoform powder. In addition the boracic acid tampon and mouth-wash are employed as in the milder cases.

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A CLINICAL LECTURE ON SENILE ENLARGEMENT OF THE PROSTATE.

Delivered at the Westminster Hospital, Dec. 13th, 1898,

By **CHARLES STONHAM, F.R.C.S.,**
Surgeon to the Hospital.

IN TWO PARTS.—PART II.

GENTLEMEN,—The treatment of enlarged prostate may be considered from three points of view:—First, there is the palliative treatment of the uncomplicated condition; secondly, that of the complications; and thirdly, that aimed at absolutely curing or removing the condition. The treatment, in the great bulk of cases, is palliative, that is to say, it is aimed at overcoming the effects of enlarged prostate without attempting in any way to cure the enlargement itself. The treatment aimed at curing the enlargement itself is all operative, and is not undertaken in ordinary circumstances, in view of the patient's age, and possibly concomitant damage to his kidneys, both of which conditions render him a bad subject for operative treatment, and moreover, all such operations are liable to be disappointing.

If a man presents himself to you with enlarged prostate, absolutely uncomplicated, the proper course to pursue is to teach him to pass a catheter. In order to carry this out, you will be wise to insist upon his remaining indoors for at least a week or ten days under your personal supervision until you have not only taught him to pass the instrument, but until he has become more or less accustomed to its use; for in passing a catheter on any man, there are certain accidents which may occur which may be in themselves very serious, and even fatal. It is by no means an uncommon event that after passing a catheter for stricture or other cause, the patient has a very acute and severe form of urinary fever, which, if his kidneys are at all diseased, may be rapidly fatal. Such a danger becomes increasingly more serious if the patient is old, and especially if there is evidence not only of enlargement of the prostate and its effects, but also some complication such as cystitis or chronic

nephritis. The object of keeping the patient in the house, and of your exercising supervision over him, is that you may empty his bladder and relieve what has been an over-distended organ (and which has perhaps remained so for many months), with the least possible danger to himself. The best plan is to have the patient's bowels open the night before you are going to pass a catheter, and to see that he is taking plenty of hot water, or some diuretic drink, such as citrate of potash, to ensure his kidneys being well flushed. If the weather be cold, you must see that there is a fire, and that the temperature of his room does not fall below about 62° F. Then you will pass the catheter, and draw off some or all of the residual urine. If there is only about six ounces in the bladder you may draw it all off; but if, as is sometimes the case, there is a pint or perhaps more, it is not wise to draw it all off at the first operation. Occasionally the sudden emptying of a bladder which has been chronically over-distended has caused the patient to die from syncope, owing to the sudden relief of pressure from the pelvic veins. By the gradual method you may completely empty the bladder in three or four days, and then teach the patient to use the catheter himself.

Subsequently the treatment consists in the passage of the catheter as often as may be necessary. In some cases where there are not more than three or four ounces of residual urine, it will generally be sufficient if the patient passes the catheter for himself once a day, preferably at night. In others, with six or seven ounces of residual urine, it will be necessary to pass the instrument night and morning. In others again, it may be essential that the catheter should be passed as often as the patient feels the desire to empty his bladder. In some cases you will find that the frequency of the desire to empty the bladder by passing a catheter becomes such that some other radical means for its drainage becomes necessary. I shall refer to this again presently.

Now with regard to the most suitable catheter. All sorts of catheters have been employed in the treatment of enlarged prostate, the Coudé, bi-Coudé, Jacque's, gum elastic, and the larger silver prostatic catheter have all their advocates. I may say about the last at once that you ought never to use them; few surgeons now use a metal instrument in cases of enlarged prostate; it offers no

possible advantages over a soft instrument, and it is much more likely to do damage to the enlarged middle lobe of the prostate. The Coudé catheter generally slips in quite easily. The catheter which I believe in most is the ordinary English catheter, which is curved somewhat like a note of interrogation—?. The catheter should have a stylet passed along it, and being bent to this shape is left so for a few days, when you will find that it will retain it. Personally I have never failed to pass such an instrument into the bladder past an enlarged prostate; it must be introduced with the concavity of the main curve looking towards the dorsum of the penis after the stylet has been withdrawn, and the catheter straightened out. There is an irresistible tendency for the catheter to regain its old shape as it passes along the urethra, and it will then ride over an enlarged middle lobe when perhaps no other instrument will do so. I believe it to be out and away the best.

An exceedingly important thing is to advise your patient with regard to keeping his catheters clean. There has been many and many a man killed either by the want of receiving instructions as to how this should be done or of carelessly neglecting them. If a man can conveniently use a Jacque's catheter (and such can be used in many cases of enlarged prostate), he should be told to sterilise it *before* and *after* use by boiling: this is one of the great advantages of indiarubber. If, on the other hand, the patient has to use a gum elastic catheter, the best way to clean it is to wash it through with boracic acid with a washbottle, and keep it in boracic acid. When you are teaching the patient to use the catheter himself you should always keep it in boracic acid, though of course under ordinary conditions a man cannot do so. What you should do is to give him his washbottle and some boracic acid solution, or, if you prefer it, some mercuric chloride solution, and tell him that before using his catheter he is to syringe it through and wash it out with the solution, and go through the same process afterwards. He should keep his catheter in a tin box or in a long glass bottle such as one uses for tooth brushes. Many old gentlemen with enlarged prostates have a habit of wrapping their catheters up in a piece of paper and putting them in their inside pockets: occasionally you will find such a patient produce a catheter from his hat! Of course that is absolutely wrong; it is

courting the occurrence of cystitis and neglecting ordinary antiseptic principles.

The general treatment of an uncomplicated case of enlarged prostate, must obviously also be directed to the avoidance of complications. The patient should live plainly, but he should live well; he should be abstemious in the use of alcohol, and his ingesta generally should be such as will not throw much work upon his excretory organs. If he has a tendency to renal disease you should diminish as far as may be his nitrogenous food, such as meat, and he should live chiefly upon milk, eggs, fish, fowl, and so on. The patient must be warned against alcoholic or sexual excess, either the one or the other is liable to give rise to acute congestion of the prostate, which, if already enlarged, may swell to such an extent as to induce acute retention of urine; or, short of this, the patient will have serious trouble in passing his urine which may necessitate some surgical interference; possibly he will pass a considerable quantity of blood per urethram. Again, patients should be warned against staying out late at night, against sitting on damp grass, or going out in cold damp weather, all for the reason that they may excite congestion of the gland. I generally tell patients who are the subjects of this trouble that they ought not to be out after sundown, and I think this is a pretty good rule for them to follow.

With regard to the treatment of the complications of enlarged prostate, let us consider first of all complication disease of the kidneys. If a man has practically sound kidneys and is taught to pass his catheter in the way I have told you, and to keep it clean, the mere fact of the enlargement, given ordinary care on his part, ought not to shorten the patient's life by a day. But if, on the other hand, his kidneys are diseased, and especially if they have been so for some time, the mere introduction into catheter-life may kill him in six weeks or six months, for he may develop a state of chronic urinary fever, gradually fall into ill health without anything obviously wrong to account for it; he cannot sleep, his appetite falls, he becomes feeble, and complains that his general health is suffering. This is probably a condition of kidney which is out of your control, and for which you are not responsible; it has been set up by damage caused by long neglect of the use of the catheter. At the same time there are certain forms of renal

disease which you may, by injudicious treatment on your part, excite. For example, if a patient goes a long time with chronic cystitis, or if you give him an attack of acute cystitis by using a foul catheter, you may wake up a kidney which would otherwise have done good work for a long time to come; acute diffuse nephritis may be set up in this way, with probably points of suppuration here and there in the cortex of the organ. The only way you can avoid such a disaster is by insisting on the ordinary treatment being carried out, and, above all, by insisting upon cleanliness.

A patient with enlarged prostate may develop cystitis in consequence of the irritation of calculi, or of the introduction of septic material from without; the latter ought never to occur. Supposing that the condition is dependent upon calculi, they must be removed, but how this is to be accomplished with the minimum risk will depend upon circumstances. If possible, you should remove the calculi by lithotrity; but sometimes you find the general condition of the patient is such that such a prolonged operation as lithotrity is not justified. The kidneys may be in a weak condition, and it may be that the urethral irritation which the necessary instrumentation will excite is more than they can stand; in such cases it is better to do what would, at first sight, appear to be a more serious operation, but which, owing to the shorter time it takes, is really less serious—namely, suprapubic cystotomy. Another occasional bar to lithotrity in these patients is the size of the middle lobe of the prostate, with a large pouched bladder, at the bottom of which the stones lie behind the prostate. Under these circumstances, although the man's general condition may be such that he could stand lithotrity, yet the mechanical disadvantages are so great that suprapubic cystotomy will be the more suitable operation.

In the case of cystitis occurring independently of the irritation of stone, you have to treat the condition on ordinary principles. There is septic material in the bladder, and if you leave that septic material there the condition will become worse, the septic micro-organisms will extend from the bladder up the ureters, and thus to the kidneys, and the patient may die of suppurative nephritis. Therefore you should pass a catheter with a large eye, and wash out the bladder once or twice, or even thrice a day, with the hope of removing the

septic matter, and thus subduing the cystitis and improving the general condition.

You may find sometimes that the condition of the bladder is such, and the irritability of the urethra so great, that repeated washing out does not suffice. Either it does not keep the bladder clean enough, and the patient is gradually sinking from cystitis and renal mischief, or the urethral irritation is so great that the patient will not tolerate it. Under such conditions you have to drain. How you will do this must depend upon circumstances. Some surgeons recommend very strongly perineal drainage. To my mind, the disadvantage of that, in the abstract, is this: that you have to go through the enlarged prostate, and, with the middle lobe of the prostate enlarged, you may find it is a difficult thing to get into the bladder. Another point is that the depth at which the bladder lies in cases of prostatic hypertrophy is frequently very considerable, and you may not be able to reach the bladder well. It is said that one advantage of perineal drainage is that it is easily and quickly performed, and that through the perineal wound you may be able to remove a portion of the enlarged prostate. At the same time, all these statements might be made with equal truth with regard to suprapubic cystotomy. As a general rule, I should drain these patients above the pubes; this can be safely done by thrusting a good-sized trocar and canula into the distended bladder below the reflection of the peritoneum. The canula may be left in for a sufficient time to ensure a patent opening, and is then replaced by a rubber drainage-tube. It is, however, better to make a small opening above the pubes, pull the bladder well up, and then make a sufficient opening into it to introduce the finger; you can thus explore the calculi which may be present, although you have failed to find them, and, if found, they can be removed. Another point is that, if the patient is in good condition, you can extend the operation and take out the middle lobe of the prostate. The disadvantage of having a permanent opening above the pubes is no greater than that of having one in the perineum.

The next complication is acute retention caused by congestion of the prostate. This ought to be avoided by attention to the ordinary rules which I have given you. The patient should live quietly, keep himself warm, remain indoors in bad and

damp weather, should avoid alcohol, especially sparkling wines, and must abstain from sexual excitement. With acute retention the agony the patient suffers is pretty extreme. His prostate is swollen and extremely tender; the desire to micturate is intense, but he may be totally incapable of passing any urine, or he may be able to pass a few drops mixed with blood which causes him smarting pain; the general condition is serious.

Supposing you meet with such a case of acute retention with congestion of the prostate, the first thing you have to do is to try to pass a soft Jaques' india-rubber catheter, and if you are successful the best plan is to tie it in, a procedure which the patient will probably tolerate, and hence you avoid the difficulty and irritation attending repeated instrumentation. But if you find that he proves intolerant of the constant presence of the instrument it must be withdrawn. At the same time, you should freely open the bowels by enema, and give a suppository of belladonna. Hot baths are exceedingly beneficial, they tend to lessen the irritability of the bladder, and to diminish the congestion of the prostate. You will frequently find that in attempting to pass a catheter in such a case the patient bleeds pretty profusely, but this is really not such a serious matter as might at first sight appear. The bleeding undoubtedly relieves the congested condition of the prostate, and, under ordinary circumstances, does not do any harm. Supposing you find that it is impossible to pass any form of catheter, yet the patient's symptoms demand urgent relief, you are driven to perform perineal or suprapubic cystotomy, and for the reasons I have already stated the latter is the operation to be selected.

You will have noticed that the treatment I have so far alluded to has been merely that directed to the mitigation of the *effects* of enlarged prostate and of the possible complications of that condition, but I have not yet said anything about the radical treatment of the prostate itself. The operations performed with a view to a radical cure are by no means certain in their object, and are, moreover, of some severity and danger considering the parts involved—the age of the patients, and the fact that they are very frequently the subjects of diseased kidneys. Prostatectomy may be done either through the perineum or above the pubes. There are some who advocate a median

incision in the perineum followed by enucleation of portions of the gland and removal of its middle lobe. Others think that the best plan is to open the bladder above the pubes, and then enucleate the large middle lobe. For my own part, I have no doubt whatever that the suprapubic is not only the better operation but that it is the only one which ought to be performed. In looking at the anatomy of enlarged prostate one well knows that it is not the fact of enlargement which does the harm, it is its seat. Only a very slight enlargement of the middle lobe may be sufficient to cause the most severe symptoms, and it appears to me that the readiest and safest method to reach this part is not through the narrow channel of the perineum, but by making an opening above the pubes. I have performed suprapubic prostatectomy on several occasions, but I have never yet operated by the perineal route, and I never shall; perhaps this is rather a dogmatic statement, but it will have to be under very peculiar circumstances if I am ever led to do it. I have seen the perineal operation performed on several occasions, and it has always struck me that it is far more severe and less satisfactory than is the suprapubic method. By the suprapubic method you can assuredly do the thing more certainly; in the perineal operation you are working to some extent in the dark.

With regard to removal of the prostate, there is no trouble about it. As soon as you have exposed the part you simply divide the mucous membrane over the middle lobe, and enucleate it with your finger. The hæmorrhage is generally pretty smart, but it usually ceases almost at once on the application of sponge pressure and ice. It is not a matter which need worry you much; but it is well to bear in mind that these patients, being old and very likely having bad kidneys, are not the sort of people who will stand much loss of blood, and therefore you must take every possible means to arrest the bleeding as soon as it occurs. After the operation it is customary to drain the bladder above the pubes for a varying period, depending on the amount of clot in the bladder and the condition of its lining membrane. You may combine permanent suprapubic drainage with prostatectomy. The radical treatment of enlarged prostate by puncturing the gland with the actual cautery is not, in my judgment, a good (I had almost said justifiable)

method, and I shall, therefore, merely mention it.

Two other operations have recently come into prominence owing to the advocacy of White, of Philadelphia. These are castration and vasectomy. It was supposed, in fact it has long been taught, that enlargement of the prostate had certain analogies with the development of fibroid tumours of the uterus. Although this may be true in some respects, I believe, if you think over the matter, you will find that the analogy is not quite as real as at first sight might appear. However, working on that analogy, it was suggested by White that removal of the testicles might cause atrophy of the prostate gland, in exactly the same way that we know fibroid tumours of the uterus frequently undergo involution at the climacteric period, and that their involution can be hastened by removal of the uterine appendages. There is no doubt whatever that in proper cases castration does produce atrophy of the prostate; double castration causes atrophy of the whole gland, single castration is followed by atrophy of the prostate on the same side. Castration is followed by a certain degree of amelioration in the symptoms, so much so that in some cases you find that a patient who previously had to withdraw all his water by catheter, will, perhaps a fortnight or three weeks after castration, be able to pass six or eight ounces himself without resorting to the catheter except occasionally. The last case of castration which I had was on an old gentleman whose case followed that course. For years he had used a catheter, and from him I had removed many stones at different times; after castration his condition very much improved, the prostate atrophied considerably, and the state of his bladder was much better; there was improvement in his general condition, and he passed six or eight ounces of urine with ease. All cases are however, not suitable for the operation of castration. If the man is very advanced in years, if his general health is feeble, and if his kidneys are obviously working improperly, and if the prostate is a dense hard prostate, it is not right to perform castration upon him. The best cases, no doubt, are those in which the patients are much troubled by their symptoms, in whom catheterism is badly borne, but whose kidneys are fairly healthy; the patient should be comparatively young, with a soft prostate which is enlarging pretty rapidly. In

cases like that I certainly would castrate, but rarely in others. You must not forget that although castration is, generally speaking, not a serious operation, yet that sometimes after double castration these old men develop acute mania, from which they may recover in a few days, but in not a few instances they succumb.

As an alternative to castration, it has been suggested that the vas deferens should be either divided or a portion of it excised. These operations have, in some measure, been adopted rather in deference to the wishes of the patient. If you suggest to a man that you should do a double castration on him (I do not care how old he is), he is horrified, and it may be a very long time before you can even get him to listen to such a proposal; but if you suggest vasectomy, he does not seem to so much care. The fact is that, having lost the substance, he likes to keep the shadow. But it is easy to see that vasectomy is not anything like such an efficacious operation as castration. At the present time we are hearing a great deal about the internal secretions of various organs—such as the testicle, ovaries, breast, and so on,—and it is possible that the mere retention of the testicle, quite apart from its being connected with its duct—because, practically, this duct is, at the patient's age, functionless, or nearly so—may have its effect upon the prostate. It is the removal of the testicle, rather than of its duct, which has a beneficial effect. At any rate, if the patient rigorously declines to have castration performed, vasectomy may be done. Again, if the patient's condition is such that some radical means are demanded to rid him of his trouble, but his general health is bad, his kidneys are diseased, his age great, no surgeon would be justified in doing prostatectomy, and few would care, under these circumstances, to risk a man's life by double castration. Under these circumstances, the mere division of the vas deferens with a tenotome, or cutting out half an inch or so of it, is a comparatively safe procedure which might be tried.

You will find, as a rule, that the improvement after castration is quite marked in a fortnight or thereabouts; such improvement will probably continue for six months, or even longer. If there is no improvement in the condition within a month after the operation, the probability is that none will follow.

PYORRHŒA ALVEOLARIS AND ITS RELATIONS TO GENERAL MEDICINE,

BY

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(Continued from p. 335.)

Diagnosis and Treatment.

HAVING now a theoretical acquaintance with pyorrhœa, and its effects, both locally and upon the system generally, the next thing is to learn to recognise it when it comes under our observation. But since, as in other affections which are met with in daily practice, a scientific diagnosis comprises not only the recognition of the local condition, but also the estimation of any underlying dyscrasia, so, we must not content ourselves with a simple diagnosis of pyorrhœa. We must ascertain whether it is merely a local condition which can be removed by local treatment, or a local expression of a constitutional vice. We must therefore study in succession and separately the local condition and the constitutional state of the patient. We shall take these in order.

THE LOCAL CONDITION.

To cure pyorrhœa it is important to recognise it in its earliest stage. Unfortunately, this is rarely done by the dentist, who overlooks the affection until streptococcus invasion has taken place and pus pockets have formed. The most favourable time for cure has then passed. In order to simplify the study of pyorrhœa we shall divide it into four types, and discuss the diagnosis and treatment of each *seriatim*. The treatment of the first three of these can quite well be carried out by a medical man with no special dental training or knowledge.

Type 1.—This is the earliest stage of pyorrhœa, and there is not of necessity any visible tartar on the teeth. There is merely a gingivitis present, which will, if not checked at this stage, inevitably proceed to ulterior change. The margins of the

gums are inflamed, and beneath them are deposited on the necks of the teeth, dark and hard nodules or scales of the so-called serumic calculus. If not promptly cured, inflammatory degeneration of the pericementum will inevitably ensue, with eventual loss of the teeth. The edges of the gums are red, swollen, and spongy, and bleed when roughly touched either with a tooth-brush or the fingers. The act of sucking exerted in smoking a pipe will also cause them to bleed readily. It may, or it may not be possible, to cause pus to exude from under them by pressing with the finger tips.

The affection in this early stage, when it is not strictly pyorrhœa, because pus is usually not present, is frequently met with in debilitated and anæmic subjects, and among those convalescing from acute diseases. The diagnosis is made certain by the finding of serumic tartar under the edges of the gums. This, as has been already remarked, is deposited from the serum poured out as discharge, and is a certain sign of inflammation.

The first thing, then, after having discovered the presence of a gingivitis, is to examine for serumic tartar, and thus find out whether the affection in question is a trivial condition, or whether it is the first stage of pyorrhœa alveolaris. The instruments required for this purpose are a set of Cushing's scalers, see fig. 4. Some of these instruments, it will be noticed, have terminal hooks, as shown in fig. 4, *d, e, f*. The method of using them will be described presently.

The treatment of pyorrhœa in this stage is not difficult, and usually successful, and comprises the removal of the tartar, the application of an astringent to the inner margins of the gums, and the prescription of a mouth-wash for daily use.

The removal of the tartar.—To avoid repetition the directions for doing this will be deferred until types 2 and 3 are discussed.

The astringent application.—My favourite for this condition, and one which I have used for many years, is sulphate of copper reduced to an extremely fine powder. To apply it properly you require a small point of wood, about half an inch in length, tapering to a fine extremity. Such a piece of apparatus may be made from one of Bryant and May's large matches, whittling the end down to the proper shape, and finishing it off with a piece of fine sand-paper. It is conveniently set

at right angles in a special holder made on purpose, which may be procured at any of the dental depôts. The following is a full size photograph of the apparatus (Fig. 1).

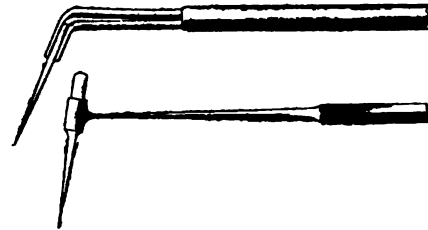


Fig. 1.

You will also require about two drachms of finely-powdered sulphate of copper in a small wide-mouth stoppered bottle. You must also have ready to your right hand a piece of blotting-paper, a small glass of cold water, and a large tumbler full of tepid water. Set the patient facing a good light, so that the inside of the mouth is well illuminated. Now, having selected the tooth on which you propose to operate, dip the wooden point into the water in the small glass, remove surplus moisture by touching it with the blotting paper, and dip it into the bottle of sulphate of copper, some of which will adhere to it. Now wipe off the powder on to the neck of the tooth close to the gum margin, and gently pack it down under the edges of the gum. Continue this process until you have treated all the affected teeth, and a blue line above the margin of the gums shows that the operation has been properly performed. Now let the patient rinse his mouth with warm water, which you have in readiness, and send him away with the following prescription:

R	Glyc. acid. carbol.	3iv
	Glyc. acid. boric.	3iv
	Potassæ. chloratis	3ij
	Euthymol	3iv
	Aqua anisi
	Aqua menth. pip.	...	āā	ad 3viii

Mix.—To be used as a mouth-wash night and morning.

It will also be found to materially expedite the cure if the patient use a few drops of listerine upon his tooth-brush. Indeed, a very good mouth-wash may be made by mixing one ounce of this preparation with six ounces of water.

The packing with sulphate of copper should be

done several days in succession, then every alternate day for a week, and then twice a week for another fortnight. The patient will then probably be cured.

Type 2.—On examining the patient's mouth, we see the sides of the teeth incrustated by well-defined masses of tartar, the lower edges of which are in contact with the margins of the gums. These are inflamed, reddened, and eroded. On close examination one may see between the edges of the gums and tartar respectively, a narrow zone of uncovered tooth, bathed in pus. On rinsing away this pus with the stream of a syringe, we are unable to squeeze any more from under the edges of the gums. On passing an instrument under the edges of the gums we may, as in type 1, find nodules of serumic calculus, but on examining very gently under the edges of the gums with a blunt instrument as in type 1, we cannot pass it down along the side of the root. There are, therefore, no pockets, and the affection is an example of type 2 and not type 3. The pus comes from the ulcerated edges of the gums, and is the product of the inflammatory process. The pyorrhœa is only in the incipient stage, as the periodontal ligament is intact, and we should readily cure it.

The local treatment will be exactly the same as described for type 1, with the addition of first removing the tartar from the sides of the teeth, and a preliminary treatment to subdue the local inflammation before the packing with sulphate of copper is commenced.

It will at this juncture be convenient to describe the proper method of removing tartar from the teeth. It is quite an easy operation if performed with a correct technique and with suitable instruments, and well within the power of any surgeon without any special knowledge of dentistry.

The necessary instruments are the following :

(a) Two or three crooked or hoe-shaped instruments of various sizes for the removal of the bulk of large concretion, by a pulling motion (Fig. 2).

(b) A set of Howe's scalers (Fig. 3) to be used in a similar manner for removing smaller portions, and (c) a set of Cushing's scalers for removing with a pushing and pulling motion the last particles of salivary and serumic calculi (Fig. 4). Some of these are also used in probing for pockets.

In the treatment of this stage of pyorrhœa we shall also require—

(d) A hypodermic syringe, provided with a special cannula (Fig. 5), the cannula to be made of gold, and very fine. This is used for the

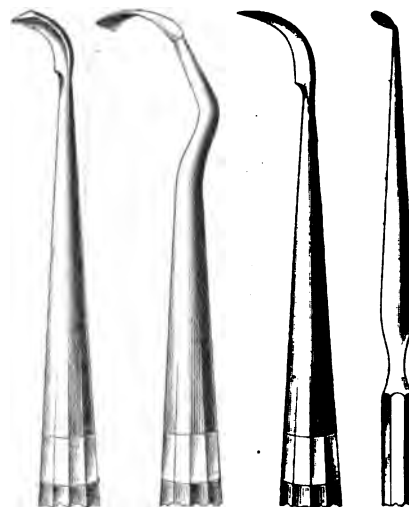


Fig. 2.

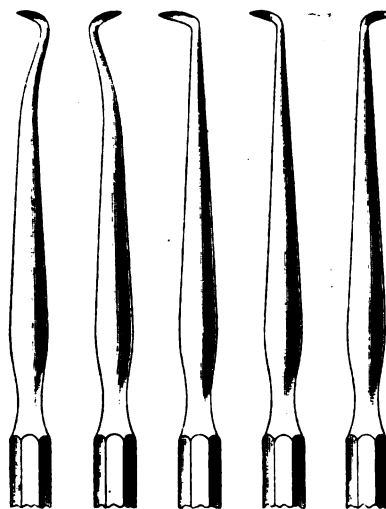


Fig. 3.

application of medicated solutions to the periodontal membrane, and to the pockets met with in types 3 and 4.

There is a right and a wrong way of removing tartar. In removing the large salivary calculus, a hoe-shaped instrument must be taken, and the end passed under the mass of tartar where it touches the gum. The instrument is then pulled towards the crown of the tooth. The accompanying illustration will make this plain (Fig. 6).

Salivary tartar around the sides of the teeth can readily be removed by the novice, but the dislodgement of the small hard scales of serumic calculus under the edges of the gums, and in pockets is much more difficult. The technique is

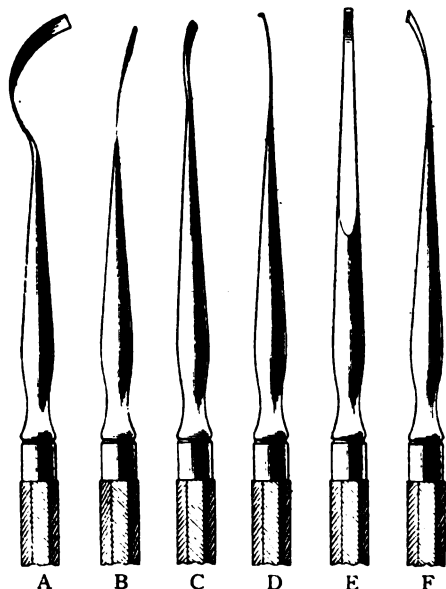


Fig. 4.—These scalers are of two kinds. Some such as A, B, C, are for detaching the calculus by pushing. Others, as D, E, F, are provided with a terminal hook, and are used for hooking out the *débris* detached by the former.

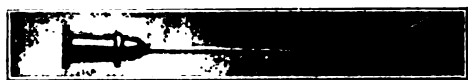


Fig. 5.

as follows (see Fig. 7):—Take a Cushing scaler, and holding it between the thumb and fore-finger, with the concavity of the blade towards, and its long axis corresponding, with that of the tooth, pass it gently down the sides of the latter, exercising a gentle pressure against it. When its extremity

reaches a scale of tartar it will be arrested. The chisel edge of the scaler is now in contact with the point where the nodule of tartar joins the tooth. If you now continue the forward movement of the scaler, the calculus will start right away from the tooth without any particular force having been used. Perfect control must be kept over the instrument in order to prevent it from suddenly moving forwards as soon as the resistance offered by the scale of tartar has been removed, and thus wounding the peridental membrane.

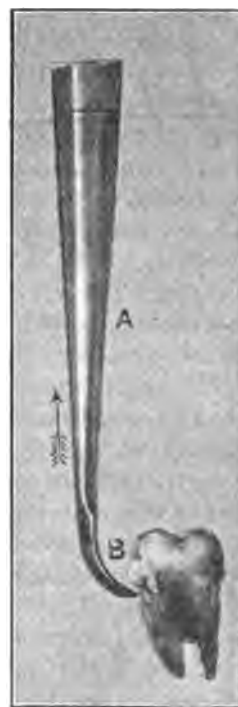


Fig. 6.—A Scaler. B Calculus. † Direction of pressure.

The process is the same when pockets are present (see type 4). The instrument is carried right along the root in the axis of the tooth, and every particle of tartar is loosened as it is encountered. Then with a "Cushing" hooked scaler of suitable size and shape (see Fig 4, *d, e, f*), remove the loosened and detached scales with a drawing or pulling movement.

It is most important that *every particle* of tartar should be thoroughly removed, and a careful search must be made for it, as any small particle overlooked will certainly keep up the irritation, and effectually prevent the healing process.

After the removal of the deposits, peroxide of hydrogen solution should be syringed under the edges of the gums, as by its effervescence it will wash up the particles which have been detached, and materially assist their removal. The gums may now be packed with sulphate of copper as already described, or may be painted with tincture of iodine every other day for a week.

In removing tartar at this stage of the disease, it is not necessary to avoid scarifying the gums; in fact, this will do good by removing blood, but one must be *very careful* not to wound the peri-

pus will be seen to well up along the sides of the teeth.

A careful examination with a thin flat blade will show that destruction of the peridental membrane has taken place, as the instrument will pass further than it should along the sides of the tooth into little pockets which have formed which contain pus. These pockets are characteristic of this stage of the disease. They follow the length of the fibres of the peridental membrane, and run up alongside the root of the tooth. They may be single or multiple, and may attack the lingual, buccal, or proximal sides of the teeth. The tendency is eventually to the destruction of the whole peridental membrane, as the pockets slowly widen, usually irregularly, until they encircle the teeth. At the same time that these changes are going on in the membrane, gradual absorption of the edges of the alveolar wall is taking place. In some cases, whilst the inner portion of this is being destroyed, deposit of new bone is taking place on its outer surface. The result of this is that the alveolar wall becomes lipped, and by its everted edge holds away the gum from the tooth. On examination with the instrument then we shall—

(a) Find pockets.

(b) Establish the presence of nodules and scales of tartar on the root of the tooth in the pocket.

(c) In many cases be able to make out the eroded and roughened edges of the alveolus.

(To be continued.)



Fig. 7.—A Scaler. B Calculus. † Direction of pressure.

idental membrane, as this is *intact*; and if you should be so unfortunate as to puncture it, you will open the door for bacterial invasion, and possibly produce type 3.

Type 3.—In this stage of the disease the peridental membrane has had its continuity destroyed at certain points, and consequently pockets have formed which have been infected with pus-producing organisms. It differs from type 4 inasmuch that there are, however, no loose teeth.

The gums may possibly be merely reddened at the margin, but when pressure is applied to them

The Production of Sex at Will.—Friedmann ('Klinisch-therapeutische Wochenschrift,' 1898, No. 30, p. 1156; 'Gazette hebdomadaire de médecine et de chirurgie,' January 8th), as a result of investigation into this problem, says that in order to resolve it these three questions must be answered:—1. Is it possible, by artificially modifying the material exchanges, to deflect from its primitive direction the normal course of the physiological functions of the organism? 2. Can any explanation be given of the fact that in certain families only boys, and in others only girls, are produced? 3. What are the most appropriate means to influence the material exchanges in the reproductive system of the animal organism so as to produce at will a given sex?

The first question has been amply answered in the affirmative by Darwinism. As to the second,

he says that women with a tendency to the exclusive production of one or other sex present signs of a degeneration so marked as to constitute a pathological condition—a sort of androgynous or gynæcogynous cachexia.

Haeckel has shown that the phenomena of heredity must be attributed to physical and chemical causes, and that the sexual characters must pre-exist in the original germ before the definition of the genital apparatus. The androgynous and gynæcogynous cachexiæ must be meant to imply that the female element in the first, or the male element in the second, is destroyed by its opposite in some organico-chemical process. Consequently, to answer the third question it is necessary to seek for either the male or the female element deposited in the ovule a sort of toxalbumin, so that one of the two elements may predominate over the other. Empirically he finds in ovarine the substance to combat the female, and in spermine that to overcome the male element. By submitting a female guinea-pig to the administration of tablets of ovarine from October 26th, 1897, to February 15th, 1898, he succeeded in obtaining the gestation of a single male 'normally developed. Another female, similarly submitted to subcutaneous injections of spermine, became pregnant with a single female. The uniparous pregnancy, he says, seems to indicate a lowered reproductive power.

N. Y. Med. Journ., March 18th, 1899.

The Toxæmic Factor in Diabetes Mellitus.

—As the result of a clinical study, McCaskey ('*Medicine*,' January, 1899, p. 1) formulates the following conclusions:—All cases of persistent glycosuria are cases of diabetes mellitus of varying grade. Diabetes mellitus is a disease of diverse origin, the unity of the clinical picture being for the most part dependent upon the glycæmia and the glycosuria, which are mere incidents, although dominating factors of the disease. Phloridzin-diabetes is not essentially different from clinical diabetes; it renders plausible the assumption of a chemical factor, either as a primary or as an important secondary element in the clinical type of the disease. Normal sugar transformation in the blood, the failure of which is responsible for the glycæmia and the glycosuria, results from the presence in the blood of a chemical product, derived in man principally, if not exclusively, from

the pancreas, and thrown directly into the blood from the pancreatic cells, without the intervention of a duct. The direct chemical antagonism of this substance by another is no more improbable than such an antagonism of a toxin by an antitoxin. It is probable, on both clinical and experimental grounds, that certain chemical poisons, for the most part of gastro-intestinal origin, but possibly also resulting from faulty tissue metabolism, or as a perverted internal secretion from glands not necessarily ductless, either directly or indirectly antagonise, in whole or in part, the sugar-destroying substance in the blood, thus giving rise to glycæmia and glycosuria, and thus in a certain group of cases either primarily causing or at least exaggerating the clinical phenomena of diabetes mellitus. If further investigation should corroborate the conclusions here provisionally set forth, it would be advisable hereafter to investigate the bacteriology of the stomach and the intestines in cases of diabetes mellitus; and if evidences of virulent bacterial, protozoan, or other parasitic growth are found, these conditions should be met by suitable treatment—not with the expectation of entirely supplanting dietetic treatment, but as an important auxiliary to the latter, possibly rendering its restrictions less severe, with less resulting impairment of nutrition.—*Med. Record*, Mar. 18th, 1899.

A Case of Nasal Vertigo.—'*Arch. Intern. de Lar., O., R.*,' September—October, 1898. The patient was a lady of thirty-eight, who consulted the author for some trivial throat affection. Three small polypi were found in the right middle meatus, and were removed under cocaine. Ten day later the patient returned, and stated that she had been a new woman since the previous visit. She then related for the first time that for more than a year she had suffered with giddiness on making the least movement, and this had been very marked when walking. On several occasions she had fallen. A variety of treatment had failed to give relief, and supposing the trouble to be intractable, the patient had accepted it as the inevitable. She was therefore quite unprepared for the complete suppression of the vertigo, which took place immediately after the removal of the polypi. There is, therefore, no question here of suggestion or of counter-irritation.—WAGGETT, *Journ. of Laryngol., Rhinol. and Otol.*, Ap., 1899.

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CLINICAL LECTURE ON TUMOURS OF THE CEREBELLUM, OPTIC THALAMUS, AND FRONTAL LOBE OF THE CEREBRUM.*

BY

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to the National Hospital for the Paralysed and
Epileptic, Queen Square.

GENTLEMEN,—I propose to deal with a certain group of cases of intra-cranial tumour, to which a good deal of attention has been directed during recent years, and some excellent examples of which came under my observation during the time that I was pathologist to this hospital. Owing to the latter circumstance, it will be possible for me to illustrate the subject during the course of the lecture by lantern slides of the conditions, as found on necropsy, in some of the cases to which I refer.

The group to which I wish to call your attention is comprised of tumours in situations so entirely different, and so widely removed the one from the other, that to those of you who do not happen to be familiar with the features which the cases may present in common, it may at first sight seem strange that tumours of the cerebellum, optic thalamus, and frontal lobe of the cerebrum should present features sufficiently in common, in so far as their clinical manifestations are concerned, as to justify their being considered collectively as forming a special group. Strange as it may seem, however, tumours in these three situations have certain features in common, which cause them to resemble each other to a variable degree in different cases. This fact is of special interest when we recall the further fact that these three regions within the skull, though widely separated from each other, have important anatomical connections. To my mind, the chief difficulty in diagnosis which arises is not

* Delivered at the National Hospital for the Paralysed and Epileptic, Queen Square.

so much the liability to mistake a tumour in the cerebellum for one in either of the other two situations, as that of mistaking a growth in the frontal lobe or optic thalamus for one in the cerebellum.

During the course of my remarks I shall have two main objects in view—a consideration of any resemblances which the members of this group bear to each other, and an examination of any features, which may be regarded as sufficiently distinctive to differentiate one from another.

What, then, are the phenomena common to the different members of this group of intra-cranial tumours, which justify their being considered collectively?

I shall, in the first place, relate the symptoms which they may possess in common, without any comments as to the relative frequency or severity of these symptoms in the individual members of the group, leaving such detailed considerations until we come to deal with the points which serve to distinguish a tumour in one situation from one in another. Moreover, I shall reserve any comments I have to make with regard to my own opinions as to the resemblances, real or supposed, until we come to consider the differential diagnosis of the individual members of the group.

Leaving out of consideration headache, vomiting, and optic neuritis, symptoms common to all forms of intra-cranial tumour, *ataxy or inco-ordination* of movement, which, as we are all aware, is the great feature which characterises cerebellar tumours, is in the forefront. This symptom, then, may also be present in greater or less degree both in cases of tumours of the frontal lobe of the brain and in those situated in the optic thalamus. Various observers have commented on the fact that this symptom is one which occurs in the different members of the group now under consideration; but I may specially refer you to the work of Dr. Bruns, of Hanover (*"Die Geschwülste des Nervensystems"*), a paper by Dr. Williamson, of Manchester, which deals with the symptomatology of gross lesions of the præ-frontal region of the brain (*"Brain,"* 1896, p. 346), and a recent discussion opened by Dr. Beevor at the Neurological Society on the localisation of intra-cranial tumours (*"Brain,"* 1898, p. 291).

As a result of experimental ablation of the cerebellum, I have been able to determine that a

characteristic attitude obtains when one lateral half of the organ is removed, and I have also had opportunities of observing this same attitude in cases of cerebellar tumour in man. In that Schiff and Nothnagel have described a certain attitude as resulting from lesions of the optic thalamus also, it is necessary for us to give some attention to this subject.

Nystagmus is another symptom which may be present in any of the cases of this group, or, more correctly, nystagmoid jerking of the globes when they are voluntarily moved laterally, or, it may be, in a vertical direction, as there may be no spontaneous nystagmus as long as the eyes are in the resting position.

Anosmia or loss of smell, while occurring both in frontal and cerebellar tumours, has not; so far as I am aware, been supposed to be in any way specially related to tumours of the optic thalamus.

Absent knee-jerks, like the last symptom, is supposed to be especially common in cases of cerebellar and frontal tumours.

Mental defects, on the other hand, while occurring in cases of frontal and optic thalamus tumours, do not belong to the group of symptoms which serves to link these cases to those in which the tumour is in the cerebellum.

Loss of control over the sphincters has been supposed to be one of the points which characterise frontal tumours, and has also been met with, in so far as the sphincter of the bladder is concerned, as an early symptom in some cases of tumour of the optic thalamus.

Let us next analyse the symptoms that have just been narrated, with a view to ascertain how far distinctions between the different members of the group of tumours can be determined by a careful study of the very symptoms which cause them to resemble each other.

Inco-ordination, though a symptom which may characterise a part of the clinical picture of tumour in any of these situations, is not met with nearly as frequently, either in frontal or optic thalamus cases, as it is in cases of tumours of the cerebellum. Moreover, while the ataxy may be only slightly marked in a given case of cerebellar tumour, it may reach a degree of intensity which is not seen either in frontal or optic thalamus tumours. So, too, there appears to be an important distinction in regard to the way in which the inco-ordination

manifests itself; at least, if one may judge from the published records of recent cases which are in harmony with one's own observations. In cerebellar tumours inco-ordination does not only show itself by a reeling or ataxic gait, but a variable degree of general titubation may be observed, in which the head, neck, and trunk share. Moreover, inco-ordination is commonly manifested in both upper and lower extremities when the various tests are adopted as the patient sits or lies in bed. In frontal cases, on the other hand, while the unsteadiness of gait and falling in some one or other direction may be features, neither general titubation nor inco-ordination of the individual limbs are so manifest. Optic thalamus tumours appear to be more commonly responsible for inco-ordination, which manifests itself in some individual limb in the shape of choreiform or athetoid movements—intention tremor and the like—rather than by ataxy of gait, reeling and falling, and so forth.

As I have already said, these possible distinctions as to the way in which inco-ordination manifests itself in the individual members of the group we are considering, are suggested by the statements that have been made in connection with recorded instances in such affections, which are in harmony with one's own observations, and in calling your attention to the matter I merely do so to indicate the lines on which our future investigations should be based, rather than as meaning that sufficient observations have been made to make these points valuable in the differential diagnosis of the class of cases under discussion.

Attitude.—As far as has been determined by experiment, and by clinical observation, the attitude to which I have already referred, and to which I wish to call your attention more in detail, does not obtain in cases of tumour situated in the frontal lobe; and while there may be some resemblance between the attitude resulting from an experimental ablation of one half of the cerebellum, and that which occurs in the case of a lesion of the optic thalamus, I am not aware that tumours in the latter situation have been observed to produce this attitude. I have, however, seen what one is disposed to call the cerebellar attitude in the case of a large tumour situated in the hinder part of the brain, in which case the attitude may have resulted from interference

with the optic thalamus, or, possibly, from pressure on the superior cerebellar peduncle. The cerebellar attitude as seen in man consists in approximation of the side of the face to the shoulders on the side of the lesion, while the head is at the same time rotated on its vertical axis so that the chin points to the normal side. In the experimental lesions of the optic thalamus, produced by Schiff and Nothnagel, the head looked towards the uninjured side.

Nystagmus, like ataxy, is very much more common and more pronounced in cerebellar tumours than in the case of growths in either of the other two situations, and, whereas, it may be either spontaneous or evoked only on voluntary displacement of the globes from their resting positions in cerebellar cases, it is usually the latter variety of nystagmus, more correctly described as nystagmoid jerks, which is met with in cases of tumour of the frontal lobe or optic thalamus. Spontaneous nystagmus or pronounced nystagmoid jerks or displacement of the globes from their resting positions, point with a strong degree of probability to the seat of the neoplasm being the cerebellum, as opposed to either of the other situations under consideration.

As far as I am aware *loss of smell* has not been supposed to be specially related to lesions of the optic thalamus. That this symptom should obtain in cases of tumours of the frontal lobes of the brain is scarcely to be wondered at when we remember the close proximity of the olfactory tracts and bulbs; indeed it is surprising that smell should be preserved as often as it is in cases of tumours in this situation. Some years ago I had, in common with others, a strong impression that smell is commonly lost in cases in which the neoplasm is situated in the cerebellum; but while there is no question that this defect is met with in some of these cases, a larger experience and more recent statistics prove that the occurrence of anosmia is less frequent than my earlier impressions would have led me to suppose. The mechanism by which this loss of smell is brought about in cerebellar cases is not quite clear, unless we accept the hypothesis that it is dependent on a neuritis comparable to the change which may occur in the optic nerves. Nevertheless, the clinical fact remains that in cerebellar tumours, as in those located in the frontal lobes of the brain,

loss of smell may be a symptom; this symptom, however, is more common in the latter class of case, and, moreover, when there is unilateral loss it points with a greater degree of probability to the frontal lobe on the side of the loss being that affected, rather than its fellow of the opposite side.

Like the last symptom, I am not aware that *abolition of the knee-jerks* has been supposed to be in any way specially related to tumours of the optic thalamus, but as has already been said, such loss is supposed to occur with especial frequency both in cerebellar and frontal tumours. My own experience with regard to loss of knee-jerks in cerebellar cases is very similar to my experience in regard to loss of smell in these cases. There certainly was a time when I regarded absent knee-jerks in connection with evidences of intra-cranial tumour of doubtful seat, as lending support to the probability that the neoplasm was situated in the cerebellum, and that I was not alone in that belief is evident from the statements we find in papers and text-books which deal with the subject. Subsequent experience obliges me to abandon such a position as quite untenable. Certain recent observers have insisted on the frequency with which the knee-jerks are abolished in frontal tumours, a fact which in itself robs the symptom of the diagnostic significance it was once supposed to have in cerebellar cases. But experience teaches us more than this; we find that absent knee-jerks are far too frequently met with in new growths in other situations in the brain to make the symptom of any diagnostic importance either in cerebellar or frontal tumours. We have now come to recognise the fact that any large neoplasm within the intra-cranial cavity, and especially if it be one of rapid growth, may be and commonly is attended *with great diminution or abolition of the knee-jerks*.

A study of the symptomatology of frontal tumours makes it clear that *mental symptoms* are prominent, and what is far more important in diagnosis is the fact that they as a rule occur early, and usually persist throughout the whole course of the case. A certain amount of mental defect, variable in degree in different cases, may be met with in the later stages of a tumour of the brain irrespective of its precise situation; but when mental symptoms occur early in the clinical course of a given case, this is strongly in favour of the

tumour being situated in the frontal lobe. The mental defects vary in character in different cases, and often when there is nothing very definite that can be clearly described, there is a peculiarity or oddness of manner which is very suggestive to those familiar with this kind of mental state. Loss of memory is usually a prominent feature, and there may not only be loss of memory for names and events, but the patient may fail to recognise his nearest relatives and friends, or may mistake strangers around him for certain of his relations, they become dull and apathetic, and to such an extent may this be met with that such patients pass their evacuations under them without concern. Possibly it is this state of things which has led to the belief that loss of control over the sphincters is a sign in favour of the seat of the tumour being the frontal lobe. There is no attempt on the part of the patient to take the initiative in any matter; he takes no notice of what is going on around him, and it is difficult to fix his attention, sustained conversation being usually impossible.

Oppenheim has described a peculiar hilarious mental condition with unmeaning laughter, which Bruns regards as of diagnostic significance in frontal tumours. Delirium has been noted in some cases, especially at night.

As has already been said mental symptoms cannot be regarded as forming part of the clinical picture of a case of cerebellar tumour; but it is otherwise with some cases of optic thalamus tumours which come to resemble growths in the frontal lobes in consequence of the mental defect which may be present, and which have been characterised by a dull apathetic condition and slow mental state in some cases, while in others it has been rather a condition of mental excitement that has been present.

It is singular how rarely we meet with speech defects of a truly aphasic character, even when the tumour is in the left frontal lobe, and that in persons comparatively advanced in life; we may even meet with a variable amount of hemiplegia as a result of pressure on, or invasion of the motor region immediately behind the frontal lobes, without any concomitant disturbance of speech. We are accordingly led to assume that this is owing to any disturbance of Broca's convolution being so gradually brought about that the corresponding region of the opposite side of the brain

is able to take on the work concerned with outgoing speech processes to a sufficient extent to prevent any aphasic defect resulting. How great a part the region of the brain in the right cerebral hemisphere, which corresponds to Broca's convolution in the left, may participate in speech processes in some people is strikingly illustrated by a case recently recorded by Dr. Byrom Bramwell,* of Edinburgh, in which destruction of Broca's convolution by softening in a right-handed man was followed by such very temporary motor aphasia that it is difficult to come to any other conclusion than that arrived at by Dr. Bramwell, that the corresponding area of the brain on the right side had been sufficiently educated and active to immediately take up and carry on the work of Broca's convolution.†

Nevertheless, if aphasic defects be present, they not only point to the growth being in the frontal lobe as opposed to the other seats we are considering, but they also indicate that the left side of the brain is affected, provided that neither the person himself nor one of his ancestors has been left-handed.

Loss of control over the sphincters has not so far as I am aware been claimed as in any way specially related to tumours of the cerebellum, so that the existence of this defect in any case in which diagnosis is in doubt, warrants our looking to the cerebrum rather than to the cerebellum for the explanation. Now we have already seen that this symptom has been claimed as of some importance in the symptomatology of frontal tumours, and we have also seen that at any rate loss of control over the sphincter of the bladder has been met with as an early symptom in some cases of optic thalamus tumours. But when we come to compare the frequency of occurrence of loss of control over the sphincters in frontal as opposed to optic thalamus tumours, we find that the symptom is much more commonly met with in the former than in the latter class of case. Before leaving this part of my subject, it behoves me to say that what is described as loss of control over the sphincters,

at any rate, in so far as the frontal cases are concerned, and probably the argument holds good in optic thalamus cases also, in, at any rate, the majority of the cases, is not due to any paralytic defect of the muscles, but is rather the result of the mental hebetude which we have seen is so common in frontal tumours, and which allows of the calls of Nature being obeyed without concern as to where the excreta are evacuated, and thus the patient passes everything under him without being apparently aware of what has happened.

As we have now reviewed the symptoms, the existence of which may make a tumour in one of the situations under consideration resemble one in another of these situations, let us next ascertain what other symptoms may form part of the clinical picture in any of these cases, and what indications they furnish as to the probable seat of disease.

Fits are certainly in favour of the tumour being in one of the situations in the cerebrum, as opposed to its being in the cerebellum. It is true that general convulsions have been met with in exceptional cases of cerebellar tumour, and that convulsions have also been observed to affect the muscles of the limbs on the side of the cerebellar disease alone; but these attacks resemble tetanus seizures, as a rule, rather than convulsions such as result from discharges of cerebral origin, and in any case they are quite the exception. In regard to any localising value of convulsions when we are considering frontal as distinguished from optic thalamus tumours, all that can be said is that both general and Jacksonian fits may be met with in tumours in either situation, that both forms of convulsions are, however, more common in the case of frontal tumours, and that a Jacksonian fit makes the probabilities in favour of the frontal lobe still stronger.

Hemiplegia can only result in the case of frontal tumours either by pressure backwards on the Rolandic region or by direct extension of the growth to this part of the brain. In many cases, one or other of these modes is responsible for the presence of a variable degree of hemiplegia, in which, as a rule, the different parts of the affected side suffer in unequal degree, and in which the arm is usually earliest and most affected. This is not an invariable rule, however, in that I have met with at least one instance in which the leg was first to suffer where the tumour involved the superior

* 'Brain,' 1898, vol. xxi, p. 343.

† Of equal importance, and more nearly connected with our present subject, is the case published by Dr. James Collier ('Lancet,' March 25th, 1899), in which Broca's convolution was completely destroyed by a neoplasm in a right-handed young woman, who, at no time during her illness presented any aphasia.

frontal and marginal convolutions, and there is no reason why the face should not suffer first, as it indeed does in some cases. In optic thalamus growths it is common to meet with a certain degree of hemiplegia, and, as in the case of frontal tumours, the arm appears to be prone to suffer in greater degree than the leg; this was so in eight out of nine cases of tumour in this situation collected from recent literature by Dr. Michell Clarke.

Curiously enough, when any weakness occurs as a direct result of a lesion of the cerebellum in man, the hemiplegia is one in which the arm suffers in greater degree than the leg, a point which appears to be at variance with experimental results in quadrupeds, as in them the posterior extremity on the side of the lesion seems to suffer in greatest degree. Happily for diagnosis, hemiparesis is rare, as a result of cerebellar affections in men, except when we have the acuter processes, such as abscess, to deal with. It is quite exceptional to meet with loss of power in one arm and leg as a result of a tumour situated in one half of the cerebellum, though we must be prepared to meet with such a condition of things, as cases of the kind undoubtedly occur; and it is further necessary for me to remind you that when such hemiparesis obtains in cases of cerebellar tumour, as a direct result of the cerebellar defect itself, by which I mean not indirectly induced by pressure of the growth on the pyramidal tract in the pons or medulla oblongata, the loss of motor power is in the limbs on the same side of the body as the side of the cerebellum affected.

As regards hemiplegia, then, while it may result from a tumour in any of the three situations we are considering, it occurs with greatest frequency in optic thalamus cases, although frontal tumours are commonly the cause of such loss of motor power, whereas it is altogether exceptional to meet with such paresis where the tumour is situated in one half of the cerebellum.

Closely allied to the question of hemiplegia is that of *facial paralysis*. Now you are all, of course, aware that we have to deal with two types of facial paralysis as resulting from lesions of the nervous system, and that we speak of the one as cerebral and of the other as peripheral. By the first expression we mean a paralysis of the muscles of the face, which results from a lesion of the

centre for facial movements in the cerebral cortex, or of the path from this centre through the corona radiata, internal capsule, and crus, to the nucleus in the pons, from which the fibres of the facial nerve spring; while by the latter is meant a lesion which involves the nucleus of the facial nerve in the pons, or the emergent fibres of the nerve anywhere in their course from this collection of nerve-cells to the muscles which the nerve supplies. Now you further remember that the chief characteristics of the first type of affection of the face are that the muscles of the lower half are alone or chiefly affected, while the upper escape to a variable degree, and that emotional movements of the lower half of the face may be considerably preserved, although voluntary movements are very defective. In contradistinction, the points by which the peripheral type are recognised are by the fact that the upper suffers in equal degree with the lower half of the face, while there is no difference between the emotional and voluntary movements of the lower half, both suffering equally.

My reason for troubling you with these points of common knowledge is, because they are important preliminaries to what I have to say in regard to the value of facial paralysis as an indication of the probable seat of lesion in the group of cases we are considering. Either tumours situated in the frontal lobe, or those situated in the optic thalamus may be responsible for facial paralysis of cerebral type; *i.e.*, affecting the lower half of the face alone, or in greater degree than the upper, as a part of a hemiplegia produced by the effects of the new growth. In these cases, of course, the paralysis of the facial muscles is on the same side of the body as the paralysis of the muscles of the limbs. Tumours in the cerebellum never give rise to the type of facial paralysis which we have just said may be met with in the case of frontal and optic thalamus tumours; the hemiplegia, or more correctly the hemiparesis which may occur as a direct result of a cerebellar lesion, is one in which the limbs are affected, but in which the face takes no part, and we have already seen how exceptional it is to meet with such hemiparesis when the lesion of the cerebellum is a tumour. But tumours of the cerebellum may and do not unfrequently give rise to facial paralysis, this being, however, of the other type of which I spoke when prefacing the consideration of this part of my subject. It is then

the peripheral as opposed to the cerebral type of facial paralysis which we meet with as a result of tumours of the cerebellum, this result being brought about by direct pressure on the facial nerve between its emergence from the pons and its exit from the skull through the internal auditory meatus. At a recent discussion on the localisation of intra-cranial tumours which took place at the Neurological Society, and to which I have already referred, Dr. Beevor brought forward evidence to show that it is only those tumours of the cerebellum situated in the anterior part of the posterior fossa which give rise to facial paralysis in virtue of their close proximity to the facial nerve, while tumours situated in the cerebellar fossa are not attended with this result. It is well to remember that in cerebellar lesions where hemiparesis, the direct effect of such a lesion and facial paralysis, from pressure on the facial nerve occur together, both involve the muscles on the same side of the body, this being due to the fact that paralysis of the limbs as a direct result of a cerebellar lesion involves those on the same side as the lesion, and not those of the opposite side, as is the case in a hemiplegia of cerebral origin. The importance of this fact will become obvious to you when you recall another fact, viz., that a lesion of the pons may give rise to a facial paralysis similar in type to that met with in cerebellar cases, and that such a lesion may also give rise to concomitant hemiplegia, with this difference however, that in the case of such a pontine lesion the state of things met with is what we speak of as a crossed paralysis, that is one in which the facial muscles are paralysed on the side of the lesion, while it is the muscles of the opposite limbs that are at the same time affected. This is, of course, the case when the lesion involves the lower part of the pons where the facial nucleus is situated, whether that lesion be due to some process originating in the pons, or whether it be due to some morbid process such as a neoplasm commencing in the cerebellum and involving the pons secondarily.

To return to the point in which we are mainly interested in this lecture, we have seen that facial paralysis of cerebral type may be caused by tumours in the frontal lobe or in the optic thalamus, while tumours of the cerebellum are never responsible for this type of paralysis of the facial muscles, but may give rise to a paralysis of peri-

pheral type, in which the whole of the muscles on one side of the face are affected, and not only those of the lower half of one side, as is the case in the cerebral type of paralysis. Now are we in a position to say that just as the cerebellar tumours never give rise to facial paralysis other than of peripheral type, so frontal and optic thalamus tumours never give rise to facial paralysis other than that of cerebral type. The answer must, I think, be in the negative; for it is a curious and interesting fact, though one which time will not allow me to do more than mention incidentally in passing, that tumours in any part of the brain may give rise to paralysis or, more commonly, paresis of this or that cranial nerve, as a result apparently, of the deleterious action by general increase of intracranial pressure on the nerves. This being the case, both frontal and optic thalamus tumours may give rise to facial paresis of peripheral type, though the frequency or severity of such paralysis does not in any way approach that met with in cases where cerebellar tumours bring it about. Before leaving this part of our subject it is necessary for us to refer to a most interesting observation by Bruns, that in tumours of the optic thalamus there may be distinct impairment of the movements of the facial muscles on the same side during emotional movements, while they show no similar defect on voluntary effort, the very reverse of what obtains in the ordinary form of facial paralysis of cerebral origin, in which you remember that emotional movements of the face are commonly preserved in great degree in the presence of it may be very great defect on voluntary movement.

Discussion of the value of facial paralysis in the differential diagnosis of the cases under consideration, leads us to deal next with *defects of hearing* consequent on interference with the auditory nerve, in that this nerve is so intimately associated with the facial in its anatomical relations after they have emerged from the pons. While deafness to a variable degree may result in tumours of the frontal lobe or optic thalamus, as in tumours of any part of the intracranial cavity, as a result of the deleterious action of general increase of intracranial pressure of which we have already spoken, it occurs with greatest frequency and severity in cases of tumour of the cerebellum; and, as pointed out by Dr. Beevor in the discussion at the Neuro-

logical Society, this defect, like facial paralysis, only occurs when the tumour of the cerebellum is situated in the anterior part of the posterior fossa, and not when the new growth is in the so-called cerebellar fossa. As regards its significance in the group of cases which forms the subject of this lecture, nerve deafness is in favour of the seat of the neoplasm being the cerebellum, and when this symptom is present in conjunction with facial paralysis of peripheral type on the same side, there are no reasonable grounds for regarding the growth as situated in either of the two other situations with which we are concerned, viz. the frontal lobe or optic thalamus.

Does *optic neuritis* help us at all in the difficult problem of differential diagnosis which now occupies our attention? A larger experience has taught me to be less enthusiastic with regard to the degree of support to be expected from affection of the optic nerves, though I still consider that they afford us valuable information on the subject. Some years ago, like my colleague Dr. James Taylor, who brought forward the results of his investigations in an important paper communicated to the Ophthalmological Society, a paper which you will find in the 'Transactions of the Society,' 1894, vol. xiv, I was of opinion that in cases of cerebellar tumour optic neuritis usually occurs early, is severe, and is attended with marked swelling of the discs, a view which I still hold, and which I am glad to find supported by Mr. Marcus Gunn, as a result of his experience of the state of the optic nerves in a large number of cases of intracranial tumour. He sums up the matter as follows in the part he took in the discussion at the Neurological Society, "intense double optic neuritis, with much swelling and surrounding retinal change, coming on quickly suggest the cerebellum." An important fact, the outcome of an analysis of 601 cases of cerebral tumour, by Dr. J. M. Martin, is that a difference in the degree of neuritis on the two sides is more common in cerebral than in cerebellar tumours, so that neuritis limited to one side, or more severe on one side than on the other is in favour of the cerebrum rather than the cerebellum. When we come to inquire more closely into the state of things in regard to the regions of the cerebrum, with which we are specially concerned on the present occasion, we find that while optic neuritis may, of course,

occur in association with tumours in either situation, and while it may be equal or unequal in intensity on the two sides in either case, inequality is more common in frontal tumours, and that not only may there be inequality, but that the neuritis may be limited to one side; moreover, we find that the eye alone or most affected in cases of frontal tumour is more commonly that on the same side as the neoplasm than that on the opposite side.

In the absence of optic neuritis, we might reasonably expect to derive valuable information from the *visual fields*, for we naturally would not expect any alterations in the fields of vision either in frontal or cerebellar tumours, whereas, in virtue of its anatomical relations with the hinder part of the internal capsule, and the fibres entering that structure from the optic radiation, nothing would be more natural than that we should meet with hemianopia or blindness of one half of the retina, as a result of interference with the fibres conducting visual impressions to the half-vision centre in the occipital lobe on the same side. In our speculations with regard to frontal and cerebellar tumours we are not disappointed, for in neither do we find any alteration of the fields of vision in the absence of optic neuritis; but it is surprising to find that in recorded instances of optic thalamus tumours hemianopia is singularly rarely mentioned. This may, of course, be due to the fact that the amount of blindness due to optic neuritis has prevented the detection of blindness of one half of each retina as a result of interference with the conducting path to the half-vision centre in the occipital lobe. Even allowing for this possibility, however, one would have expected hemianopia to be more commonly detected, in that optic neuritis as a rule takes some time to lead to blindness, instead of which hemianopia, like hemianæsthesia is conspicuous by its absence in cases of tumour of the optic thalamus. In the course of these remarks on the fields of vision I have hinted that the presence of optic neuritis may vitiate our results in regard to the significance of limitation of the fields of vision in any direction. You must always bear it clearly in mind that limitations of the fields of vision may be the result of optic neuritis, and not hastily conclude that this or that defect present is consequent on a lesion of the visual centres themselves, or the

paths which connect them with the retina, without giving due weight to any part the optic neuritis present may play in this connection.

Anæsthesia.—We may most conveniently consider the question of the presence or absence of hemianæsthesia next, in that, like hemianopia, it may be produced by a lesion of the internal capsule, and might, therefore, be expected to yield valuable information in optic thalamus tumours. Although a certain amount of blunting of sensibility results from experimental ablation of the cerebellum, so far as I am aware this defect has never been met with as part of the clinical picture of a tumour of the cerebellum in man. So, too, in tumours limited to the frontal lobes of the cerebrum this symptom is absent, as you would naturally expect to be the case, in that this part of the brain is not itself concerned with the reception of sensory impressions derived from the cutaneous surface of our bodies. You are, no doubt, aware that there has been a good deal of difference of opinion as to the part of the cerebral cortex concerned with the reception of such sensory impulses, Dr. Ferrier having ascribed this function to the hippocampus major and hippocampal gyrus, while Professors Horsley and Schäfer found no justification for this belief, and regarded the gyrus fornicatus as the part concerned with the reception of ordinary cutaneous sensory impressions. In addition to this chief representation of ordinary sensory impressions in the cerebral cortex, the experimental observations of Dr. Mott and the clinical observations of Professor Horsley, based on the results of operations conducted on the so-called "motor" area of the human cortex, seemed to leave no room for doubting that some sensory representation exists in the Rolandic region also, so that this region is strictly "sensory-motor," and not purely motor, as some of us may have been led to believe.

Professor Schäfer * has, however, more recently arrived at conclusions which are at variance with those of Dr. Mott, and states that "tactile sensibility is not localised in the same part of the cortex from which voluntary motor impulses directly emanate."

It will be clear to you that all of these seats in which sensory processes have been supposed to be localised are too far removed from the optic thalamus

to allow of a tumour in the latter situation giving rise to any disturbances of sensibility through implication of either of the parts of the cortex concerned with sensory representation. But it will be none the less clear to you that a tumour of the optic thalamus may reasonably be expected to cause disturbance of ordinary cutaneous sensibility, through implication of the adjacent posterior limb of the internal capsule, either by the indirect effect of pressure or by direct infiltration of this part of the capsule by the growth; for you, of course, remember that the fibres of the internal capsule concerned with the conduction of ordinary sensory impulses are situated in the posterior part of the posterior limb of the capsule. As I have already said, however, such evidence of implication of the capsule, as shown by any blunting of cutaneous sensibility, is singularly infrequent; and this is the more surprising, in that in some of these cases in which cutaneous sensibility is preserved there is evidence of disturbance of motor processes, manifested by a variable degree of paralysis, no doubt consequent on pressure on, or implication otherwise of, the part of the internal capsule which is concerned with the conduction of motor impulses from the Rolandic region of the cortex cerebri to the periphery. As cutaneous sensibility is, however, blunted in some cases of tumour of the optic thalamus, the presence of this symptom would weigh in favour of the optic thalamus being the seat of the growth, where the other signs present caused us to be in doubt as to whether the tumour is to be located in this part, in the frontal lobe, or the cerebellum. Slight comparative anæsthesia, with erroneous localisation of sensory impressions, can alone result in cases of tumour of the frontal lobe, and that only when such tumours are sufficiently extensive as to interfere with the contiguous Rolandic region of the brain.

Tumours of the cerebellum are not uncommonly attended with *paresis or paralysis of ocular muscles*, a result that may be brought about by pressure on the nerves at the base of the brain, in which case one or other or both sixth nerves may suffer, not uncommonly the one on the side opposite to the seat of the tumour when this is unilateral. The third nerve is much less frequently affected in this way, though in common with other observers I have seen an external squint of the eye on the side of the cerebellar tumour. Another way in

* 'Journ. of Physiol.,' 1898, vol. xxiii, p. 310.

which cerebellar tumours may be responsible for paralysis of ocular muscles is by secondary implication of the pons, in which case the sixth nucleus is especially liable to suffer, the result of this being not only loss of the external rectus action on the side affected, but also defect of the internal rectus muscle of the opposite eye in so far as it is concerned in the conjugate movement of this eye in association with its fellow, the result of this being that both eyes are habitually turned away from the side of the lesion, and there is inability to voluntarily turn them to that side.

In that the ocular movements are represented in the cortex of the hinder part of the middle frontal convolution, it might at first sight appear likely that defects of ocular movements are likely to be particularly frequent in cases of tumours in this region of the brain; this cannot, however, be said to be the case. Except in so far as concerns such defects as may be the result of the deleterious effects of the general increase of intra-cranial pressure on the ocular nerves at the base of the brain, tumours of the frontal lobes are not specially prone to give rise to paralysis of eye movements. That such defects of ocular movements as occur are to be explained by pressure on the nerves at the base, is suggested by the fact that the defect most commonly met with is weakness of one or other, or of both external recti, muscles supplied by the sixth nerve which you will remember is specially prone to suffer from the effects of intra-cranial pressure owing to its small size and long intra-cranial course. In consequence of the weakness of the external recti, there is, of course, a varying degree of convergent squint met with, together with a variable degree of loss of power of movement of the globes outward. On the other hand, you will remember that the results of a sudden lesion in connection with the part of the cortex in which eye movements are represented, results in conjugate turning of the eyes to one or other side, that is either away from or towards the side of the lesion, the former resulting in the case of an irritative lesion of this part of the cortex, while the latter comes about when the lesion is sufficiently destructive to produce paralysis. The former of these conditions has been met with in connection with frontal tumour, the growth reaching the surface, and thus causing irritation of the cortex. That the latter

effect is not observed as a symptom of frontal tumours is not a matter for surprise when we remember how soon this paralytic defect is compensated for even in acute lesions which bring it about; this being so it is natural to suppose that the slowly produced damage consequent on the growth of a neoplasm may allow of compensation going on *pari passu* with any paralytic defect that is being produced. Certain other defects, such as slight impairment of upward movement and the like, met with in cases of frontal tumour, may possibly depend on interference with the cortical centres for eye movements, but we are not in a position to come to any definite conclusions on this point. In some cases of optic thalamus tumours, ptosis has been met with as a late symptom.

The Local Use of the Aqueous Extract of Supra-renal Glands of the Sheep in the Nose and Throat.—In the 'New York Med. Journ.' December 24th, 1898, Swain concludes a paper with the following words:

1. We have in the aqueous extract of supra-renal glands a powerful local vasoconstrictor agent and a contractor of erectile tissue, which it is safe to use in very considerable amounts without any dangerous or deleterious effects locally, or to the general constitution of the individual.

2. These local effects can be reproduced in the same individual apparently any number of times without entailing any vicious habit either to the tissue or to the individual.

3. The use of the extract seems to rather heighten the effects which may be expected from any given drug which may be locally used after it.

4. In acute congestions it has its widest application and greatest opportunity for good; but also in certain chronic conditions of hay-fever type, where œdematous tissue seems prone to develop, it can be relied upon as one of the most helpful adjuvants which we have at command. The only difficulty seems to be in producing it in quantities and in preventing decomposition on standing, which objection will be probably easily overcome by laboratory experiments.—*Therapeutic Gazette*, March, 1899.

PYORRHOEA ALVEOLARIS AND ITS RELATIONS TO GENERAL MEDICINE,

BY

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(Concluded from p. 399.)

As regards treatment, the first thing to do is to remove all deposits of tartar in the manner already described. There are, however, several points which must be kept in mind, if we would make an unqualified success.

1. Commence at one tooth, and remove every particle of tartar before proceeding to the next.

2. Whilst doing this, syringe every now and again with peroxide of hydrogen, of which the best form is pyrozone. The effervescence will not only mechanically bring to the surface the *débris* which you have removed, but will show the position of the pus pockets. Peroxide effervesces with pus.

3. In cases where it is specially difficult to remove all the particles of tartar, we may derive great assistance from the use of a 1 in 20 solution of sulphuric acid syringed into the pockets. Allow this to remain in a few minutes, and then neutralise with a solution of carbonate of soda introduced in the same manner.

4. It is a good practice, as a preliminary, to pack the pockets with cotton wool, wetted with a 3 per cent. solution of pyrozone containing a little cocaine, as this both dilates and blanches the pockets and renders the subsequent operation comparatively painless.

Having now removed all deposits of tartar, and scraped the edges of the alveoli (if we have found it necessary), we have several alternative methods of treating the pockets to secure healing, and the adhesion of the gum to the teeth. There are a good many remedies, and unfortunately too many people are trying to make reputations by proposing new ones. The following have stood the test of

practical experience, and are indicated under various conditions.

(a) The pockets may be simply syringed out twice a week with a strong solution of peroxide of hydrogen (pyrozone). The patient in the meantime uses a 3 per cent. solution as a mouth-wash.

(b) The pockets may be syringed out with a solution of peroxide of hydrogen containing 1 gr. of perchloride of mercury to the ounce. Express this, dry the pocket with hot air, and then introduce a 30 per cent. solution of chloride of zinc. Repeat these two applications every day for a week. You now direct the patient to attend every four days, and on each occasion syringe the pockets with peroxide as before; but instead of the chloride of zinc, introduce with a brush a few drops of the following mixture:

R. Ol. cinnamomi	3iv
Ol. gaultheriæ	3iv
Acid carbolic (crystals)	3j

Mix.

At the same time you give the patient a prescription for this same mixture, but diluted with an equal quantity of oil of lemons or parolein, to be applied to the gums daily with a brush, and order him to use as a mouth-wash simple cinnamon water.

(c) The application of lactic acid is especially advocated by Younger, who claims to cure by this means twenty-four out of twenty-five cases ('Dental Cosmos,' 1896, p. 676)! The acid is warmed slightly, and injected with a platinum-pointed syringe clear to the bottom of the pocket. It is not washed out, but allowed to remain there. The gum is protected during the application by a piece of cotton wool, smeared with vaseline. He affirms that one treatment for each pocket is usually sufficient.

(d) Another method is to introduce pure sulphuric acid into the pockets with a platinum wire. Only the smallest quantity must be taken on to the wire, and it must be carried by one movement right down to the bottom of each pocket. The pocket must be syringed immediately afterwards with a solution of bicarbonate of soda.

(e) Trichloroacetic acid, in a 20 per cent. solution, has been strongly advocated by some writers, who claim that it is especially indicated when the edges of the gums are tumified and everted.

(f) Sudduth recommends packing the pockets with the following mixture :

℞ Zinci sozoiodat.	...	gr. viij
Morph. acetat.	...	gr. ij
Lanolin.	...	3j

Mix.

(g) A composition in the form of a syrupy brown liquid was devised by Dr. Berlioz, and communicated to the French Academy of Medicine in 1893. It is applied to the pus pockets with a camel-hair brush, and very good results have been recorded from its use. He applied the name to it of "steresol," and it has the following composition :

℞ Purified gum lac...	...	270.0 grm.
Purified gum benzoin	...	10.0 "
Balsam of Tolu	...	10.0 "
Carbolic acid crystals	...	100.0 "
Chinese oil of cinnamon...	...	6.0 "
Saccharin...	...	6.0 "

Alcohol to make 1 litre. Mix.

(h) The pus pocket may be regarded as a fistula, and treated with the application of the galvanocautery. This very severe method of treatment would be of undoubted efficacy in cases which have resisted all other methods ; but personally I have never needed it, as I have been quite satisfied with the results obtained by milder measures.

(i) Another method of treating this stage of pyorrhœa is by means of cataphoresis. There can, I think, be no doubt that whatever applications give good results, when applied in the ordinary manner, will produce much better ones when applied cataphorically. The difficulty in treating pus pockets, is to cause the medicament used to penetrate deeply enough into the affected tissues to destroy all microbes. To do this in the ordinary way we are forced to use strong acids and caustic applications. By cataphoresis we can drive, by means of the electric current, a mild antiseptic into the gum, and at once destroy all germs, and stimulate the diseased tissues to a healthy action. The method of application is not difficult. The first thing is to apply a rubber dam or napkin to the mouth ; the next is to dry the pus pockets and root canals with alcohol ; cotton wool is then moistened with the fluid which we wish to apply, and packed round the teeth and into the pockets. A suitable platinum electrode is then placed upon the cotton, and the negative one upon some indifferent part of the

body. A current is then passed for a few minutes of such a strength as will be just perceptible to the patient. There is no pain in this application and very little discomfort. (Full directions for this method of treatment is given in the 'Dental Cosmos,' vol. xxxix, in different articles by various authorities.) Many drugs may with advantage be used by cataphoresis. Personally I use chiefly oil of cassia, watery solution of iodine, euthymol and iodine, and euthymol iodine mixed with sanitas oil.

Whilst the surgeon is treating the pockets during the patient's visits, the latter should sedulously carry out an auxiliary treatment at home which may be comprised in—

(1) *Mouth-baths.*—It is not sufficient, and this point should be strongly impressed upon the patient, *simply to rinse the mouth out with the solution used.* This should be retained at least five minutes in the mouth. In severe cases such a mouth-bath may be taken after each meal ; later on, night and morning. The following are eligible formulæ :

(a) Solution of carbolic acid 1 in 100.

(b) Solution of chloral hydrate 1 in 100.

(c) One of Dr. Seiller's tabloids (Parke, Davis and Co.), dissolved in 2 oz. of water. These admirable preparations contain in varying proportions the bicarbonate, chloride, benzoate, and salicylate of sodium, with the oils of thyme, menthol, eucalyptus and gaultheria.

(2) Daily massage of the gums with the fingers.

(3) The use of a suitable tooth-powder.

In all cases of pyorrhœa it is of the utmost importance to prescribe a tooth-powder for habitual use by the patient, which is especially adapted to the conditions obtaining in his mouth. If this be acid, which should be ascertained by litmus paper, an alkali should predominate in the powder.

Either of the following are good :

℞ Cretæ precip.	...	3iv
Sodæ bicarb.	...	3ss
Pulv. radidis iridis	...	3ss
Pulv. saponis alb.	...	3j
Acidi carbolic	...	℥xxx
Ol. eucalypt.	...	℥xxx
Ol. caryophilli	...	℥xx
Ol. limonis	...	℥x
Ol. menth. pip	...	℥xx
Ott. rosarum	...	℥vij

Mix.

R Saccharin	0 20 grm.
Thymol	0'1 "
Acid boric.	2'0 "
Salol	4'0 "
Pulv. saponis alb.	10'0 "
Calc. carb., magnes. carb.	āā	20'0	..	
Ol. menth. pip.	0'1 "

Mix.

Type 4.—In this group we find the same conditions as in the preceding type, but with the complication of loose teeth. The affected teeth project further out of the jaw than the sound ones, and appear to be forced partially out of their sockets. Such is really the case. The growth of inflammatory material which subsequently becomes converted into bone, gradually diminishes the depth of the sockets, with the result that the teeth are gradually extruded, being often partially rotated and distorted in the process.

The treatment of this form or rather stage of the disease can only be efficiently treated by an expert in dental surgery; but as the results obtained are little short of miraculous, it will be of interest to mention the means employed.

The treatment consists in extracting the affected teeth, opening up the pulps, and removing all pulp and nerve tissue from the pulp cavity and root canals, treating and filling these with an appropriate antiseptic material, filling the crown with a suitable stopping, sterilising the tooth, drilling out the socket with a proper instrument, replacing the tooth, and retaining it *in situ* by a specially prepared splint, until bony union has taken place between it and the alveolus.

Of course this procedure has been preceded by the treatment of the other affected teeth as in type 3. In cases where teeth are merely loose and do not project to any great extent, good results may be obtained by fixing them in position either by simply tying together with silk, or by the application of a proper splint.

Irregularities in the bite should be corrected. If any tooth strikes too hard, it is a good plan to put temporary caps on the adjacent ones on each side to relieve the pressure.

This replantation of teeth has been known to the dental surgeon for a very long time, and we really do not know who originated it. It is not used as much as it deserves to be, as it requires that the dentist should have considerable surgical

skill and knowledge. The important point to bear in mind in order to make a success is that everything must be absolutely aseptic. One of the best solutions to use is hydronaphthol, 1 in 300, although chinosol will probably replace it. The operation should be performed by the following steps:

1. Place the tooth in the antiseptic solution, and raise to 180° Fahr.
2. Take the tooth out of the fluid and wrap it in floss silk, to protect it during the further manipulations.
3. Remove the pulp, and fill the tooth in the usual manner.
4. Drill out the new socket for the tooth.
5. Place the tooth in position.
6. Take a plaster impression and make a metal splint to go over both surfaces of the tooth.

In the front teeth it is a good plan to insert a wire into the pulp cavity, the end of which projects a quarter of an inch from the lingual surface of the tooth. The edge of the splint can then be passed between this and the tooth, and will hold it firmly in position.

THE TREATMENT OF THE CONSTITUTIONAL STATE.

At the same time that local measures are being adopted to get the mouth into a healthy condition, we must employ constitutional treatment to remove any dyscrasia which may possibly cause the pyorrhoea. And it is here where the dentist often fails. Not having the technical knowledge to estimate the physical condition of the patient, he attempts repeatedly, persistently, but usually fruitlessly, to attack the disease by local measures. He scrapes away the accumulation of tartar, and when it reappears, as it shortly does,—*the cause being still there*,—he simply repeats the process.

It is necessary, therefore, to bear in mind that we must treat seriously any abnormal nutritive state or cachexia which may be present. As my readers are medical men, it will be quite unnecessary to do more than simply pass in review a few of the commonest of such conditions, and touch upon any point which can be elucidated from the standpoint of the dental surgeon.

The chief conditions which appear as factors in the production of pyorrhoea are:

1. *Affections or conditions which induce or perpetuate catarrh of the mouth or fauces.* By far the most frequent of these is chronic nasal

obstruction, due to swelling or hypertrophy of the mucous membrane covering the inferior turbinated bones. As all specialists in diseases of the nose will tell you, this condition is more often than not unrecognised by the practitioner, owing to the fact that the said obstruction only occurs in many cases during the night. In the daytime there may be little or no actual stenosis, and the surgeon on examining the nasal cavity with the speculum and frontal mirror, finds a fairly good passage. But at night, when the patient is asleep, the turbinates swell up, with the result that mouth breathing takes place until the patient awakes. The effect of this is to keep up a catarrhal condition of the pharynx, and secondarily of the oral cavity. The treatment, of course, I need not allude to.

(2) *Syphilis*.—In this affection it is possible for pyorrhœa to arise in two ways—from the cachexia produced in severe cases by the disease itself, and by the irritation attending the elimination of mercury and iodide of potassium from the gum margins. *It is, therefore, of the greatest importance to examine most thoroughly the mouth of the patient before commencing specific treatment.* Remove all tartar, treat all diseased roots, remove all roughnesses, and cure any pockets which may be present. It will be readily understood that the importance of this is not exaggerated when we bear in mind that we cannot carry on the specific treatment much beyond the point when gingivitis has appeared, without salivating the patient. It thus unfortunately happens that when we commence a course of mercury without having first set the mouth in order, we have to stop it just when it is commencing to do good. If we start the course with the mouth in proper condition, we can delay materially the advent of the specific effects upon the gums by directing the patient to use several times during the day an astringent mouth-wash. The one in use at Aachen, during the cure there, is substantially as follows :

R Aluminis 3j
Plumbi acetatis 3j

Dissolve the salts separately in 2 oz. of water, mix, and filter away the precipitated salts. To the filtrate add Aqua 3ij.

A favourite addition of mine to this is a small quantity of listerine or euthymol.

(3) *Gout*.—As we have seen, gout is probably a more frequent cause of true pyorrhœa than was

until quite recently suspected. Such being the case, we should in all cases try to determine whether it is present or not, as our treatment will be certainly modified by the conclusion which we arrive at. It would be out of place in this paper to discuss the signs of the gouty condition with which you are all familiar, I will only say that no examination of a case of pyorrhœa should be deemed complete without a laboratory examination of the blood and urine. If any signs are obtained which will justify us in suspecting gout, the patient, at the same time that local treatment to the mouth is being carried out, should be placed upon an appropriate diet, and the hygienic measures and medicines prescribed which are indicated. I will only add that, in my own practice, I am sure that I have observed that daily baths, sometimes containing carbonate of soda, moist friction of the skin, plenty of exercise and fresh air, with limitation of alcohol, sugar, and fat, have materially hastened the cure of many of my cases of pyorrhœa.

Administration of Potassium Iodide to Individuals with Sensitive Stomachs.—By Dr. Aldrich ('Clev. Med. Gaz.,' 1898, XIV, No. 1, p. 43).

The practical therapist rarely experiences trouble in administering iodide of potash to people with sensitive stomachs; but, unfortunately, such cases do occur, and with the very people where we desire an active and efficient iodine medication. Some of these stomachs seem so sensitive to the most minute dose of iodine that the practitioner is at his wits' end to both medicate and retain his patient. The writer has had a recent and trying experience with a man afflicted with spinal syphilis, who seemed unable to retain the smallest dose of iodine in an excessively irritable stomach. The conventional trial of the bitter tonics was a failure, as was the administration of the salt in a starch solution. The use of maltine with wine of pepsine was suggested and tried with the happiest result. In two other cases of non-specific disease where iodine was illy borne, the same preparation was used as a vehicle without gastric disturbance. — *Post-Graduate*, March, 1899.

DEMONSTRATION OF CASES

AT THE

NORTH-WEST LONDON CLINICAL SOCIETY.

Held at the North-West London Hospital,

Dr. MILSON in the Chair.

MR. J. G. TURNER showed a case of ununited fracture of the jaw. He said he had hoped to show two cases, one in which the fracture had united, and the other in which it had not. The interest was that in both of these cases there was great difficulty in obtaining union. The present case of fracture of the jaw remained ununited for nine months, in spite of most careful treatment by Hammond's wire splint, which kept the parts in very good apposition, but at the end of that period it was almost as movable as when first seen. He was debating whether to wire it or not, but he thought he would wait because the patient looked ill, and he feared suppuration. During the following week the patient developed a sore on the upper eyelid, which he (Mr. Turner) diagnosed as a tertiary syphilide, which spread down the side of the nose. Mr. Juler and Mr. John Griffith saw the case, and agreed with the diagnosis. The patient was then put upon iodide of potassium and mercury, and in a month the eyelids were perfectly well and the jaw had healed, in spite of the fact that the Hammond's splint had been left off, and there was nothing but an ordinary four-tailed bandage used. He now had a perfectly united jaw. The other case, not shewn, was that of a man who came for a large swelling on the right side of his face. He said he had had the toothache for six weeks. The teeth were extracted, and the swelling went down, but four weeks afterwards pain and swelling again came on. On examination, Mr. Turner found an ununited fracture, of which the patient knew nothing. He could give no history except that six years before he had had a very serious "bash up," which he did not enlarge upon, and a year before had fallen upon a brick. Bearing in mind the case now before the Society, he inquired for syphilis, and got a history of a

hard sore and two months' treatment. He treated him with anti-syphilitic remedies, and the swelling cleared up. He got fibrous union but not bony union. These two cases showed the importance of treating general diseases when difficulty in obtaining union of a fracture was experienced. Indeed, in any intractable local condition, the possibility of the influence of other diseases should be thought of.

MR. JACKSON CLARKE said the cases enforced a very valuable lesson. He had showed a young man before the Society some time ago who had had a large peri-urethral abscess, which he had opened, and performed an operation for stricture. He found that there was a large peri-urethral abscess, and that the urethra was practically constricted from the meatus up to an inch from the posterior end of the bulbous part, and there the urethra was destroyed. However, now, some six months after operation, the patient could pass, twice a week, a thick, No. 14 size, soft catheter for himself. He had had no trouble with the urethral part, but had lately had a sore on the side of his leg, which was evidently a tertiary syphilide. This event caused Mr. Jackson Clarke to think that part of the urethral changes might have been due to syphilis. In obstinate cases it would be quite easy to give a very thorough anti-syphilitic course, as Mr. Turner suggested.

DR. GUTHRIE showed a case of progressive muscular atrophy, the patient being a man aged fifty-four, by trade a carpenter. He was quite well until six years ago, and then suffered from what was called some form of blood-poisoning. There was general weakness, but he had no pain. This weakness was followed by gradual loss of power in the wrist, then in the muscles of the thumb, then of the forearm, first of the left arm and then of the right. Within the last three months the right arm had begun to get worse while the left improved. Present condition: Face quite unaffected. The trapezii and sterno-mastoids unaffected, also the muscles of the neck, the levatores anguli scapulæ and the rhomboids and the supra-spinatus on both sides. The deltoid muscles and also the infra-spinati were markedly wasted. The biceps muscle on each side was reduced to a very fine strand indeed; the triceps could be felt, but it was also

wasted. He had now some use in his latissimi dorsi, which was a point in which improvement had taken place, as he could do nothing with them a month ago, the arms hanging like flails. His biceps was now so wasted that he could not keep his arm flexed at all. The muscles of the forearm generally were somewhat wasted, but not so markedly as those of the upper arm. As regards the hands, there was some flexor contraction of the fingers, though not very marked; there was well-marked wasting of the thenar eminence and of the hypothenar; he had no power in the opponens pollicis on the right side, but a good deal on the left. There was no affection of the trunk muscles, and the diaphragm was unaffected, which was a fortunate circumstance in reference to the respiration. He had some wasting of the extensors of the thighs, but his knee-jerks were present in normal degree. He had some difficulty in getting up from the sitting posture, especially if the chair was low. His gait was awkward and a little spastic. He also had the condition of hammer-toe in both great toes. Dr. Guthrie went on to differentiate the condition from myopathy or idiopathic muscular atrophy, and chronic spinal meningitis. There were two forms of progressive muscular atrophy, tonic and atonic; in this case the atonic prevailed. The chief reason for which he brought the patient forward was to discuss treatment. The patient would tell them that he had improved a great deal in the past month. When first seen he had practically no use in the arms or hands; they hung like flails, but he could now execute some movement with them. The treatment adopted consisted in massage and electricity, with injections of strychnia. Massage could never do any harm, but he offered a word of caution with regard to electricity. It was obvious that, if the muscles were much wasted and the patient run down, much good could not be done by using powerful currents, but, on the other hand, much harm might result. Moreover, patients complained bitterly of the pain caused by using strong currents. Another point was that in such cases, if the patients got worse, they were apt to attribute their deterioration to the use of strong currents of electricity. Mild currents, on the contrary, were efficacious. The patient had been having strychnia injections every other day,

beginning with 3 minims, and was now taking 9 minims. Probably he would take 18 minims before leaving off. He thought it was a mistake to speak of the disease as progressive muscular atrophy, because it was not necessarily progressive; it had a tendency to become stationary. Therefore such cases were not so hopeless as they were formerly thought to be. He thought the present patient would make some improvement, though, of course, he would not recover completely.

In reply to questions, Dr. Guthrie said that he used both galvanic and faradic currents.

(To be continued.)

Head-nodding and Head-rotation usually associated with Nystagmus in very young Children.—Charles J. Aldrich ('Amer. Journ. of Med. Sci.') prefers the above title of a more correct description of a peculiar syndrome of the pathological basis of which we are at present ignorant. The disorder usually occurs between ages of six and twelve months, and the records show a great preponderance of females attacked. Gyral movements are most frequent, although a combination or alternation of the nodding with gyral movements is common. The movements are smooth, easy, regular, and rhythmical, and may stop during sleep, in the recumbent position, and (Mills) when the eyes are covered. The nystagmus may be monocular or binocular, vertical, horizontal, rotary, or mixed and alternating—one eye presenting one variety, its fellow another. The nystagmus usually precedes the development of the head movements, and often persists long after the latter have disappeared, and may recur without their association. Strabismus may be associated with the disorder. Characteristic changes in the pupil and optic discs fail to occur as a rule, nor is the vision necessarily at fault. The etiology is obscure. Most cases tend to spontaneous cure in six weeks to three months, but many cases have a tendency to relapse if the child suffer any disturbance in health. The treatment consists in nerve rest by bromides, rest in the recumbent position, tonics, correction of digestive and other disturbances.—*American Journal of Obstetrics*, March, 1899.

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A CLINICAL LECTURE

ON

DEFORMITIES FOLLOWING INFANTILE PARALYSIS.

Delivered at St. Bartholomew's Hospital, Nov. 30th, 1898,

By W. J. WALSHAM, F.R.C.S.Eng., M.B., C.M.Aberd.,

Surgeon and Lecturer on Surgery, formerly Demonstrator of Orthopædic Surgery, to the Hospital.

GENTLEMEN,—It is always a difficult matter to choose a subject for a clinical lecture. Although I have had a number of interesting cases in my wards since I have been surgeon, still so much is written nowadays that it is not easy to find anything that is new or original to say to you, so I have thought it best to fall back on past experience, and take for my subject, "Deformities following infantile paralysis." I have seen a very large number of these cases during the seventeen years I have been connected with the orthopædic department, and have been able to bring an interesting series of my old patients here for you to inspect for yourselves. To take ten years out of the period I have named, I find that from 1884 to 1895 I have had some 308 cases in the orthopædic department, besides those I have seen in private. So, having had considerable experience in the treatment of these deformities, I thought I could not do better than say something to you about this subject to-day.

Now, I need hardly tell you that infantile paralysis depends upon the destruction of the anterior multipolar nerve-cells in the spinal cord. You are aware that in the anterior horns there are a number of large cells, and it is upon the destruction of these cells that so-called infantile paralysis depends. The term infantile paralysis has been objected to because, although this form of paralysis nearly always occurs in the infant or in the young child, it has been known to occur, though rarely, in adults. On that account it has

been proposed to call it acute spinal paralysis, or acute cornual paralysis, or acute poliomyelitis. I object to all these terms because they are based on purely pathological considerations, and authorities are not altogether agreed as to what the pathological lesion actually is which leads to this wasting and paralysis. I therefore prefer to retain the name infantile paralysis as the best known, and as not committing ourselves to any particular theory.

All I have to say about the pathology of the affection here is that there are several views as to its production. One is that it is an anterior poliomyelitis, that is, an inflammation confined to the region of the large multipolar nerve-cells which you find in the anterior cornua, and which preside over the nutrition and function of the muscles. That the lesion may often be an inflammation has been proved by a few post-mortem examinations, though I have never been so fortunate—or, I suppose I ought to say, unfortunate—as to have a post-mortem examination on one of these cases myself. The condition that has been met with in these examinations has been redness at the seat of the lesion, dilatation of the capillaries and small arterioles, infiltration of the tissues with small round cells, and swelling and cloudiness of the large so-called multipolar nerve-cells. So that undoubtedly in some cases the lesion is a poliomyelitis. In many cases, moreover, the symptoms attending the onset of the disease lend support to the inflammatory view; thus I find that in 39 of my 308 cases there was clearly a preceding feverish attack, either scarlet-fever, measles, whooping-cough, or some other febrile disturbance, the exact nature of which we could not get at. But there are some cases the history of which would seem to negative an inflammatory origin. In six cases the condition came on after a convulsive fit, the child having previously been quite well. In fifteen of the cases the child was said to have gone to bed well, and to have awakened next morning with paralysis. In twenty-seven cases there was nothing observed the matter with the child till it began to walk. It appears, therefore, that the paralysis comes on sometimes quite acutely. On this account it has been said the lesion is probably not a poliomyelitis at all, but that it is more likely the result of embolism and consequent

softening. Then, again, in a number of cases the paralysis is said to come on immediately after a fall or some other local injury to the affected limb, and has hence been regarded as due to a peripheral lesion, such as the tearing or bruising of the nerve supplying the affected muscles. There is yet another theory, perhaps a more modern theory still, although it has not been proved, namely, that it is the result of some acute bacillary infection.

There is one other interesting point in the pathology of infantile paralysis that I should like to refer to, namely, the tendency that there is for the paralysis to clear up. The patient comes, perhaps, with the whole limb paralysed; but you can pretty truthfully tell the friends that it is most likely improvement will occur. One reason assigned for this is that, whilst at the focus of the inflammation or softening, or of the bacillary infection, if such it be, some of the large nerve-cells are destroyed, and the muscular fibres or muscles presided over by them ultimately undergo fatty degeneration, other of the cells are merely pressed upon by the inflammatory products, and as these clear up, the cells, not having been destroyed but only temporarily damaged, resume their rôle, and the muscles corresponding to them gradually recover. This is the clinical history you will find over and over again. Thus you may be told that one arm and one leg were at first completely paralysed, but that after a time the arm got better, perhaps quite well, whilst the leg remained more or less affected. Or you will find that, although the patient at first could not move the affected leg at all, as time went on very considerable power was regained, and only a few muscles remained paralysed.

Here are some beautiful specimens which I have had brought down from our Museum, the better to show you the condition of the muscles. You will see, even at a distance, that the affected muscles have lost their reddish colour, and appear pale, yellow, and fatty-looking. In this particular specimen, the gastrocnemius and soleus are completely reduced to fat, and the anterior group of muscles and the peronei have undergone marked atrophic and degenerative changes.

The general signs of infantile paralysis are well seen in this girl. The affected limb, you will notice, is bluish, congested, and mottled in ap-

pearance, and when I make pressure with my finger, the whiteness produced by the depression lasts some time, that is to say, the circulation through the part is restored very slowly. Her foot feels quite cold to my hand, and although the limb, when compared with that of the opposite side, is markedly wasted and atrophied, only certain of the muscles are affected. Thus you see that, although she cannot extend her leg at the knee, she is able to some extent to move her toes, and the gastrocnemius and soleus retain a considerable amount of power, as shown by the way in which her foot is held by them in the equinus position. She feels, you notice, when I touch her, so there is no loss of sensation, and there is no post-rigidity like you get in hemiplegia.

Next, as to the diagnosis of infantile paralysis generally. There are one or two affections for which you might mistake it, not so much, perhaps, in hospital practice as in private, because patients brought to the hospital have generally well-marked signs. In private, however, cases of spastic paralysis, rickets, pseudo-hypertrophic paralysis, and congenital syphilis have been sent to me as cases of infantile paralysis. But a little care will enable you to distinguish them. In infantile paralysis the most striking sign is the coldness of the limb. The limb feels cold even when no blueness or congestion is apparent. This girl is suffering from spastic paralysis of the lower limbs and right upper arm. I ask her to close and open her hand. As she opens it the wrist becomes flexed, whilst the fingers are hyper-extended, and the whole of the movements are performed in a stiff and spastic way. Again, I take one foot in my hand and try to dorsal flex it. At first it resists, and seems firmly held in the equinus position, but by continuing the pressure it gradually yields, and I at length succeed in placing it in full dorsal flexion. Her knees, you will observe, appear to be contracted and fixed in a semi-flexed position, but the deformity yields to steady pressure on the knee. When I take her legs and try to separate them, I find they are strongly held together by the adductor muscles, but by continuing the force the spastically contracted muscles slowly yield as did those of the foot and knee. This condition of the adductor muscles of the thigh is exceedingly common in spastic paralysis. Moreover, in

spastic paralysis there is generally some mental deficiency, and often defective speech and dribbling of saliva.

In some of the bone affections of congenital syphilis, and in certain cases of rickets, movement of the joints causes a considerable amount of pain. The child therefore resists motion, and holds his legs still, so that he might, to the superficial observer, appear paralytic. Both conditions, however, are so unlike infantile paralysis in other respects, that a little care and attention will easily enable you to distinguish between them.

In pseudo-hypertrophic paralysis the heels are often drawn up, and the feet held in the position of equinus, whilst in the later stages various contractions and distortions may occur. In the hypertrophic stage, the enlarged calves and glutei, the wasting of the lower part of the pectoralis major, the waddling gait, the lordosis, the absence of patellar reflex, the way in which the child stands with the feet far apart, and the peculiar manner in which he rises from the recumbent position are characteristic, and if a small piece of the apparently hypertrophied muscle is removed by a myotome, the pathological changes characteristic of pseudo-hypertrophic paralysis will be observed under the microscope.

Next, as to prognosis. If you get a case early the prognosis is good, and you may tell the friends, who generally come in a state of great concern, and ask whether the child will have another fit, and will he get worse, that the condition will improve. But if the patient is brought with a severe deformity, your answer must be a little different. You will then say that the deformity can be cured but that there will probably be a relapse, occurring more often in bad cases because of the number of muscles paralysed. Therefore the prognosis as regards life and improvement of paralysis is good. As regards permanent cure of the deformity, except in very slight cases, the forecast is bad.

As to the general treatment of infantile paralysis in the early stage I shall say nothing, as this falls under the province of the physician rather than the surgeon. When, however, paralysis has shown itself, the means at our disposal may be said to be three-fold, namely, warmth, massage and electricity, and passive and active

exercises. Warmth is essential; thus if the lower limb is affected, two pairs of thick woollen stockings should be worn, and a good cloth gaiter during outdoor exercise. A lamb's-wool stocking should envelope the limb whilst the child is in bed. The massage and electricity should be continued for long periods, say for two years or more. Short periods, as from three to six months, are practically useless. I have seen some very striking results from electrical treatment thus persevered in, and marked improvement in what appeared almost hopeless cases. Under the massage, the circulation in the limb and condition of the affected muscles are markedly improved.

Passive and active exercises for the prevention of deformities cannot be too strongly insisted on. The kind of exercises employed will, of course, depend on the particular muscles that are affected.

I now pass on to the subject proper of the lecture—the deformities that may result from the paralysis. Out of the 308 cases I have previously mentioned, I find there were 147 of equinus, 42 of right-angled contraction, 32 of varus, 32 of pure valgus, 26 of talipes cavus, and 24 of calcaneus. Seven were mixed cases. Equinus, you will therefore see, is by far the most common deformity as the result of infantile paralysis.

I show you here a very good case of almost pure equinus, but it is now partially cured. We rarely get a case in which there is absolutely nothing but equinus. There is nearly always some varus as well. Perhaps the reason for this is that the foot has a natural tendency, when the muscles are relaxed, in consequence of the peculiar arrangement of the ankle and sub-astragaloid joints, to drop into the equinus, with slight varus, position. You can notice this as I sit on the edge of a table, and let my leg hang loosely down. Although I speak of the deformity as equinus, you are not to understand by this there is no varus, but that the varus is so slight that it is little more than the natural tendency for the foot to fall inwards. The condition of equinus, with slight varus, should be distinguished from that known as equino-varus, in which the varus is marked.

What, then, is the condition in equinus? The heel is drawn up, the tendo Achillis stands out very tense, and if you look at the front of the foot

you will notice, first of all, a slight sulcus or groove just below the leg bones, and then a prominence caused by the astragalus, and then another slight groove where the astragalus joins the scaphoid; thence the metatarsal bones run in a downward and forward direction to the toes, which are bent at their metatarso-phalangeal joints at right angles to the metatarsus. A certain amount of talipes cavus is nearly always present, that is to say, a hollowing of the sole of the foot. What is the cause of this cavus in equinus? In the first place, when we stand on tiptoe we put in action the gastrocnemius and soleus. When these muscles act, the deep flexor muscles, viz. the flexor longus hallucis, and flexor longus digitorum act at the same time. The tendons of these muscles, after running round the inner ankle, pass across the sole of the foot, and when the muscles contract, act, as Mr. Ellis puts it, after the manner of a bow-string, and thus tend to increase the height of the arch. You can see this for yourself when you stand on tiptoe. In equinus the gastrocnemius and soleus are permanently contracted, causing, so to speak, a tiptoe position, and the deep muscles, continuing to follow the lead of their more powerful neighbours, also become contracted and shortened, and thus deepen the arch. A second cause of the cavus in conjunction with equinus is, no doubt, the tendency of the weight of the body, when the foot is in the plantar flexed position, to deepen the posterior segment of the arch, and, as a consequence, to approximate its piers. Thirdly, the cavus having been produced in this way, it is maintained in part by the adaptive contraction of the plantar fascia and the short sole muscles and tarsal ligaments, and in part, later, when the deformity has been allowed to remain untreated for long periods, by alteration in the shape of the bones themselves, the bones, as you see in this specimen, becoming still more wedge-shaped than usual, thus producing a narrower but higher bridge. When this has occurred, the cavus cannot be overcome except by long-continued mechanical pressure, or, failing this, by some operation on the tarsal bones themselves.

After this digression let us consider the ætiology of the equinus itself. The immediate causation is paralysis of the anterior groups of muscles. In a

well-marked case there is paralysis, more or less extensive, of the tibialis anticus and extensor longus digitorum, extensor longus hallucis, and peroneus tertius. But why does the foot take the equinus position when the anterior muscles are paralysed? There are two theories to account for this. The first is that the gastrocnemius and soleus, being unopposed by the paralysed anterior muscles, draw the foot into the plantar flexed position. That may be called the unopposed action view. It is not the one now usually accepted. The second and more probable reason is that, when the anterior muscles are paralysed, the foot, which has a natural tendency to drop into an equinus position by its own weight, does so unchecked, and the posterior muscles become adaptively shortened and contracted in the same way as the hamstring muscles undergo adaptive shortening, as in the case of tuberculous disease of the knee for example, when the joint has been allowed to remain in the flexed position.

But there are different degrees of equinus depending upon the extent of the paralysis of the anterior muscles. I show you here several patients with the common form. But if you look at this case you will see a very different state of things. Instead of the toes standing out at an angle with the foot at the metatarso-phalangeal joint, they are doubled under, so that the patient walks on their dorsal aspects. This is due to the more or less complete paralysis of the anterior muscles, especially of the extensor longus digitorum and extensor longus hallucis. You will notice the corns over the dorsum of the toes, and the extreme disability and lameness with which the patient hobbles about. In some cases intractable ulcers form, and the condition becomes pitiable indeed. You might ask why we do not always get this condition. The reason is because some of the anterior muscles, or parts of these muscles, escape the paralysis, and at the same time, as the foot becomes fixed by the contraction and shortening of the posterior muscles they are put on the stretch, that is, the distance between their insertions is lengthened, and, consequently, as they contract they draw back the toes, and thus prevent them from doubling under. The condition of the toes, therefore, is at once an indication of the extent of the amenability or seriousness of the case. In this bottle is a specimen of a still more

severe degree of the deformity; indeed, so severe that amputation was necessitated. All the anterior muscles, you will observe, are converted into fat, and the foot is completely doubled under, so that the toes are nearly in contact with the heel, and the patient hobbles about on the dorsum of the foot, over which a large bursa and painful corns have formed. There is yet another condition in which all the muscles of the leg below the knee, both back and front, are paralysed. Here, although the foot hangs helplessly in the equinus position, it is not held thus, but only falls into plantar flexion by its own weight. When I take hold of the foot in this patient, you will notice that it is flail-like, that I can put it in any position, but that as soon as I leave go of it it again falls into that of the plantar flexion.

In contra-distinction to these severe grades of equinus, I now show you a patient with what may be called the slightest form of equinus; indeed, there is no equinus at all in the sense of the foot being plantar flexed. But the foot, as you see, cannot be dorsal flexed. It cannot be flexed beyond a right angle with the leg. This slight degree of equinus is known by the name of rectangular talipes, or right-angled contraction of the tendo Achillis. It depends upon very slight paralysis of some portions of some of the anterior muscles. Indeed, beyond a slight diminution in the size of the leg when compared with the opposite side, there may be little or no evidence of paralysis at all, the electrical tests showing the muscles weak, but otherwise normal. It would seem that in these cases the lesion in the cord has almost completely cleared up, leaving merely a few muscular fibres permanently affected.

Next, as regards the condition of the bones in equinus. I am fortunate in being able to demonstrate this in a beautiful specimen which was given me some twenty-five years ago. With its help, let us first look at the alteration in the foot as a whole, and then at the alteration which has taken place in the individual bones. The foot is in the plantar flexed position; it is small, and somewhat widened anteriorly in consequence of the patient having walked on the toes. The only two bones that are materially altered are the os calcis and the astragalus. The os calcis is drawn up so that the tuberosity is raised, and the long axis of the bone runs forwards and downwards. The

posterior superior facet is prolonged backwards, and articulates with the tibia. The astragalus is tilted out of its socket, and the cartilaginous surface of the anterior part of the trochlear facet has disappeared, while posteriorly the facet is prolonged beyond the bone on to the facet on the os calcis, which articulated with the tibia. The cuboid and cuneiform bones are more wedge-shaped than normal in their antero-posterior axes, and so the metatarsal bones have the surface of their heads prolonged on their dorsal aspects. The foot is in the plantar flexed position, and in this position it has become fixed by the contraction of the posterior ligaments, and, as a consequence, the articular surfaces which are unopposed have to a great extent lost their cartilages. The anterior ligaments are stretched and elongated, the anterior part of the foot is bent downwards at the transverse tarsal point producing the cavus, and in this state is firmly held by the adaptive contraction of the plantar ligaments. The plantar fascia with the muscles has been removed, the better to show the bones.

As I have already said, equinus is usually combined with a slight degree of varus. Sometimes, moreover, the varus becomes a more predominant feature, and the condition may be truly spoken of as equino-varus. The muscles then in addition at fault are the peronei. In other cases there is some amount of valgus due to the involvement of the tibial muscles.

In many cases of equinus, equino-varus, and equino-valgus of paralytic origin, other muscles in addition to those acting on the foot may be affected. Thus the quadriceps extensor may be paralysed when, as in the patient I now show you, the leg cannot be extended on the thigh. In other cases, again, the pelvo-femoral muscles may be implicated, and the patient may be unable to either flex or rotate the limb upon the abdomen; whilst in another case, again, the whole of the limb may be involved, and the leg simply hangs on the trunk as a helpless and useless appendage. To determine the condition of the quadriceps, you ask the patient to straighten his leg; you find he is either unable to straighten it at all, or else that the muscles may be so weak that he is unable to do so if you make the least resistance on the leg with your hand. You must not be deceived by his pulling up his thigh by

his pelvo-femoral muscles, and swinging the leg upwards with his hand, as you see this boy do when I ask him to give me his foot, nor by his being able to walk, which patients often can do when the leg has been placed in the extended position; indeed, in some cases, it is astonishing to find how well the patient can walk, even when the paralysis is severe, by placing his hand upon the lower part of his thigh, and thus forcing the knee into the extended position, and holding it so during progression. When the paralysis of the quadriceps is very slight, it may be necessary to apply electrical tests to determine what parts are actually affected.

Paralysis of the pelvo-femoral muscles, when the rotators alone are involved, may escape our notice. You might be a little puzzled at first to account for the inversion or eversion of the foot, as the case may be, when the child walks. To appreciate the amount of paralysis, first lay hold of one leg, and then of the other, and rotate in and out, as I do now. The want of tone and loss of resistance in rotator paralysis, when compared with the opposite side, is most marked, even in slight degrees. In extreme degrees, as you see here, the limb rolls from side to side as if nothing was holding the femur in its socket. In this particular patient the limb rotates through so wide a circle that the toes can be made to point directly backwards. It is important to estimate the condition of the quadriceps and pelvo-femoral muscles, and the extent to which they are affected by the paralysis, as it has a direct bearing both on the prognosis and treatment.

I have already called attention to the diagnosis of the deformities due to infantile paralysis from those due to spastic paralysis. Let me now say a few words on the diagnosis of the paralytic from the hysterical equinus. I have here the cast of a foot of a girl who had well-marked equino-varus, and I have seen during my hospital career some five or six other cases of this condition. The foot was held as here shown in the cast, rigidly in the equino-varus position, and the friends declared that it was always held so. In the girl from whom the cast was taken, the calf on the affected side was slightly smaller in circumference than that on the opposite side, and the limb was slightly blue and congested, but so was the other one. At first sight, therefore, the condition re-

sembles the paralytic. On taking hold of the foot, however, and making pressure upon it, as if to correct it, the muscles are felt to act somewhat spasmodically, and at length to yield; the foot will then, as a rule, be completely restored. At the moment of yielding a certain amount of ankle clonus is set up. The condition of non-persistent clonus at the moment of yielding is very characteristic of the hysterical form of the affection. Further, under the influence of electrical stimulation, the contracted muscles will momentarily yield, and the foot will be drawn into a dorsal flexed position. An anæsthetic will, as a rule, show the absence of any organic contraction in the affected tendons. I say as a rule, because I know of cases where, in consequence of the foot having been held in the deformed position for a long period, the muscles had undergone adaptive shortening, and complete restoration was not possible without tenotomy.

Another condition for which the paralytic equinus might possibly be mistaken is the equinus which is at times met with as the result of tuberculous disease of the ankle or tarsus. A little care and attention should discover the signs of the tuberculous affection, such as pain on pressure or handling, slight swelling and increase of heat, and hence that the wasting and deformity are due to disease of the ankle, and not to paralysis.

Again, paralytic equinus may sometimes be simulated by the condition known as "position equinus." When a foot has been held or kept in a plantar flexed position for any length of time, as by the pressure of the bedclothes in the case of fracture or Pott's disease, where no cradle is used, the foot may become fixed in this position by the adaptive shortening of the calf muscles, and, as in such a case there is some wasting of the anterior muscles, you might attribute the deformity to paralysis. The history of the case, however, should prevent you falling into any error. But do not forget that this form of equinus may really be due to paralysis. I have seen several cases where, after Macewen's osteotomy, the anterior muscles have been paralysed from pressure or stretching of the peroneal nerve as it winds round the end of the fibula. Sensation in such a case, however, in the parts of the skin supplied by the nerve will also be impaired or lost, and the paralysis, as a rule, will pass off with time.

I have already briefly referred to the general treatment of infantile paralysis. We will now consider in detail the treatment that may be required for the various deformities resulting from it. The treatment will necessarily vary with the age of the patient, the condition of the affected muscles, the amount of shortening of the limb, the state of the thigh and pelvo-femoral muscles, the condition of the joints and of the opposite limb, the presence or absence of trophic changes in the skin, or of corns, bursæ, or ulcers; all these matters have to be taken into account. The treatment may be divided into the operative, the manipulative or physiological, and the mechanical.

I will say a word or two about each of these in general, and then refer more particularly to the special requirements for the individual deformities:—

1. *Operative treatment.*—The following operations may be required: Division of the tendo Achillis, division of the plantar fascia, transplantation of tendons, removal of bones or portions of bones, arthrodesis or fixation of the joint, and, in several cases, amputation.

There is not much that I need say about division of the tendo Achillis. It should, of course, be done subcutaneously. The puncture is best made on the inner side of the tendon, since the posterior tibial artery runs behind the inner ankle, and might be pricked if the knife were introduced from the outer side. The best position for division is at the thinnest part of the tendon, just above its insertion into the os calcis. You are told in the books that the foot should be relaxed when you are passing the tenotome. Personally, I object to it being relaxed, because whilst it is in a state of contraction you can feel the tendon more easily. In a child, I hold the foot with one hand, and put in the tenotome with the other, and thus have the tendon more under my own control. In an adult, if you have a cautious assistant, there need be no danger of cutting through the skin, although he keeps the tendon tense the whole time. The skin, should, of course, be carefully cleansed and rendered aseptic, as for all operations. Very rarely, indeed, does anything go wrong in these cases. Out of the many hundreds I have done myself, or superintended when done by my clinical assistants or dressers, I have only seen two suppurate;

one when I was first appointed to the orthopædic department, the other not till a few months ago. I think the cause of the trouble was that a little too much pressure was used in applying the plaster bandage. After the tendo Achillis is divided, the foot should be rectified as much as possible, and fixed by a plaster bandage applied over a thick bandage of cotton wool. You need not mind how far the two ends of the tendon are apart. Sometimes there has been as much as two inches in adults between the ends of the tendon, yet the gap has become filled up by fibrous tissue, and finally good strong tendon was formed. The foot and ankle should be kept in the plaster case for ten days or so; the bandage may then be taken off and reapplied for another week or two, and after that the instrumental treatment begun.

The plantar fascia should not be divided, as is generally taught, at its most prominent part, because it has there a very thin covering of skin, which will, after division, become adherent to the fascia, and you will have a painful cicatrix for many months. I have known a case in which the scar remained painful for twelve months. On that account I have for many years given up dividing the fascia in this situation, but instead have divided it far back where there is a thick covering of granular fat between it and the skin. I make my puncture superficial to the fascia, turn the knife to the fascia, and cut towards the bone, when it yields readily enough. If other bands are left, they are divided afterwards. After the operation the foot should be stretched out, and in this position fixed in a plaster bandage. If the patient can afford it, you may, in place of the plaster, apply a splint with a jointed sole-piece acted upon by a cogwheel, and then gradually, day by day, screw it up into a better position.

The transplantation of a tendon of a sound muscle into the tendon of a paralysed one, known as Nicoladoni's operation, after the inventor, is perhaps best done by making a hole in the tendon of the paralysed muscle, then severing the tendon of the sound muscle, and passing its proximal end through the hole and fixing it by four sutures.

Arthrodesis consists in shaving off the cartilage from the bones forming the ankle or knee-joint for the purpose of procuring bony ankylosis, and so obtaining a fixed, in place of a flail-like, joint.

I have here nothing to say about the removal of

bones or portions of bones, nor need I detain you with any remarks on amputation, which severe measure should only be resorted to in extreme and intractable cases.

2. *Manipulative or physiological treatment* should always be employed whether operative or mechanical treatment or both have been necessary or not. It consists in shampooing, massage, electricity, and a course of passive and active exercises. It has for its object the strengthening and bringing into action of such muscles or muscular fibres as have not been destroyed, but are merely wasted and inactive. The exercises have to be varied according to the individual muscles affected, and the consequent nature of the deformity.

3. *Mechanical treatment.*—Except in very slight cases some form of mechanical apparatus is generally necessary to prevent a relapse of the deformity when this has been overcome by operative or mechanical means, or to hold the parts in a rectified position, into which they can be readily brought, but in which they cannot voluntarily be retained on account of the paralysis of the muscle. Some form of mechanical apparatus may sometimes be required to correct the deformity either with or without resort to operation.

The various kinds of apparatus that may with advantage be employed will be discussed at our next lecture, in which the details of treatment for each special deformity will be considered.

Administration of Milk.—A preferable time for administering milk, especially in the poorly-nourished females, is early in the morning, one hour before breakfast, the directions being that the patient shall lie still for a quarter of an hour thereafter; it is often found then that instead of rising fatigued, with no appetite for breakfast, the patient gains in strength and enters on the day with a vigour quite unknown before. Weak females should always lie down in a darkened room for a half-hour nap after taking the milk at twelve and five o'clock. The soporific qualities of warm milk at bed-time or in the night are sometimes remarkable.—L. Duncan Bulkley ('Journ. of Amer. Med. Assoc.' December 17th, 1898).

The Monthly Cyclopædia, February, 1899.

WITH Dr. DONALD HOOD IN THE WARDS OF THE WEST LONDON HOSPITAL.

November 14th, 1898.

GENTLEMEN,—You will remember I showed you this man on a previous tour round the wards. He was admitted as a severe case of sciatica. It was, as I thought, an affection of the sacro-iliac joint. I announced my intention of blistering over the joint; that has been carried out. The patient has been three weeks under treatment, and is now practically well. He has lost the pain, and can turn freely and comfortably in bed.

This boy was admitted some months ago, but I did not see him in the early stage of his illness. He had swelling of the abdomen and of the chest on the left side. There were symptoms of what was supposed to be effusion into the left chest. Dullness over the cardiac area was much increased. The question naturally arose whether the condition was due to pericarditis, or whether it was due to pleuritic effusion. He was aspirated on one or two occasions, but the needle failed to find fluid. In the tube of the needle when withdrawn were some large nucleated cells, and it was thought probably that the disease might be of a malignant nature affecting the glands of the chest. Subsequently his abdomen became very much distended. He is suffering now from extreme ascites, and he still has a very large area of dulness over the cardiac region. You will find it extends over the median line, and far beyond the normal cardiac dulness. He has a tense abdomen. In addition to these symptoms he has a very congested face, but not so much so as when he first came into the hospital, for then it was markedly livid and cyanosed. The superficial veins are still very prominent and turgid. The patient probably has some disease in connection with the mediastinum. It may be of a malignant nature. Still I hardly think it is likely to be that, because he has improved since he came into the hospital. Taking into consideration all the symptoms of the case I think we are justified in looking upon it as one where there is an adherent pericardium, with much thickening of the pericardial sac. There is also

an increase in the connective tissue of the mediastinum. Therefore we may regard it as an example of that rare form of disease in which there are indurative tissue changes affecting the mediastinum and pericardium, pressing upon the large vessels, thus bringing about the oedema and the ascites.

This boy has had an accident, a heavy roll of canvas falling upon him; soon afterwards he was admitted under my care with abdominal symptoms. We have to differentiate between peritiphlytis or inflammatory mischief about the right iliac fossa directly caused by the accident. The fact that the symptoms commenced so soon after the accident makes it probable that there is some causal relation. There is very extreme tenderness over the cæcal area, and you will notice he suffers considerably when touched in that region. He has vomited, and the temperature is 102. He has had nutrient enemata, but no opium.

This man, aged fifty-two, was sent to me about three weeks ago for an opinion. He is a cashier in a very large house of business in the city, where he has most responsible duties to perform. He has been in the service of this firm thirty years, during which time he has scarcely required medical advice. A few months ago the firm noticed that he evidently was not well. He complained of extreme pain over his chest, and in one or two of these attacks he had fallen down. From the character of the pain and the symptoms complained of, there is no doubt that these attacks were "angina." I have carefully examined him, but can find nothing wrong with the heart, except perhaps that the first sound is a little shorter than normal. In a man of his age one naturally thinks of commencing atheroma, or renal degeneration, but there were no such symptoms. I was struck by the flattening of one side of his face compared with the other. I asked if he had had any trouble with regard to swallowing or speech, and was told that he had had difficulty in speaking, that his tongue was large, and the food collected between his left cheek and his gum. He was unable to whistle, though six months previously he could do so without any difficulty. His articulation was imperfect; at times he could scarcely make himself understood. Saliva was apt to run from the mouth. All the symptoms put together seemed to denote a very early condition of what we look upon as a bulbar paralysis, or, as some

call the disease, glosso-labio laryngeal palsy. The case presents many points of difficulty. I ascertained that some months before seeing me he had ridden a bicycle to Brighton and back in one day; that, in a man of fifty-two, may be a great strain, and it may have damaged his heart.

Again, twenty years ago he appeared to have had specific mischief. What we have therefore to determine is whether this is a case of ordinary glosso-labio-laryngeal-palsy, or whether it is syphilitic mischief affecting that part of the cerebro-spinal axis, and is his heart condition dependent upon pneumogastric irritation? My own impression is that the whole series of clinical features presented by the case depends upon some syphilitic process in the bulb. I have given him iodide of potassium and perchloride of mercury in large doses, with the result that there is already a great improvement in his condition. He tells me that the symptom which troubles him most is a feeling of dulness or numbness in the finger-tips. Although he can pick up a pin well enough, he cannot feel it properly. His handwriting was not affected. These cases are not easily diagnosed when first seen, so I have taken him into the hospital for careful examination and watching.

This young woman was sent in by a medical man for diagnosis. It is a case of gastritis, with but little trouble in the stomach but pain. Last week I dwelt upon some of these cases. Here there has been partial perforation with subsequent adhesion.

One such case I had watched for over ten years. The patient, a woman, came to me about twelve years ago complaining of pain over her stomach. She told me that for some long time at irregular intervals she suffered from gastric pain. The patient herself was inclined to look upon the pain as neuralgic, an opinion which the symptoms seemed to warrant. I saw her again two years later, and then the pain was distinctly gastritis; but there was no vomiting, simply a gnawing pain over the pyloric end of the stomach. So severe was this pain that I thought she ought to be placed under treatment for gastric ulceration. In a month she was free from all pain. Three or four years later there was some slight return of the pain, but nothing very great. Two years ago she had a severe attack of influenza; this was followed by an attack, prolonged and pronounced, of the old

pain. This was in July, and I only saw her once or twice. She went into the country paying visits during August and September, and at each house she went to she was obliged to call in medical aid, and from these medical gentlemen she received a variety of opinions. Some of her attendants looked upon the discomfort as neuralgia, others as gastritis. I saw her at the end of September, when she was still suffering greatly, she still had no vomiting, and had never had an attack of hæmatemesis. She was kept in bed and on a milk diet. Early last December, while feeling fairly well, she had a sudden seizure of severe pain. I saw her within an hour of the attack, and my impression was that perforation had taken place. Improvement took place so soon that I questioned the accuracy of my diagnosis. However, within two or three days there was evidence of disease in the right pleura. A needle failed to find fluid. The patient now had practically no stomach symptoms, but a hectic temperature, wasting, and much pain, and symptoms of a sub-phrenic abscess. Exhaustion set in and she died. Post-mortem we found the pyloric end of the stomach and the duodenum enormously thickened, the duodenum being one inch thick in some parts. The chest complication was not pleurisy, but a septic pneumonia. There can be little doubt that here was the explanation of these years of recurring gastric pain. The lessons I have learnt from this and similar cases are responsible for my giving so much attention to this case before us of apparently slight stomach pain occurring after several months of definite gastritis.

This girl's age is seventeen. She was in the hospital some months ago for enteric fever, and was discharged cured. A fortnight later she returned complaining of swollen legs. After a long illness this may be but a slight matter, and entirely due to the debilitating influence of the disease. But when such a condition follows enteric fever I think we should make a careful examination as to the cause. One of the commonest sequelæ of enteric fever is venous trouble. I have purposely avoided using the word phlebitis, because I believe that in many cases of so-called post-enteric phlebitis there is no marked inflammatory condition present, and there may be a simple temporary blocking of the vein; a passive rather than an active condition. So here

we have to decide whether this girl's trouble is attributable to venous trouble, giving rise to œdema in her legs, or whether it is due to the debility caused by her long illness. Her legs are not much swollen, and I could detect no trouble in her veins. I am therefore inclined to look upon it as a relaxed condition of the tissues caused by the illness through which she has passed.

A case in connection with such œdema may be sufficiently interesting to warrant my mentioning it. A man, forty years of age, was sent up to me some eighteen years ago, with a note from his doctor saying that he was suffering from Bright's disease and dropsy, and that his condition was very unsatisfactory. The patient was very much troubled about himself. I asked him some questions to supplement the doctor's statement of the case, and on examining his urine with heat I was struck by the amount of curdy deposit. I made only a cursory examination, and concluded it was albumin. I examined one leg, and found it very tense and swollen, and it seemed characteristic of the dropsy which ensues in Bright's disease. Then I asked to look at the other leg, and was astonished to hear the man say that it was not swollen. Now, for one leg to be swollen with œdema and the other in its normal condition, is quite opposed to Bright's disease. Then I did what I ought to have done earlier, namely, made a more careful examination of the urine. Then I found that the curdy masses which simulated albumin were phosphates. The patient some months before had passed through an attack of typhoid fever. As a result of the fever he suffered from blocking of the femoral vein, hence the extreme œdema of the one leg. The urine, on boiling, became thick with firm white deposit. This with a much swollen leg, a pasty, anæmic complexion, suggested renal disease, and he was sent up to me as a bad case of Bright's disease. The patient has remained well from that day to this.

The woman in the next bed presents a very good example of an enlarged spleen. It appears she was admitted last June under another physician, with enlargement of her spleen and pain, but she had no other symptoms. Her blood showed no excess of white corpuscles. Under treatment with arsenic and rest her spleen resumed its normal size. A few days ago she was re-admitted with a spleen larger than when she first came in, and she

has now an additional symptom, enlargement of the lymphatic glands in other parts of the body, namely, in the groin, axilla, and neck. It therefore seems to me that the case is one of those described as Hodgkin's disease, in which you may get a true anæmia, but with no excess of white corpuscles. There is also a gradually increasing exhaustion. These cases are difficult to diagnose at first. When there is a more or less general enlargement of the lymphatic glands it is impossible to know whether it is a commencement of Hodgkin's disease, or whether it is a simple inflammatory enlargement. Such cases are linked together by very varying factors; progress may be rapid or slow. Anæmia may be markedly present or occurs late in the course of the disease, the size of spleen will be found to vary much; generally the enlargement is not extreme, but there are many instances of an enormously enlarged organ.

Spinal-Cord Disease presenting Unusual Symptoms.—Dr. LESZYNSKY reports the case of a man of thirty-five years of age, who had been in perfect health up to June, 1896, at which time he had fallen from a considerable height, striking on his buttocks. Immediately afterward he had been paraplegic, and had had no control over the bladder and rectum. There was now incomplete paraplegia, with slight spasticity affecting the adductor muscles on both thighs. There was a loss of the pain sense and the temperature sense, with preservation of tactile sensibility. At one time this had been supposed to be characteristic of syringomyelia, but it was now known to occur in connection with several other disorders. The most unusual feature in this case was the absence of the knee-jerk and the presence of ankle clonus. The speaker said there had been only twelve cases reported in which this condition had been observed. If the ankle clonus were looked upon simply as an exaggeration of the Achilles reflex, the matter would be simplified a good deal. This patient also presented an extremely rare condition, *i. e.* a gluteus medius clonus. The man had evidently had a hæmorrhage into the spinal cord in the lumbar region, which had resulted in the cutting off the reflex arc of the knee-jerk. Below this point the hæmorrhage had done no harm, and the descending degeneration had taken place in the lateral columns of the cord.—*Medical Record*, April 1st, 1899.

MEETING OF THE SOCIETY OF ANÆSTHETISTS,

At 20 Hanover Square;

The President, Dr. DUDLEY BUXTON,
in the Chair.

DR. FLUX read the following paper:

Administration of Nitrous Oxide Gas by an Open Inhaler.

MR. PRESIDENT, LADIES AND GENTLEMEN,—I propose this evening to put before you a method which I have devised for administering N_2O gas by means of an open inhaler.

The objects of the method are, in addition to the induction of anæsthesia:

1. To save the patient from the discomforts of the accurately fitting air excluding face-piece.
2. To enable the patient to breathe the gas at the atmospheric pressure.
3. To maintain free access of air to the patient.
4. To admit the gas to the inspired air, only in such amount as circumstances may require.

The whole of these objects are obtained by using the open inhaler in the way I will describe.

It is, perhaps, hardly necessary to point out that the admission of gas to air in an open inhaler is *not* the same thing as admitting air to gas in a closed one—the act of admission implies previous limitation or exclusion.

In using the open inhaler, it is the gas which is admitted to the air, and not air which is admitted to the gas—the air is permanently present, the gas is merely a temporary addition.

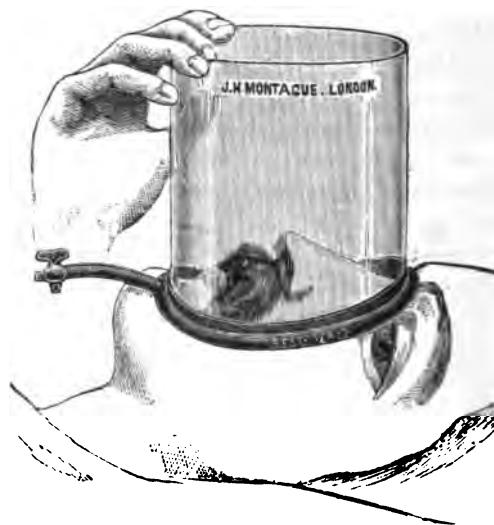
The open inhaler and the method of using it are both very simple.

The inhaler widely open at the top (and which I will refer to again later), is adapted to the face of the patient.

Into the wide mouth of the inhaler a gentle but sufficient stream of gas is allowed to flow—to induce anæsthesia it is only necessary to admit gas during the act of inspiration. When anæsthesia has been once induced it can be kept up indefinitely by allowing the patient an occasional breath of gas—the inhaler, being removed or not in the intervals between the breaths as may be convenient.

Should it be desired to use the gas as a prelude to ether, all that need be done is to put the patient well under the gas, and then proceed to ether in the usual way. It will be found that the patient will pass quietly into deep ether anæsthesia without any appreciable change of colour or of respiration.

In cases of protracted operations *under gas* it is seldom necessary to keep the patient in the most



profound state of æsthesia, for as the anæsthetist has the gas under complete control, he can raise and lower the degree of anæsthesia as often and as quickly as circumstances may require.

Apart from the open inhaler no special apparatus is required, all that is necessary being to deliver the gas easily into the inhaler. An ordinary apparatus, without the face-piece, answers all purposes;

the two or three way stopcock makes a very useful *tap*, and the gas supply can be regulated by the foot.

The effects produced upon the patient when gas is given with the open inhaler are generally as follows:—After the first few breaths the agitation caused by apprehension subsides; the patient assumes a sleepy appearance, and, in from five to ten seconds, is unconscious; the breathing becomes steady and tranquil, and, by the end of from fifty to eighty seconds, the patient is completely anæsthetic. The only visible alteration being a change from a condition of wakefulness to one of sleep, which is so tranquil that onlookers who have not previously seen this method employed, can hardly believe that the patient is ready for operation.

No excitement, stertor, lividity or convulsive movement, or any sign of asphyxia, occurs throughout the administration, and resulting anæsthesia; in fact, the whole proceeding is characterised by extreme placidity. In no case has it been necessary to use force to restrain the patient either during or after the exhibition of the anæsthetic. Even when administering with unsuitable or improvised inhalers, or under adverse or previously untried conditions, the results have been almost uniformly satisfactory.

I have asked several physicians to watch cases for me; and although the administrations have lasted as long as twelve minutes, and the patients have varied in age from seven *months* to seventy years, they have observed no disturbance of the condition of the patients, beyond a *slight* increase of pulse rate during the first few moments of inhalation—the pulse soon settling down to a quiet unchanging rhythm. It was impossible to determine, however, whether the initial slight increase in pulse-rate was due to agitation or to the anæsthetic.

The return to consciousness when the patient is not disturbed is placid and tranquil; he quietly opens his eyes, looks about him, and realises his position. If the operation is continued beyond the duration of true anæsthesia signs of apparent discomfort will appear some seconds before clear consciousness is established.

In spite of many experiments as to size and shape of inhalers, and of variations in the manner of regulating the gas supply, inseparable from

attempts to decide upon the most practical procedure, complaints of after-effects have been few and far between, and even when present have been but of a trifling and very transient nature. In cases where the operations have lasted several minutes, and the patients have been under constant observation both before and after, no subsequent effects of any sort have been observed.

At first I simply used as an open inhaler some textile fabric; for example, a napkin or towel folded and adapted to the face in such a manner that the nose and mouth were partially enclosed in a more or less cup-shaped cavity, completely open at the top. As an extemporised face-piece is not always convenient, and is often wasteful, I have had some more permanent inhalers constructed which give very satisfactory results. These inhalers are made in two forms, one cylindrical for use when the patient is lying down; the other, somewhat more cup-shaped, for use when the patient is sitting up. The material used in their construction is clear thin celluloid, a substance which is at once light, transparent, and very pliable; it is non-absorbent, and can be easily and rapidly cleansed. It is very desirable that the inhaler selected for use should be selected with due regard to the size of the patient. In using the open inhaler, it is important that when it is applied to the face the adaptation should be very accurate; if it is not so, there will be rapid escape of gas.

When a suitable open inhaler is accurately applied to the face, there cannot be any escape of gas unless due to overflow resulting from too free a supply. The weight of the gas prevents escape upwards, and unless leakage is permitted between the margins of the inhaler and the face, there can be no escape downwards. The gas, when given in the open inhaler, being freely exposed to the air, can only be inhaled at the atmospheric pressure, and the pressure can neither be increased by an excess of gas nor lowered by a reduced supply. The gas as it falls into the inhaler is warmed by the breath of the patient, and by the surrounding atmosphere—an important point when the cold produced by the rapid evaporation of the gas on leaving the cylinders is remembered.

The open inhalers are, as will be seen, unconnected with the rest of the gas apparatus, and are not, as is the case with closed inhalers, in

direct communication with bags, valves, and tubes, all of which must of necessity be more difficult to keep clean and aseptic than an isolated inhaler.

To render an ordinary adult anæsthetic by this method, an average expenditure of from seven to eight gallons of gas is required, a further amount of three or four gallons for each additional minute will suffice to maintain anæsthesia.

The chief advantage gained by the use of the open inhaler is increase of comfort to the patient, who, therefore, becomes less reluctant to submit to operation. The whole of the discomforts which are associated with the closed face-piece vanish. The whole of the proceedings are quiet and easy, and the patient's apprehension is reduced to a minimum. He is not disturbed by the rush of gas or the sound of valves. He cannot be forced to breathe under artificial pressures. His nose and mouth are free to the air, and he does not have to breathe from a closed and stuffy cavity into which all sorts and conditions have breathed before him.

He does not experience that sense of helpless confinement and of impending suffocation which is so frequently complained of, and which creates so widespread a prejudice against taking an anæsthetic.

There are some other advantages. The patient's face is freely exposed to observation throughout the administration.

The amount of gas admitted to the inspired air can be increased or diminished at discretion.

When using the open inhaler, as the full anæsthetic effect of the gas is comfortably and quietly obtained in the presence of the free air, special arrangements for artificial aëration are not required.

Whatever can be done with gas can be done with an open inhaler.

Mr. BELLAMY GARDNER said, that while fully appreciating the advantage of the simplicity of the forms of face-piece which Dr. Flux had sent round, and the simplicity of the whole method, he was convinced that there must be some fallacy in the fact stated by Dr. Flux, namely, that a large quantity of air, and a very small quantity of gas was necessary for that form of anæsthesia. He helped Dr. Hewitt with a large series of experiments with a mixture of known percentages of air and nitrous oxide gas, and when they got up to certain percentages, over 30 per cent.—the air being mixed most accurately and

carefully in a gasometer—they found that true anæsthesia was almost unobtainable. In breathing into both the forms of face-piece which had been exhibited he found that the air taken in was, after the first breath, extremely warm, and he had no doubt that for some reason—either due to the amount of aqueous vapour present in the expirations or some other physical reason—the expirations did not escape to the degree which at first sight one would expect. He thought, therefore, that it was very probable that both these face-pieces were open to any amount of atmospheric mixture, that expirations containing CO₂ were mixed with the gas; and though probably Dr. Flux and others who had tried it could very nicely adjust the gas and air mixtures, it seemed that less than twenty per cent. of air was mixed with the gas.

Mr. BALDWIN said he was not particularly qualified to speak on the subject; but, as the method was a new one, and probably very few members had seen it used, he thought a few words would not be out of place, because he had seen Dr. Flux use it. The length of time taken to produce anæsthesia was certainly not more than usual, while the duration of the anæsthesia was certainly not less than was ordinarily the case. The condition of the patient had been most natural and extremely good, entirely free from jactitation and other unpleasant symptoms. He was of opinion that the anæsthesia produced was longer than usual. He had not had the pleasure of Dr. Flux administering the anæsthesia by this method for any of his own operations; he had simply seen him employ it for others. He had, however, already derived some benefit from the method, inasmuch as, when some of his own patients had expressed an unwillingness to take gas on account of the unpleasantness of the closed face-piece and the feeling of suffocation they underwent, he had been able to assure them that there was, at any rate, a method by which they could take gas perfectly well, which dispensed with the closed face-piece and the obvious sense of impending suffocation produced.

Mr. CARTER BRAINE said he also had had the pleasure of seeing Dr. Flux administer the anæsthetic by the method he explained at the Dental Hospital of London, and he was very much surprised at the state of the patients. Although he watched them very carefully, he was unable to tell

when they were anæsthetised. They simply seemed to remain in a sort of normal sleep, and he would not have known when to stop the administration. At the end of the administration he obtained Dr. Flux's permission to be himself anæsthetised by the method. He had had gas many times, but never so comfortably as then. The face-piece was put into position, and the gas fell in from the top. He breathed perfectly naturally, and did not seem to be taking gas. He thought the whole thing was a farce. What stage of anæsthesia was ultimately obtained he could not say, but Dr. Flux would, perhaps, be able to speak on that point. The next thing he was aware of was that he was simply in the chair, staring straight in front of him; then somebody told him to lift his arm, and he did so. At the time he was convinced he had not been under the anæsthetic at all. But very shortly afterwards he found he had been under it, because his knees seemed very shaky, and there was no doubt he had had a good dose of nitrous oxide, for he was afterwards told that the anæsthesia had lasted two and a half minutes. He could, therefore, bear out the statement that it was a most pleasant way of taking nitrous oxide. As to the degree of anæsthesia obtained, of course he could not give an opinion.

Mr. TYRRELL asked whether Dr. Flux could obtain sufficient anæsthesia to produce loss of the conjunctival reflex by his method; also whether there was any difference between the behaviour of men and women with the anæsthetic. He thought there was sometimes a difficulty in keeping up nitrous oxide anæsthesia in men, and he could imagine it would be rather more difficult by this method.

Mr. WILLETT asked whether Dr. Flux had tried his method of anæsthesia for other operations, such as those in general surgery, and, if so, why he should stop the gas and go on with ether as described in his paper? It would be interesting to hear how long he had been able to keep a patient under by his method.

Mr. Low said it would be interesting to know what was the test of the anæsthesia here? It appeared that there was no stertor.

Dr. FLUX was also asked whether anæsthesia had ever failed by that method, and how many cases he had given it in.

Dr. FLUX, in reply, said Mr. Gardner had quoted

some sums and calculations of Dr. Hewitt, in reference to oxygen and gas. As far as Dr. Hewitt's researches were concerned, he thought they were extremely excellent, but they all applied to the closed inhaler. The actual chemistry of respiration when applied to the open inhaler he, Dr. Flux, had not tried, indeed, he had only used the method for so short a period that it had been impossible yet to arrive at statistics or averages. All he could say was that what he had stated so far was fact. He found that by using the open inhaler one had a very great control over the gas and over the condition of the patient. Gas was added as required, the air with its 20 per cent. of oxygen being merely diminished by the excessive flow of gas or otherwise. Of course if they blew in such a blast of gas that they drove out all the air, they would naturally drive out all the oxygen; but in inhaling under ordinary atmospheric pressure he took it that, as the solubility of gas in a fluid at the temperature of the body was only a certain amount, if that certain amount were given at the atmospheric pressure and they did not lose it, he failed to see how the patients were to avoid taking it. Whether or not they were given a great deal over and above what they could absorb, he did not think was material. But the fact remained that with an open inhaler and by using the gas at discretion they could get a result which he had never seen obtained with gas, and a very protracted result could be obtained if the procedure were continued long enough. As to the question of conjunctival reflex, that reflex would very often disappear, it could always be made to go if the anæsthesia continued long enough. But the conjunctival and lid reflexes were not very sure signs to go by, they were not quite dependable. If they were absent, of course, the patient was anæsthetic, but even if they had not absolutely disappeared anæsthesia deep enough for all practical purposes might be present.

The onset of anæsthesia was known by the absence of sense of feeling. When the patient was soundly asleep, they might conclude he would not feel anything. He had given it in 350 cases altogether in that way, and except in cases where the gas supply had failed, or had not been under sufficient control, or when he had used leaky masks or anything of that kind, the cases had run extremely truly, and he had had very little trouble

with them. Still, no one was infallible, especially in carrying out experimental matters.

As to men *v.* women, his greatest experience had been with women, and mostly in dental work, and in dental work at hospitals he found there were about twenty women to one man. Still, when men came he found they ran as true with the anæsthetic as did women. Regarding stopping short and continuing with ether for protracted operations, it was simply a question of supply. If there were a sufficient supply of gas he saw no reason why, with an open inhaler, they should not go on for a very long time. A very short time ago, as he mentioned in an article which he contributed to a journal, he put a common house mouse into the bottom of a pot which was open at the top, and into which a little gas had been put. That mouse remained five and a half hours without moving a limb, and as soon as he took it out, it prepared to run away. He pinched the mouse in the interval frequently, but it, apparently, did not feel, because it did not move.

Orexin in the Vomiting of Pregnancy.—F. Hermann, in the 'Therapist' of February, 15th, 1899, reports his results with the use of this remedy in nine cases. It was given in 4½-grain doses in wafers. In not one did the remedy fail of the desired effect, and relief was afforded in most cases after a few doses. The effect is said to be lasting, even after the drug is discontinued. In a number of cases in which the usual remedies had been tried and had failed, or gave but temporary relief, vomiting was promptly stopped by orexin.—*Medicine*, April, 1899.

Methylene Blue in the Treatment of Rheumatism.—Le Moine reported to the French Academy of Medicine ('Le Progrès Médical,' February 4th, 1899) that he had employed methylene blue in the treatment of rheumatism. It did not diminish pain, but it did lessen fever, and he thought it possessed a selective action upon the micro-organisms of the disease. In the treatment of rheumatism its value is equal to the salicylate of soda. It is necessary to employ a pure product, as the drug is often adulterated with the salts of zinc, which cause cystitis and other unpleasant effects.—*Medicine*, April, 1899.

DR. NICHOLLS sums up, in the 'Montreal Medical Journal,' for March, an article on "Bright's Disease and the Bacillus Coli":—

1. The different forms of Bright's disease are to be regarded as various stages in the same general process, there being a unity prevailing the whole pathological picture.

2. All forms of nephritis are due in the immense majority of cases to infective agents; the acute, to the usual specific germs of the primary disease, and the chronic, as a general rule, to the bacillus coli, though other germs may, sometimes, be concerned.

3. Acute interstitial inflammation and subsequent connective tissue hyperplasia are the keynote of the process; this is, however, preceded by parenchymatous degeneration.

4. The point of invasion by the bacillus coli is the gastro-intestinal tract; for other germs it may be various.

5. The liver and mesenteric glands are the first barriers of defence; and the endothelial cells of the capillaries and secreting tubules of the kidney have the power of ingesting bacteria, this being an attempt at inhibition and elimination.

THE VITÆ ORE COMPANY, of 39, Temple Chambers, E.C., have submitted for examination a mineral ore preparation, which forms a very useful therapeutical agent, to be taken by the patient as a chalybeate mineral water, made by dissolving the dry, ash-coloured, and almost impalpable powder supplied by the company. One factor, which pre-eminently accounts for the good results obtained by its use when clinically tested, is that this ore is largely composed of compounds of iron, easy of solution, and capable of being assimilated with great facility, and absence of the usual constipating effect. This dry powder, the aqueous solution of which enables the well-known beneficial results of the therapeutic use of iron to be arrived at without digestive derangement, is certainly a remarkable and noteworthy advance in the means we have at our command for ensuring return to health in those suffering from the numerous disorders attended with anæmia. The composition of this material, formed from a natural product, and the process of preparation, are published, ensuring that the substance is subjected to no artificial treatment.

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